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P. A. M. Dirac.

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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.

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".'

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 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 $A.B$ did not equal $B.A$. 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 Manci 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 $\hbar c/2$. An interesting implication of this result is not only that the pole strength of any magnetic pole would have to be a multiple of $\hbar c/2e$, where e is the electron charge, but that if there exists a pole of strength $n\hbar c/2e$, 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, 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 viscocity. (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.

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.

He concerned himself only with the electron and the electromagnetic field, although he was well aware of the existence of other particles and fields. He felt that quantum electrodynamics was the easiest case, and until this was resolved satisfactorily it made no sense to look at the new particles.

He made this point as early as 1941 to Eliezer, who thought then of doing some work about mesons. 'Our theories for all particles have some serious difficulties when we consider how they interact with each other. It is better to try to solve the difficulty for the simplest of all particles—the electron—before dealing with more complicated ones.' He retained this view consistently.

In addition to his many attempts to reform quantum electrodynamics he also continued working on the magnetic monopole. He later lost interest in the idea. When he was invited to attend the 1981 Trieste meeting on monopoles in quantum field theory, he replied (Craigie *et al.* 1982): 'I am now inclined to believe that monopoles do not exist. So many years have passed without any encouragement from the experimental side. It will be interesting to see if your conference can produce any new angle of attack on the problem.'

Nevertheless, his work on this topic had an important impact on theoretical physics, because in many recent attempts to formulate new theories the concept of magnetic monopoles is essential. He also continued to work on the large-numbers hypothesis. Other work concerned general relativity, usually with the aim of quantizing it.

The Diracs spent the postwar period, until Paul's retirement in 1969, in Cambridge, with interruptions for short visits abroad and for longer sabbatical periods. The first of these was in 1946, when he spent the spring semester as a member of the Princeton Institute for Advanced Study, and the autumn at Princeton University. He returned to the Institute on sabbatical leave for the academic year 1947–48. He took sabbatical leave again for the year 1954–55, intending to go again to the Princeton Institute, on the invitation of the Director, Robert Oppenheimer. However, he was refused a visa by the U.S. Department of State. Dirac believed that the reason was probably his many prewar visits to the Soviet Union, but noted that he had already been given a visa on three occasions since the war.

Instead he arranged to visit the Tata Institute of Fundamental Research in Bombay for the first half of the year. The family then travelled by sea to Japan and then to Canada, where he took up a visiting appointment for the remainder of the year with the National Research Council at Ottawa. During this voyage Dirac was very ill with hepatitis, contracted in India, and on arrival at Vancouver had to have close medical attention, the family occupying one floor of a private mansion on the University of British Columbia campus. Dirac was so poorly that it was considered essential for him to receive medical attention in the U.S.A. He quickly received a visa, no doubt on humanitarian grounds, and moved to Princeton, where he recuperated for many months in a house of the Institute of Advanced Studies, until he was considered well enough to take up his appointment at Ottawa for the rest of that summer.

In the autumn of 1955 he was a Visiting Professor at Moscow University, and lectured there and at other institutions. When he was asked at the University to state briefly his philosophy of physics, he wrote on the board:

'PHYSICAL LAWS SHOULD HAVE MATHEMATICAL BEAUTY'

and this has been preserved there to this day.

The discovery, in 1957, of the non-conservation of parity surprised most physicists, but to Dirac the possibility had occurred earlier as quite reasonable. In his 1949 paper on forms of relativistic dynamics (79) he says 'I do not believe that there is any need for physical laws to be invariant under [such] reflections, although all the exact laws of physics so far known have this invariance'. He goes on to explain that relativity only requires that the laws of physics be unchanged by any change in the position or velocity of the observer. All such changes can be generated by infinitesimal transformation, but this does not apply to reflections. When G. Herzberg reminded him of these remarks in his introduction to a lecture by Dirac in Ottawa in 1959, Dirac said he had forgotten that he had expressed these thoughts in print.

At K. J. Le Couteur's Ph.D. examination in 1948 Dirac asked why he thought relativistic wave equations (the thesis subject) should be invariant under space reflections. When Le Couteur could not answer properly Dirac looked at him sadly and said 'Well, it is not so for living matter'. When parity violation was discovered Dirac commented, it is said, 'Well, there's nothing about parity in my book'. In retrospect it appears that one should have paid more attention to his remarks.

In Cambridge Dirac tended to work at home. He had a room in the Arts School, which was used mainly for talking with students and others, but when the Department of Applied Mathematics and Theoretical Physics (DAMTP) was formed in 1959, the space in the Arts School was no longer available, and he did not accept the offer of a room in the DAMTP building. He had given up his college room when he moved to Cavendish Avenue in 1937, following a plea from Cockcroft, then College Bursar, as the College was short of rooms.

There were many visitors. Manci's outgoing personality brought many friends, and Paul helped to make them welcome.

Over the years many people had asked Dirac why he stayed in Cambridge for the full term of his Lucasian professorship in the face of many attractive offers of prestigious appointments in America. His reply was always that he felt it incumbent upon himself to give a proper lead to the younger theoreticians in Britain, and not to leave them, a feeling that may have stemmed from a letter written to him by Martin Charlesworth of St John's College while he was on leave in America during 1931, who wrote 'Cambridge is much as usual, though I miss your kindly irony. *Do not let them persuade you to stay in USA*. Here, here is your home.'

On Paul's retirement in 1969 he felt free to depart, and the family moved to Florida. After visiting appointments at the Center for Theoretical Studies of the University of Miami, Coral Gables, and at Florida State University, Tallahassee, he accepted, in 1972, the offer of a research professorship in the latter. There his working habits changed, though not of course the nature of his work. Adapting himself to local customs he left home every morning, carrying his lunch, to return in the late afternoon. He still worked mostly on his own; his closest contact in the department was his research associate, Leopold Halpern.

He retained his contact with the Center for Theoretical Studies. He spoke regularly at the Coral Gables Conference (later named Orbis Scientiae) usually giving the first scientific lecture. He regularly conferred the annual Oppenheimer Prizes at the Center, the first of which had been awarded to him in 1969.

In 1972, at a conference in Trieste, there was a banquet to celebrate his 70th birthday (Mehra 1973). At the same time a volume of essays about Dirac's work was published (Salam & Wigner 1972).

A volume planned to honour his 80th birthday was not ready when he died, and will be published as a memorial volume (Kursunoglu & Wigner, 1986).

In Florida he kept up his custom of going for long country walks, but gradually his failing health prevented this. He had to undergo a very serious operation in 1982, but he put up with the discomforts patiently. When his physician suggested that a patient who had to stay indoors should have a hobby, Dirac said that his hobby was 'thinking'.

His last talk at an Orbis Scientiae meeting was in January 1983 (194). The topic was his opposition to the currently accepted renormalization theory. Although his health was evidently failing, he spoke clearly and firmly, if softly. His lecture will be published in his memorial volume.

He died on 20 October 1984 and was buried in the cemetery at Tallahassee.

Personal characteristics

Dirac was a legend in his lifetime. Numerous anecdotes were circulating about him, some apocryphal, others misunderstood in their significance. Yet some of these anecdotes help us to understand the way his mind worked.

People regarded him as a very silent person, and indeed he had no idle conversation, but he was very articulate when he had something to say.

What he did say was often surprising because he always took the subject under discussion seriously. J. Mehra tells the story of a visit to Cambridge when he was anxious to meet Dirac, and was brought by a friend to dinner in St John's College and placed next to Dirac. After some silence Mehra thought a remark about the weather might be in order, so he said 'It is very windy today'. To his surprise Dirac got up, and walked to the door, making Mehra think he had said something offensive. But Dirac opened the door to look out, returned to his seat and said 'Yes'.

In physics he disliked discussing unsolved problems or hypothetical questions. It was said that when you asked him a question about physics, his answer would be 'yes', 'no' or 'I don't know'. S. Chandrasekhar recalls that in listening to him expounding his views Dirac repeatedly interjected 'yes', and then explained: 'When I say yes, it does not mean that I agree; it means only that you should go on.'

He did not have the common, and often trivial, associations. At a social gathering people commented on the fact that in the past few years all the children born to Cambridge physicists were girls, and someone said 'It must be something in the air'. After a considerable pause Dirac added 'Or in the water'.

Once arriving for a dinner party when his hosts were late, he was waiting in the company of the grandmother, who was knitting. He observed the process in silence, and then said 'There must be two ways of doing this.' As every knitter knows there are two topologically different styles of knitting.

He applied the same concentrated and unconventional attitude of thought to problems in real life and in fiction. S. F. B. Tyabji, a retired lawyer from India who studied under him, discovered that Dirac was very anxious to meet E. M. Forster, the novelist, who also lived in Cambridge. Tyabji, who knew Forster, invited both to dinner. When they met, Dirac asked (evidently in relation to Forster's *A passage to India*): 'Could there not have been a third person in the cave?' It seems that this possibility, which would have meant that neither of the characters had told a lie, had not occurred to anyone. But Forster replied 'No. Absolutely not. There was no-one else in the cave.' This reply satisfied Dirac's curiosity, and he did not speak again during the evening.

On one occasion in the late 1950s there was a question of inviting some young people to a party, and Dirac was asked if he had any students. 'I had one, but he died'. This has sometimes been understood to mean that he never had another student at any time, but he evidently meant that at that particular time he would have had one student if he had not died; however, we have not been able to identify this student.

Another anecdote of earlier days is told by Kronig: During some meetings in Copenhagen people were saying that Pauli was putting on too much weight, and Dirac was told to watch that Pauli did not eat too much. Pauli, entering into the spirit of the plan, asked Dirac at coffee how much sugar he was allowed in his cup. Dirac said 'I think one piece is enough for you', adding after some thought: 'I think one is enough for anybody,' and finally 'I think the lumps are made in such a way that one is enough for anybody.' This was not, of course, meant very seriously, but it does suggest a belief in the orderliness of the world.

This fits in well with his search for simple and mathematically beautiful laws of physics. Niels Bohr once said: 'Physics is the hope that a simple and rational description of nature is possible'. For Dirac this hope amounted to near certainty. His taste for tidiness came out when Niels Bohr dictated the draft of a paper to Dirac, and at one point stopped and said 'Now I don't know how to finish this sentence'. Dirac remarked 'I was taught at school never to start a sentence without knowing the end of it'. His papers certainly give the impression of having been written in conformity with this rule.

As a young man he was very fond of his car, but was hardly an accomplished driver. The story went around Cambridge that the speed of his car had only two eigenvalues: zero and full speed. (This was an allusion to the electron, whose velocity components, according to the Dirac equation, have only two eigenvalues, plus and minus light velocity.) When he had a small car accident, and a friend, who was with him, called the police, arranged for an ambulance, etc., Dirac asked 'Have you been in an accident before?' When the answer was no, he asked: 'Then how did you know what to do?'

He tackled domestic problems also by rational reasoning. In Cavendish Avenue he was a passionate gardener. His approach to gardening showed great energy, and much logic, but not always great success. He would insist on every apple being picked even if this meant a perilous climb to the top of the tree.

We have already mentioned his fondness for long walks. In Princeton these took him into the woods below the Institute, which were not well kept, with the paths getting overgrown. So Dirac started to work there, and proudly showed visitors the areas he had cleared. A. Pais reports that on one occasion he helped Dirac in this work. The physical work made Pais too hot, and he took off his shirt and singlet and hung them on a nearby branch. Dirac noticed this and, in a gesture of sympathy, took off his tie and hung it near them.

He was fond of his daughters. He did not find it easy to demonstrate his affection, but he often took them along on his walks, and evidently enjoyed their company.

As a young man he was an agnostic. The Churchill College Archives contain several letters to him from Mrs Isobel Whitehead, the mother of the Oxford mathematician with whom Dirac was friendly, trying to convert him. His replies have not been found, but as the attempts at conversion continued, they evidently had failed. Heisenberg (1971) talks about a conversation, in which Dirac expressed his views, when Pauli remarked 'I see. There is no God and Dirac is His prophet'.

It is believed that in later life his views mellowed. The story goes that in reply to questions from a student why the universe obeyed certain rules, Dirac replied impatiently 'Because God made it that way.' This can hardly be taken as firm evidence of a belief in God, any more than Einstein's famous remark that God does not play dice, but it does show a softening of his position. According to Mrs Dirac he had deep religious feelings, though critical of some activities of churches. He accepted membership of the Pontifical Academy, and wrote for them a number of scientific and biographical notes (126, 135, 155–157, 160, 169, 180).

Scientific work

We shall discuss Dirac's papers by subject, rather than chronologically.

1. *Fundamental papers on quantum mechanics*

Dirac's best-known and most important work consists of his contributions to the foundations of quantum mechanics. We have already mentioned the genesis of his first quantum mechanics paper (8) and his discovery of the relation between commutators and Poisson brackets. The background to this paper was the great step forward made by Heisenberg in moving towards a consistent basis for the quantum theory, which until then had grafted the Bohr–Sommerfeld rules on much of the old Newtonian dynamics by *ad hoc* prescriptions.

Heisenberg's starting point was to associate the components of radiation with two atomic states, so that they became matrix elements. He then developed rules for the multiplication of these quantities (which were, in fact, the rules of matrix calculus, though he did not know this at the time). Heisenberg was surprised to find that by these rules the product of two factors depended on their order: $A.B$ was not identical with $B.A$.

Dirac was also at first taken aback by this result, but then realized it was an essential part of the theory. He started to study the properties of non-commuting quantities, for which in his next paper (9) he introduced the name 'q numbers'.

To specify the algebra of non-commuting, or q, numbers one has to specify the 'commutator' ($A.B-B.A$). Heisenberg had obtained some results about commutators in a rather *ad hoc* way. Dirac saw that they were identical with the Poisson brackets of classical mechanics, except for a factor $i\hbar$. He regarded this as a satisfactory postulate but he decided not to use this in his paper; instead he derived the commutators by considering the general process of differentiation of one q number with respect to another. His reasoning on this point is rather hard to follow, and Heisenberg, in his correspondence with Dirac, showed that he found considerable difficulty with it. Reference 8 then derives the relation with the Poisson brackets as a consequence of his postulate. The paper established Dirac's own personal style. He also discussed the consistency of the new theory. In this respect he was more interested in logical completeness than the Göttingen group.

In the next paper (9) he tackled the problem of the hydrogen atom. This paper was submitted in January 1926, barely five months after he first saw Heisenberg's paper. The hydrogen atom had been the first success of the Bohr theory, and was evidently a crucial test for the new mechanics, but its solution in terms of the Heisenberg matrix method is far from easy. Dirac derived for the energy levels the result $R/(B+n)^2$ ($R =$ Rydberg's constant, n an integer, and B an unknown number). For a complete answer it would have to be shown that with the choice $B = 0$ there would be no matrix elements leading to states with negative n, but he did not succeed in proving this. He did not pursue the work after hearing that Pauli had obtained a full solution.

Dirac went on to test the theory by working out the matrix elements giving the frequencies and intensities of the multiplet components in the anomalous Zeeman effect (10).

By the time he wrote his paper 'On the theory of quantum mechanics' (14), submitted in August 1926, Schrödinger's papers on wave mechanics had begun to appear, including the proof of the equivalence with Heisenberg's matrix mechanics. At first Dirac did not like Schrödinger's approach. He says (162) 'At first I felt a bit hostile towards it...we had already a perfectly good quantum mechanics...why should one go back to the pre-Heisenberg era...?' However, after Heisenberg wrote to him explaining how wave mechanics could supplement quantum mechanics, Dirac took up the new technique enthusiastically.

In reference 14 he discusses the symmetry properties of the wave function in the case of several identical particles, and arrives at the distinction between what are now called Fermi–Dirac and Bose–Einstein statistics. He points out (162) that the idea was really due to Fermi, who published it in an earlier paper, which Dirac had read but forgotten. But his paper derived the possibility of either kind of statistics from wave mechanics. He comments: 'The solution with symmetric eigenfunctions must be the correct one when applied to light quanta.... The solution with antisymmetric eigenfunctions is probably the correct one for gas molecules, since it is known to be the correct one for electrons in an atom, and one would expect molecules to resemble electrons more than light quanta.' Even as logical a mind as Dirac's can occasionally jump to misleading conclusions!

In reference 16 he developed what is now called transformation theory. This shows that wave mechanics and matrix mechanics are really special cases of a quite general approach. The description can be based on any complete set of commuting variables, with an amplitude whose square gives the probability of an observation finding the particular values of these variables. Wave mechanics chooses the particle coordinates; matrix mechanics chooses the energy and any other constants of the motion. The concepts and the notation developed here by Dirac form part of the basic tools of every physicist today.

This paper also introduced the δ function. This had been used previously by Heaviside and others, but Dirac's use of it in dealing with continuous variables started its appeal to physicists. Its use was at first resisted by mathematicians, including even John von Neumann, who was himself an expert in quantum mechanics. Physicists went on using it; many years later it was made respectable by the 'theory of distributions' of Laurent Schwartz.

A number of papers deal with radiation, starting with reference 14, which treats the absorption and stimulated emission of light by atoms (Einstein's B coefficients). These radiative processes can be dealt with by including a given external light wave in the quantum equations for the atom. This problem had already been discussed by Schrödinger in the last of his papers deriving wave mechanics, but Dirac's treatment was clearer and more complete.

In reference 17 (written in Copenhagen) he goes much further and applies the quantum theory also to the electromagnetic field, which allows him to include also the spontaneous emission consistently. He uses two different approaches to the treatment of radiation: one is to write a wave equation for light quanta, treated as particles. This wave equation has, for each of the particles, the same form, though not the same physical significance, as Maxwell's equation for the electromagnetic fields. The other approach is to treat the fields as dynamical variables and apply to them the quantum rules. He shows the equivalence of these two approaches. This paper thus established the method of 'second quantization'. It contains the basis of field theory, which underlies all subsequent work on the quantum theory of the electromagnetic field and the many new fields introduced in physics since. Reference 18 (written in Göttingen) extends the theory to include dispersion. This paper gives the first quantum mechanical derivation of the Lorentzian shape of spectral lines.

Dirac was very conscious that quantum mechanics was then still non-relativistic, and he made an effort to extend it to situations for which relativity was essential. This work led to two papers (11, 15). In the latter he uses the Klein–Gordon wave equation, but he was not satisfied with it. After having developed the transformation theory (16) he was firmly convinced that ***a correct wave equation should contain only first-order time derivatives,*** whereas the Klein–Gordon equation is of second order, a difficulty that did not seem to worry his contemporaries.

It was by this reasoning that he arrived at the 'Dirac equation' (20, 21), probably his greatest single contribution to physics. He derived it from the requirement that there must exist a positive definite, conserved, particle density. This, with Lorentz invariance, appeared to require a four-component wave function. It turned out that the four components corresponded to the two possible spin orientations and to positive and negative values of the energy.

The result that the electron must necessarily have a spin of half a unit was a great surprise and a great success of the theory, and allowed the electron spin to be fitted consistently in the description of the electron. Many years later Pauli and Weisskopf showed that quantum theory also permits particles with spin of zero or one unit if one drops the requirement that the coordinate of each particle should be observable. This disproved Dirac's result that particles must necessarily have half-integral spin, but this does not detract from his great merit in finding the right description for the electron and its spin. We have already described the tremendous impact of this paper on the physics world.

Dirac applied the equation to an approximate treatment of the hydrogen atom showing that, to the leading order in the relativistic corrections, one reproduces the Pauli equations with spin. The paper also treats the selection rules governing radiative transitions and the Zeeman effect. W. Gordon and C. G. Darwin later showed, without approximation, that the equation gives the correct energy levels for hydrogen.

However, Dirac was disappointed that the theory still contained the unphysical states of negative energy. These arise from the fact that the relativistic relation between momentum and energy,

$$E^2 = c^2p^2 + m^2c^4,$$

is quadratic. This is also true in classical relativity, but there the energy changes continuously with time, so if all particle energies are positive to start with they will stay positive. However, in quantum mechanics the particles could jump from positive to negative energy. Dirac had hoped that an equation that was first-order in the time derivative, i.e. linear in the energy, would no longer allow negative energies, but they were still there.

To get out of this difficulty he boldly introduced (26) the 'hole theory' by which all negative-energy states are normally filled so that the vacuum contains an infinite number of electrons, only deviations from that normal infinite density being observable. He shows that this theory predicts correctly the scattering of light by atoms, although the previous treatment of this relied on virtual transitions to and from negative-energy states. In the hole theory these are replaced by virtual transitions from negative-energy to positive-energy states and back.

Vacancies in negative-energy states behave like positive charges, with positive energy, and the obvious assumption would have been that this was a positively charged particle with the same mass as the electron. It is interesting to note here that he was aware very early, as we can see from the lecture (23) that he gave at Leipzig in 1928, and from the last few paragraphs of the 1930 edition of his book, that the negative-energy wave function for a particle of mass m, energy $-\epsilon$ and charge e in a given field is directly related to the positive-energy wave function for a particle with mass m, energy $+\epsilon$ but charge $-e$, in the same field (this relation is known today as charge conjugation), and that he referred to them as 'positive-charge wave functions' as opposed to the usual negative-charge wave functions. But physicists were convinced that a positively charged electron, if it existed, could not have escaped detection. Dirac therefore tried to identify the holes with protons. On this basis it would be hard to explain that the proton and the electron have so very different masses. Also the stability of the hydrogen atom could not be understood, because the electron should very rapidly fall into the hole, which was the proton, with the emission of radiation (27). Dirac speculated that perhaps the Coulomb interaction between the electrons could explain the large mass difference and suppress the decay of the hydrogen atom, but this seemed unlikely. At this point Oppenheimer suggested that negative-energy states had nothing to do with protons, and that they were all filled all the time.

Dirac then remarked (33) that even if the negative-energy states were normally all filled, electrons could be raised from them, for example by the collision of two energetic γ-rays, and this would result in a positive-energy electron and a positive particle, which he called 'anti-electron'. He remarked that with present γ-ray intensities this phenomenon could not be observed; he did not see that it could be done with one γ-ray in the Coulomb field of a nucleus. But in this remark he did predict the anti-electron, which was discovered a year later and was given the name positron. The difficulty Dirac had in getting his prediction taken seriously at Cambridge is well illustrated by the following story. In the autumn of 1932, before Anderson had published his picture of the positron, Blackett reported his triggered cloud-chamber observations at the Cavendish Laboratory colloquium, and concluded that the particles he observed to curl the wrong way were either positively charged electrons or else electrons coming upwards from the Earth. As M. H. L. Pryce recalls, Kapitza then turned to Dirac in the front row, saying 'Now, Dirac, put that into your theory! Positive electrons, eh!—positive electrons!', to which Dirac replied quietly 'Oh, but positive electrons have been in the theory for a very long time'.

Dirac's fundamental contributions to quantum mechanics include his book, *The principles of quantum mechanics*, first published in 1930 (30). It was the first full and systematic textbook on the subject, and soon became the standard work. In the first edition the initial chapters were somewhat abstract; in the second edition, published in 1935, he improved the presentation by giving more details of the physical significance of the formalism. He also added a chapter on field theory, of which the outline had become established, as further developments of his seminal papers. The third (1947) edition introduced the 'bra' and 'ket' notation of which Dirac was very fond. It separated the treatment of discrete and continuous spectra into two separate chapters, and extended the final chapter, which deals specifically with quantum electrodynamics and not with other fields. In the fourth edition, 1957, the last chapter was again re-written, with emphasis on electron pair creation and annihilation. A revised fourth edition, published in 1967, had only minor changes.

2. *Other early contributions to quantum mechanics*

In the period before World War II Dirac made many other contributions to quantum mechanics. Some are now rather dated but others remain important, if not as fundamental as the ones mentioned above. Reference 13 is a mathematical study of functional relations between q numbers. Although the result is correct the derivation makes two erroneous assumptions, as he points out in reference 162. Reference 19 shows how to handle scattering problems in momentum space. In this one has to integrate quantities that have poles because of vanishing denominators. His way of dealing with these is correct, but by modern standards clumsy.

Reference 24 deals with the use of the density matrix in statistical mechanics. This had been introduced in a paper by von Neumann, but Dirac put it in a language intelligible to physicists. His paper stimulated a very widespread use of this concept. It is typical that the density matrix is now almost universally denoted by ρ, the letter used in Dirac's paper. In reference 32 the one-electron density matrix is shown to be related to the Hartree–Fock wave function.

Another ingenious device is developed in reference 25. This is to use the permutation operator as a dynamical variable in place of the total spin. This proved useful, but was later superseded by the method of Slater determinants.

Reference 28 deals with the Thomas–Fermi approximation for large atoms, and shows that it is possible to include exchange effects, and reference 52 develops the use of complex quantities as variables in quantum mechanics. This includes the familiar creation and annihilation operators.

Reference 38 contains the idea of path integrals, an extremely powerful and imaginative formulation of quantum mechanics, later developed by Feynman, and now widely known as the 'Feynman sum over histories' or 'Feynman path integral'. This differs from the normal formalism by making it easy to gain certain insights into physical problems that would be hard to recognize otherwise. For example, by transforming to a Euclidean four space by using a pure imaginary time coordinate, this sum becomes recognized as the partition function appropriate to a particular statistical-mechanics problem. The exploitation of this connection with statistical mechanics, and of our wide experience in that field, has led to major advances recently in our qualitative (and, to a lesser extent, quantitative) understanding of the content of the strong-interaction quantum field theories proposed to account for the hadronic particles, especially in the lattice approximation for the space–time continuum. At the same time it is also different in that it uses as starting point the Lagrangian rather than the Hamiltonian, and may therefore be applicable to systems in which one cannot conveniently use a Hamiltonian. Because almost all the many theories currently proposed are based on the gauge principle and because gauge theories have their natural formulation in terms of a Lagrangian, it is this second feature that has led to the method being used very extensively in the current literature.

Dirac always regarded notation and terminology as important. After his bracket notation had come into general use he introduced (57) the idea of half-brackets for state vectors and their conjugates, and called them 'bra' and 'ket'. He was probably not aware of the colloquial meaning of 'bra'.

3. *Quantum field theory*

In 1927 Dirac had been the first to tackle the problem of quantizing the electromagnetic field. Later he saw the need for making the theory relativistic, not only in substance but in form. One approach to this is the many-times formalism, first sketched (36) for the interaction of two electrons in one dimension, and elaborated (37) jointly with Fock & Podolski. The spirit of this method, which associated a separate time with each particle, was a step towards Tomonaga's later use of a separate time for each field point in space, the concept of a 'space-like surface', which makes the relativistic properties of the theory particularly transparent.

However, it soon became clear from his work and that of others that quantum electrodynamics led to infinities, which barred the way to a consistent theory. These have several sources, including the possibility of virtual pair creation, and the self-field of the electron, i.e. the interaction of an electron with its own field, which is also responsible for the physically real radiative reaction. To study the latter, Dirac considered the classical (i.e. non-quantum) equation for an electron interacting with its own field, and formulated equations whose solutions had no infinities (55). But the equations allowed 'runaway solutions' in which the electron keeps accelerating. In reference 55 he notes that the equations amount to the assumption of an infinite negative mechanical mass that cancels the infinite self-energy, but he rejects this interpretation. This idea really anticipates that of 'renormalization'. More details of this classical theory were worked out by his student, C. Eliezer. The approach was interesting but did not help to eliminate the troubles of the quantum field theory.

Dirac felt it necessary to look for a remedy. Even when the work of Tomonaga, Schwinger, Feynman and Dyson showed how finite results could be obtained from the usual theory in spite of the infinities, and these results were shown to give excellent agreement with experiment, he was not satisfied and continued his search for a better theory. Some of these attempts were concerned with finding better variables to describe the processes, so as to identify more clearly the physically real part of the solution. This included reference 77, in which he follows Tomonaga in using a general space-like surface to specify the state of the field, rather than constant time, but in Schrödinger representation, in place of the more usual interaction representation.

References 82 and 84 develop a method to obtain equations in Hamiltonian form when there are constraints between the field quantities. This is another seminal paper because such constraints arise in all gauge theories. The method therefore applies to all modern particle theories, which always include gauge fields. Dirac also made use of it in discussing general relativity. Reference 97 suggests allowing general gauge transformations, not only those preserving the Lorentz condition. However, the difficulties over defining the vacuum state remain.

Alongside attempts to re-formulate the standard theory he considered many possible modifications to it. One of these involves a limiting process that avoids the singularities of the field. Such a method was suggested by Wentzel, and is discussed by Dirac in reference 58 and in his Bakerian lecture (61); this 'λ-limiting process' is further developed in reference 67. Reference 74 is on the same subject. A more radical departure is tried in reference 81, where the choice of gauge for the vector potential is linked to the choice of the space-like surface used to describe the state of the field.

Even more revolutionary is a proposal (86) claiming that an ether is not necessarily incompatible with relativity, if its velocity is not fixed, but has a statistical distribution. This idea is elaborated (87), relating to streams of electrons; here it is postulated that the magnitude of the vector potential, which in the usual theory is arbitrary, might be fixed, equal to the mass-to-charge ratio, m/e, of the electron. The direction of the vector is then related to the stream velocity. (It is not clear what happens if the same electromagnetic field interacts also with particles of a different mass-to-charge ratio, such as protons.) The proposed scheme is clarified in two notes (88) replying to criticism by Bondi and Gold and to Infeld, who claims that reference 87 does not necessarily imply an ether velocity. In reference 89 the scheme is extended to allow the electron stream to have vorticity. References 91 and 94 elaborate the theory further.

Then he tried to use, in Schrödinger representation, a corrected version of the usual theory (100), which eliminates the virtual creation of electron–positron pairs one at a time, while retaining the (rather rare) process creating several pairs simultaneously.

In reference 109 he considers the Born–Infeld non-linear electrodynamics, and shows that it can be put in Hamiltonian form, but its quantization gives difficulties.

In reference 114 he tries a classical model of an extended electron held together by surface tension. By using Bohr–Sommerfeld quantum conditions, the first excited state would have a mass of 53 electron masses. He suggests that this could perhaps be a model of the muon, but this hypothesis could not be maintained.

In reference 119 he proposes a novel form in which to use the Heisenberg representation. This avoids talking about vacuum-to-vacuum transitions, but reproduces all practical results of the usual theory. When this method was questioned by Perlman he replied in its defence (121). Continuing these thoughts he insisted that 'the Heisenberg representation is a good representation; the Schrödinger representation is a bad representation.' (123) The point of this remark is that in the Schrödinger equation one is using the state function, and that even for the vacuum the state function is impossible to evaluate.

In a series of lectures on quantum field theory (124) he expounds the methods developed previously (119, 123). Although some questions that give infinite answers are eliminated, one still has to do infinite mass renormalization.

He later returns to this situation (129). The Schrödinger representation is troublesome because it is impossible to determine the vacuum state. The Heisenberg equations (with a cut-off) do not have this trouble, but are hard to interpret. Another version is put forward in reference 131. This uses the Heisenberg equation for the fields but treats it as applicable to the bare vacuum. By this method one can calculate the Lamb shift correctly, but not the properties of the photon.

References 134, 132, 137 and 139 study a wave equation in which the wave function contains two internal variables besides the ordinary space–time coordinates. This equation admits only positive-energy solutions. He does not tell us what physical application he has in mind; such an equation would describe a particle without antiparticle, an object not so far discovered. In his 1975 lectures in Australia and New Zealand (170) he reports that such a particle could not have any electromagnetic interaction. Nevertheless, he returns to the idea (189) and suggests that this type of theory may prove useful.

It was disappointing that, in spite of so many determined attempts, the goal of finding a finite and satisfactory field theory eluded him. Yet he was not willing to accept the current theory, in spite of its successes. In a lecture to the meeting of Nobel Laureates in Lindau in 1982 (191) he insists:

> 'Physicists should not be working with a falsification of the Heisenberg equations. I have spent many years looking for a good Hamiltonian to put into the theory and have not found it. I shall continue to work on it as long as I can, and other people, I hope, will follow along the same lines. Some day people will find the correct Hamiltonian, and then there will be some new degrees of freedom, something we cannot understand according to classical ideas, playing a role in the foundations of quantum mechanics.'

4. *General relativity*

Dirac was interested in relativity before he became involved in quantum mechanics, and he never lost his interest in that subject. Some of his early work is concerned with writing quantum equations in a space with a general metric. Reference 54 shows how to write the Maxwell and Dirac equations in de Sitter space, and reference 104 shows that there is no difficulty with the Dirac equation in a gravitational field. For a zero-mass particle, in particular, one can still use a two-component wave function, as is done in flat space for the neutrino.

A few other papers deal with problems in general relativity as such. He considers the controversial problem of gravitational waves (106), and shows that the field energy can be defined in the presence of such waves. In his 1959 Lindau lecture (108) he gives a review of the problem of gravitational waves. This is also mentioned in his very clear published lectures on general relativity (154).

Reference 112 discusses the energy density of the gravitational field and proposes a definition that is somewhat less dependent on the choice of coordinate system than the obvious one.

However, his main preoccupation was with the problem of quantizing the gravitational field. In preparation for this he developed a technique for extending Hamiltonian theory to cases in which there are identities between the canonical momenta (102). This technique, now known as 'gauge fixing', is applied to general relativity (103). In reference 105 he uses these methods to write general relativity in Hamiltonian form and, in principle, to quantize it.

Reference 113 points out that DeWitt's method for treating gravitational interactions is incomplete, but can be improved by the method shown in reference 103. Reference 115 is a discussion remark about a theory in which the particles are 'bubbles' in the gravitational field, in a way similar to that for the electron theory of reference 114.

Finally there are some general remarks about the basis of relativity. In reference 143 he comments that if one were to discover some disagreement between theory and observation, this would not destroy Einstein's theory but merely introduce some secondary features. Remarks about the importance of mathematical beauty for fundamental equations can be found as early as 1939 (56).

The same point is made in reference 182: Einstein's principle that all states of motion are equivalent is disproved by the discovery of the cosmic microwave background. Nevertheless, we believe both in the special and the general theory. 'It is the essential beauty of the theory which, I feel, is the real reason for believing in it.' One should take the Einstein–de Sitter model seriously.

He returns to the Einstein–de Sitter model in reference 186. Agreement is improved if one takes account of the density fluctuations due to the existence of stars. This does not affect the overall results, and is compatible with the two-timescale model (see section 6 below).

5. *Magnetic monopole*

Current theory allows electric charges to exist in the electromagnetic field, but not magnetic poles. In 1931 Dirac noticed (33) that the wave equation for an electron in the field of a point-like magnetic pole has solutions, provided the strength of the pole is $n\hbar c/2e$, e being the electron charge and n an integer. Conversely, if a magnetic pole of strength $\hbar c/2e$ existed, quantum mechanics would allow only charges that would be multiples of e. In the paper Dirac remarks 'One would be surprised if nature had made no use of it'.

He discussed (78) the interaction of magnetic poles and electrons. The pole is at the end of an unobservable 'string', i.e. a line along which the vector potential is singular. The location of the string has, however, no physical significance, because any other location can be obtained by a gauge transformation, provided only its end remains anchored at the pole. This is the first time topological considerations are used in quantum physics; this work is a forerunner of more recent developments in quantum field theory involving topology.

Dirac returns to the monopole problem in several papers (159, 164, 171). In setting up a quantum theory of electrons and magnetic poles one finds infinities, but they are no worse than those found with electrons alone. To obtain reasonable results one should therefore consider monopoles and/or electrons of finite size. This requires new methods. He develops methods for dealing with such extended objects, though the specific models he considers are unrealistic in that the particles would gradually spread out. If the magnetic poles are extended, the 'strings' attached to them would also become bundles. There are similarities between these concepts and some more recent theories, also involving strings that are claimed to be free of infinities.

In a general review talk (165) he reports on the theory, and mentions the experiment of Price, which appeared at the time to give some indication of the existence of monopoles.

6. *The large-numbers hypothesis*

In 1937 Dirac put forward the hypothesis that extremely large numbers, such as the ratio between the electric and gravitational interaction of two atomic particles, had no place in the fundamental laws of physics. Because the age of the universe at the present epoch, measured in atomic units, is also an extremely large number, he conjectured that all the large numbers might be functions of the age, i.e. of time. This idea was called the 'large-numbers hypothesis' or L.N.H.

The idea was first mentioned in a note to *Nature* (50). In reference 51 he replies to an objection by Dingle, and later he spells out the scheme in more detail (54).

Reference 117 is not directly related to the L.N.H., but deals also with the fundamental constants. He considers that $e^2/\hbar c$, being a dimensionless number of reasonable magnitude, should be derivable from general principles. Then only either e or $\hbar$ can be fundamental. If it is $\hbar$, then e, the derived quantity, should contain a square root, which is unlikely. Therefore it is probable that e is fundamental and $\hbar$ derived. Then 'our whole view of the uncertainty principle would be altered'.

He did not return to the L.N.H. until 1961 when he replied to a critical letter by H. Dicke (111). According to Dicke life would be possible only for a relatively short period during the evolution of the universe. In presenting his model Dirac remarks 'I prefer the theory which allows the possibility of an endless life.'

Reference 143 invokes Weyl's theory for a gravitational constant varying with time. The paper also discusses the relation between Weyl's gauge transformation and that of electrodynamics.

Another objection raised by Teller is discussed in reference 135. Teller's argument was that a decreasing g would make the age of the Sun shorter than is compatible with geological evidence. Dirac sees two possible ways of reconciling these facts. One is that the Sun's mass might have increased by dust accretion. The other, more revolutionary, idea is that there are two timescales, one atomic time, which governs atomic and nuclear phenomena, and the other global time, appearing in the equations of general relativity. The one time would vary as a power of the other, if both are measured from the origin of the universe. He points out that this idea of two timescales was first proposed by Milne, though for quite different purposes. Dirac used this two-timescales model in all subsequent discussions of the L.N.H.

For example, one consequence of the original form of L.N.H. is that the number of protons in the universe, which is a large number, would also have to grow with time, so there would have to be continuous creation of matter. This could be either uniform in space or proportional to the existing matter density (148). But references 153 and 174 point out that the need for continuous creation can be avoided by the use of the two timescales. This also invalidates other objections (152). The idea of the two times is explained in detail in reference 187.

Reference 175 claims that the discovery of the cosmic microwave background confirms the L.N.H., which predicts a much slower decrease of temperature with time, so the radiant temperature would extrapolate back to a value about $m_p c^2/k$ at a time close to the origin, thus obviating the need for late 'decoupling' between matter and radiation.

In the same paper he points out that the L.N.H. predicts a spiralling inward of planetary orbits, which should be close to observational possibilities.

He also refers to a discrepancy between the maximum age of the Moon (181), deduced from its orbit, and the age of rocks determined by their content of radioactive elements. This discrepancy can be explained by the two timescales, because the Moon's orbit would follow global time, whereas the radioactivity would be governed by atomic time.

7. *Mathematical methods*

Dirac developed numerous mathematical methods and devices, some with an obvious application in mind, others only in the general expectation that they might lead somewhere.

For example, he pointed out that homogeneous variables can be useful in describing a particle of zero rest-mass in free space (40), and developed the use of complex quantities as operators (52). This included the creation and annihilation operators now in general use. His treatment is the clearest general discussion of their properties.

Reference 70 shows the existence of an infinite-dimensional representation of the Lorentz group. This became the thesis subject of Harish-Chandra and led to important developments in mathematics. Reference 193 introduces what he calls 'pathological' representations that, he believes, may lead to progress in physics. We have some difficulty in following the arguments of this paper. Reference 72 shows how to generate Lorentz transformations by using quaternions.

Reference 71 shows how in field theory one can define functions of non-commuting variables by using time ordering. Dirac shows (79) that there are three consistent and convenient schemes for use in relativistic theory, using different sets of surfaces: (*i*) $t = \text{const}$, (*ii*) $x^2 - t^2 = \text{const}$, or (*iii*) $x - t = \text{const}$.

Reference 96 shows that the stress tensor is ambiguous if only the total Hamiltonian is known. It is determined if one knows also a Hamiltonian density, corresponding to a general infinitesimal displacement of the reference surface. Conversely, knowledge of the stress tensor leads to a unique Hamiltonian density if the total Hamiltonian is known.

Reference 118 shows that there is a representation of the 3+2 dimensional de Sitter group for which the eigenvalues are integral for space rotations and half-integral for rotations in the 4th and 5th coordinates.

8. *Pre-quantum-mechanics papers*

The early papers that Dirac wrote while a graduate student were mostly comments on papers by senior authors, or extensions. They show, at this early stage, great confidence in his own reasoning.

Reference 1, written at Fowler's suggestion, is an intelligent application of statistical mechanics, and reference 2 shows that what Eddington calls kinematic and dynamic velocities are in fact identical. This paper was communicated by Eddington. Dirac shows (3) that Bohr's frequency condition is Lorentz invariant, provided one assumes the photon to have momentum $h\nu/c$ (a condition he attributes to Schrödinger). Reference 5 is a rather complicated discussion of adiabatic invariance, generalizing Burgers's, and reference 6 shows that Milne is wrong in attributing the line shape in stellar spectra to the Compton effect. (This paper was communicated by Milne.) Reference 7 shows that adiabatic invariance applies in a magnetic field, contrary to a statement by Sommerfeld.

From this brief digest of Dirac's work his great originality, inventiveness, vision and persistence will be apparent. His contributions to the foundations of quantum mechanics, the Dirac equation, and the positron, by themselves secure his position as one of the creators of modern theoretical physics. Of the later papers many represent ingenious, but unsuccessful, attempts to get over important difficulties. On many others the verdict is still open, but they might well contain germs, as yet unrecognized, of important future insights.

Honours

1930	Fellow, Royal Society.
	Hopkins Medal for 1927–30, Cambridge Philosophical Society.
1931	Corresponding Member, U.S.S.R. Academy of Sciences.
1933	Nobel Prize for Physics (jointly with Schrödinger).
1939	Royal Medal, Royal Society.
	Hon. Member, Indian Academy of Science.
	James Scott Prize, Royal Society of Edinburgh.
1941	The Bakerian Lecturer, Royal Society.
1943	Hon. Member, Chinese Physical Society.
1944	Hon. Member, Royal Irish Academy.
1946	Hon. Fellow, Royal Society of Edinburgh.
	Associé étranger, Académie des Sciences Morales et Politiques, Institut de France.
1947	Hon. Fellow, National Institute of Sciences of India.
1948	Hon. Member, American Physical Society.
1949	Foreign Assoc., U.S. National Academy of Sciences.
1950	Foreign Hon. Member, American Academy of Arts and Sciences.
1951	Foreign Member, Accademia delle Scienze di Torino.
1952	Max-Planck Medal, Association of German Physical Societies.
	Copley Medal, Royal Society.
1953	Foreign Member, Academia das Ciencias de Lisboa.
1958	Member, Deutsche Akademie der Naturforscher 'Leopoldina' zu Halle.
	Member, Pontifical Academy of Sciences, Vatican City.
	Hon. Fellow, Tata Institute for Fundamental Research, Bombay, India.
1960	Foreign Member, Accademia Nazionale dei Lincei, Rome.
1962	Hon. Member, Royal Danish Academy.
1963	Associé étranger, Académie des Sciences, Institut de France.
1964	Helmholtz Medal, Akademie der Wissenschaften der D.D.R.
1969	Oppenheimer Prize, Center for Theoretical Studies, University of Miami.
1973	Order of Merit.
1979	Hon. Member, Hungarian Academy of Sciences.

Dirac consistently refused to accept honorary degrees. When in 1934 he was offered an honorary degree by the University of Bristol he wrote:

'I would like to think that my main work lies in the future and that I have still to earn any honours that may eventually be conferred upon me'. Later he often explained that he could not accept an honorary degree after having refused one from Bristol, his *alma mater*. At least two honorary degrees were conferred without consulting him.

This list of honours is probably incomplete, as, characteristically, he never listed honours in his entry in *Who's Who*.

Representations of Dirac

Dirac's portrait was painted in oils by Michael Noakes in 1978 and this is now held by St John's College, with a second portrait of interest, painted about 1947 by an Indian mathematics student, D. V. A. S. Amarasekara. The College also holds a sketch of Dirac made by R. Tollast in 1963 and a bronze bust made by the Danish sculptor Harald Isenstein in 1971 from a plaster cast bust he had made in 1939. A copper bust of Dirac was made in 1973 by Mrs G. Bollobás and is now held by the Royal Society; a second copy of this bust is held by the Department of Applied Mathematics and Theoretical Physics of Cambridge University. Many photographs of Dirac exist, of course. Besides those reproduced here, those appearing in the Solvay Congress Reports (Mehra 1975) and those printed by Salam & Wigner (1972) and by Mehra (1973), it is worth drawing attention to the sequence of photographs printed in the obituary for P. A. M. Dirac by H. G. B. Casimir (1985).

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One of the frontispiece photographs was taken in about 1925 and was published in *The Old Cothamian* for 1937, the other was taken at a Lindau meeting, most probably that for 1959.

Bibliography

(1) 1924 Dissociation under a temperature gradient. *Proc. Camb. phil. Soc.* **22**, 132–137.
(2) Note on the relativity dynamics of a particle. *Phil. Mag.* **47**, 1158–1159.
(3) Note on the Doppler principle and Bohr's frequency condition. *Proc. Camb. phil. Soc.* **22**, 432–433.
(4) The conditions for statistical equilibrium between atoms, electrons and radiation. *Proc. R. Soc. Lond.* A **106**, 581–596.
(5) 1925 The adiabatic invariants of the quantum integrals. *Proc. R. Soc. Lond.* A **107**, 725–734.
(6) The effect of Compton scattering by free electrons in a stellar atmosphere. *Mon. Not. R. astr. Soc.* **85**, 825–832.
(7) The adiabatic hypothesis for magnetic fields. *Proc. Camb. phil. Soc.* **23**, 69–72.
(8) The fundamental equations of quantum mechanics. *Proc. R. Soc. Lond.* A **109**, 642–653.
(9) 1926 Quantum mechanics and a preliminary investigation of the hydrogen atom. *Proc. R. Soc. Lond.* A **110**, 561–579.
(10) The elimination of the nodes in quantum mechanics. *Proc. R. Soc. Lond.* A **111**, 281–305.
(11) Relativity quantum mechanics with an application to Compton scattering. *Proc. R. Soc. Lond.* A **111**, 405–423.
(12) *Quantum mechanics.* Cambridge University dissertation, May 1926, 136 pages.
(13) On quantum algebra. *Proc. Camb. phil. Soc.* **23**, 412–418.
(14) On the theory of quantum mechanics. *Proc. R. Soc. Lond.* A **112**, 661–677.
(15) The Compton effect in wave mechanics. *Proc. Camb. phil. Soc.* **23**, 500–507.
(16) 1927 The physical interpretation of the quantum dynamics. *Proc. R. Soc. Lond.* A **113**, 621–641.
(17) The quantum theory of the emission and absorption of radiation. *Proc. R. Soc. Lond.* A **114**, 243–265.
(18) The quantum theory of dispersion. *Proc. R. Soc. Lond.* A **114**, 710–728.
(19) Über die Quantenmechanik der Stossvorgänge. *Z. Phys.* **44**, 585–595.
(20) 1928 The quantum theory of the electron. *Proc. R. Soc. Lond.* A **117**, 610–624.
(21) The quantum theory of the electron. Part II. *Proc. R. Soc. Lond.* A **118**, 351–361.
(22) Discussion. In *Electrons and photons* (5th Conseil de physique de l'Institut International de Physique, Solvay, 24–29 October 1927, Bruxelles), pp. 258–263. Paris: Gauthier-Villars.
(23) Über die Quantentheorie des Elektrons. *Phys. Z.* **29**, 561–563. (Report on Dirac's lecture at the 'Leipziger Universitätswoche', 18–23 June 1928.)
(24) 1929 The basis of statistical quantum mechanics. *Proc. Camb. phil. Soc.* **25**, 62–66.
(25) Quantum mechanics of many-electron systems. *Proc. R. Soc. Lond.* A **123**, 714–733.
(26) 1930 A theory of electrons and protons. *Proc. R. Soc. Lond.* A **126**, 360–365.
(27) On the annihilation of electrons and protons. *Proc. Camb. phil. Soc.* **26**, 361–375.

(28) Note on exchange phenomena in the Thomas atom. *Proc. Camb. phil. Soc.* **26**, 376–385.
(29) The proton. *Nature, Lond.* **126**, 605–606.
(30) *The principles of quantum mechanics.* Oxford: Clarendon Press.
(31) Approximate methods. (Manuscript in English held at Churchill College Archives (Cambridge, England). This is the text of ch. XIA of the Russian edition of ref. 30 (M.-L. GTTI, 1932; translated by M. P. Bronshtein, ed. D. D. Iwanenko), pp. 243–257, with the note 'Author's addition to the Russian edition'.)
(32) 1931 Note on the interpretation of the density matrix in the many electron problem. *Proc. Camb. phil. Soc.* **27**, 240–243.
(33) Quantized singularities in the electromagnetic field. *Proc. R. Soc. Lond.* A **133**, 60–72.
(34) Quelques problèmes de mécanique quantique. *Annls Inst. Henri Poincaré* **1** (4), 357–400.
(35) 1932 (With J. W. Harding) Photo-electric absorption in hydrogen-like atoms. *Proc. Camb. phil. Soc.* **28**, 209–218.
(36) Relativistic quantum mechanics. *Proc. R. Soc. Lond.* A **136**, 453–464.
(37) (With V. A. Fock & B. Podolsky) On quantum electrodynamics. *Phys. Z. SowjUn.* **2**, 468–479.
(38) 1933 The Lagrangian in quantum mechanics. *Phys. Z. SowjUn.* **3**, 64–72.
(39) (With P. Kapitza) The reflection of electrons from standing light waves. *Proc. Camb. phil. Soc.* **29**, 297–300.
(40) Homogeneous variables in classical dynamics. *Proc. Camb. phil. Soc.* **29**, 389–400.
(41) Statement of a problem in quantum mechanics. *J. Lond. math. Soc.* **8**, 274–277.
(42) 1934 *Théorie du positron (Septième Conseil de Physique Solvay (Structure et propriétés des noyaux atomiques), 22–29 October 1933)*, pp. 203–230. Paris: Gauthier-Villars.
(43) Teoriya pozitrona. In *Atomnoe yadro, sbornik dokladov i vsesoyuznoi yadernoi konferentsii* (ed. M. P. Bronshtein, V. M. Dukel'skii, D. D. Ivanenko & Yo. B. Khariton), pp. 129–143. Leningrad and Moscow: Izdatelstvo, Akademii Nauk S.S.R.
(44) Theory of electrons and positrons. Nobel lectures—physics 1922–41, pp. 320–325. (Amsterdam: 1965.)
(45) Discussion of the infinite distribution of electrons in the theory of the positron. *Proc. Camb. phil. Soc.* **30**, 150–163.
(46) 1935 The electron wave equation in De-Sitter space. *Ann. Math.* **36**, 657–669.
(47) 1936 Does conservation of energy hold in atomic processes? *Nature, Lond.* **137**, 298–299.
(48) Relativistic wave equations. *Proc. R. Soc. Lond.* A **155**, 447–459.
(49) Wave equations in conformal space. *Ann. Math.* **37**, 429–442.
(50) 1937 The cosmological constants. *Nature, Lond.* **139**, 323.
(51) Physical science and philosophy (a reply to Dr H. Dingle). *Nature, Lond.* **139**, 1001–1002.
(52) Complex variables in quantum mechanics. *Proc. R. Soc. Lond.* A **160**, 48–59.
(53) The reversal operator in quantum mechanics. *Izv. Akad. Nauk SSSR*, no. 4–5, 569–575.
(54) 1938 A new basis for cosmology. *Proc. R. Soc. Lond.* A **165**, 199–208.
(55) Classical theory of radiating electrons. *Proc. R. Soc. Lond.* A **167**, 148–169.
(56) 1939 The relation between mathematics and physics (James Scott Prize Lecture). *Proc. R. Soc. Edinb.* **59**, 122–129.
(57) A new notation for quantum mechanics. *Proc. Camb. phil. Soc.* **35**, 416–418.
(58) La théorie de l'électron et du champ électromagnetique. *Annls Inst. Henri Poincaré* **9**, 13–49.
(59) 1940 Dr M. Mathisson (obituary). *Nature, Lond.* **146**, 613.
(60) 1941 The theory of the separation of isotopes by statistical methods (n.d. or p.). (Unpublished. Cohen (1951) refers to it on pp. 29 and 125 as 'British Report 1941'.)
(61) 1942 The physical interpretation of quantum mechanics (Bakerian Lecture 1941). *Proc. R. Soc. Lond.* A **180**, 1–40.
(62) (With R. Peierls & M. H. L. Pryce) On Lorentz invariance in the quantum theory. *Proc. Camb. phil. Soc.* **38**, 193–200.
(63) *The motion in a self-fractionating centrifuge.* Rep. no. BR 42, May 1942; declassified 1946 as Report BDDA 7. London: HMSO.
(64) *Expansion of U-sphere enclosed in a container.* Rep. no. MSD 2.

(65) *Neutron multiplication in a sphere of uniform density surrounded by a shell of non-uniform density*. Rep. no. MSD 3.

(66) *Estimate of the efficiency of energy release with a non-scattering container*. Rep. no. MSD 4.

(67) 1943 Quantum electrodynamics. *Communs Dubl. Inst. advd Stud.* A, no. 1, pp. 1–36.

(68) *Approximate rate of neutron multiplication for a solid of arbitrary shape and uniform density*. Rep. no. MSD 5, part I, 11 pages.

(69) (With K. Fuchs, R. Peierls & P. Preston) *Application to the oblate spheroid hemisphere and oblate hemispheroid*. Rep. no. MSD 5, part II, 12 pages, 5 figs.

(70) 1945 Unitary representations of the Lorentz group. *Proc. R. Soc. Lond.* A **183**, 284–295.

(71) On the analogy between classical and quantum mechanics. *Rev. mod. Phys.* **17**, 195–199.

(72) Applications of quaterions to Lorentz transformations. *Proc. R. Irish Acad.* A **50**, 261–270.

(73) *Quelques développments sur la théorie atomique*. (Conference faite au Palais de la Découverte, le 6 Decembre 1945.) Paris: Université de Paris, 1948. 16 pages.

(74) 1946 Developments in quantum electrodynamics. *Communs Dubl. Inst. advd. Stud.* A, no. 3, pp. 1–33.

(75) 1947 The difficulties in quantum electrodynamics. *Rep. Int. Conf. on Fundamental Particles and Low Temperatures, July 1946* **1**, 10–14. London: Physical Society.

(76) 1948 On the theory of point electrons. *Phil. Mag.* **39**, 31–34.

(77) Quantum theory of localizable dynamic systems. *Phys. Rev.* **73**, 1092–1103.

(78) The theory of magnetic poles. *Phys. Rev.* **74**, 817–830.

(79) 1949 Forms of relativistic dynamics. *Rev. mod. Phys.* **21**, 392–399.

(80) La seconde quantification. *Annls Inst. Henri Poincaré* **11** (1), 15–47.

(81) 1950 A new meaning for gauge transformations in electrodynamics. *Nuovo Cim.* (9) **7**, 925–938.

(82) Generalized Hamiltonian dynamics. *Can. J. Math.* **2**, 129–148.

(83) Field theory. In *Proc. of the Harwell Nuclear Physics Conference, September 1950*. (*AERE* rep. no. 68, 114–115.)

(84) 1951 The Hamiltonian form of field dynamics. *Can. J. Math.* **3**, 1–23.

(85) The relation of classical to quantum mechanics. In *Proc. Second Canadian Math. Congress, Vancouver 1949*, pp. 10–31. Toronto: University of Toronto Press.

(86) Is there an aether? *Nature, Lond.* **168**, 906–907.

(87) A new classical theory of electrons. *Proc. R. Soc. Lond.* A **209**, 291–296.

(88) 1952 Is there an aether? *Nature, Lond.* **169**, 146 (reply to H. Bondi and T. Gold); *ibid.* **169**, 702 (reply to L. Infeld).

(89) A new classical theory of electrons. II. *Proc. R. Soc. Lond.* A **212**, 330–339.

(90) Les transformations de jauge an électrodynamique. *Annls Inst. Henri Poincaré* **13** (1), 1–42.

(91) 1953 The Lorentz transformation and absolute time. In *Proc. Lorentz–Kamerlingh Onnes Memorial Conference, Leiden, 22–26 June 1953*. Amsterdam. (Reprinted in *Physica* **19**, 888–896.)

(92) Méthodes générales de la mécanique quantique relativiste. *Colloques int. C.N.R.S., Paris 'Particules fondamentales et noyau' 24–29 April 1950*, pp. 27–47.

(93) Die Stellung des Äthers in der Physik. *Naturw. Rdsch., Stuttg.* **6**, 441–446. (Based on Lindau lecture, 1953.)

(94) 1954 A new classical theory of electrons. III. *Proc. R. Soc. Lond.* A **223**, 438–445.

(95) Quantum mechanics and the aether. *Scient. Mon.* **78**, 142–146.

(96) 1955 The stress tensor in field dynamics. *Nuovo Cim.* (10) **1**, 16–36.

(97) Gauge-invariant formulation of quantum electrodynamics. *Can. J. Phys.* **33**, 650–660.

(98) Note on the use of non-orthogonal wave functions in perturbation calculations. *Can. J. Phys.* **33**, 709–712.

(99) Symmetry in the atomic world. *J. Scient. Ind. Res. Delhi* A **14**, 153–165.

(100) 1957 The vacuum in quantum electrodynamics. (Suppl.) *Nuovo Cim.* (1) **6**, 322–339.

(101) Elektroni i vakuum. *Vsesiuz. o-vo po rasprostraneniiu polit. i nauch. znanii*, series 8, no. 37, p. 15. (This paper is more readily available in a Czech translation, published in a regular scientific journal: Elektrony a Vakuum, *Pokroky matematiky, fyziky a astronomie* **4**, 309–317 (1959).)

(102) 1958 Generalized Hamilton dynamics. *Proc. R. Soc. Lond.* A **246**, 326–332.

(103) The theory of gravitation in Hamiltonian form. *Proc. R. Soc. Lond.* A **246**, 333–343.
(104) The electron wave equation in Riemannian space. In *Max-Planck-Festschrift* (ed. B. Kockel, W. Macke & A. Papapetrou), pp. 339–344. Berlin: Verlag der Wiss.
(105) 1959 Fixation of coordinates in the Hamiltonian theory of gravitation. *Phys. Rev.* **114**, 924–930.
(106) Energy of the gravitational field. *Phys. Rev. Lett.* **2**, 368–371.
(107) Sovremennoe sostoyanie relyativistskoi teorii electrona. *Trudy Inst. Istor. Estest. Tekh.* **22**, 32–33.
(108) 1960 Gravitationswellen. *Phys. Bl.* **16**, 364–366; also *Naturw. Rdsch., Stuttg.* **13**, 165–168. (Lindau lecture, 1959.)
(109) A reformulation of the Born–Infeld electrodynamics. *Proc. R. Soc. Lond.* A **257**, 32–43.
(110) 1961 Prof. Erwin Schrödinger, For.Mem.R.S. (obituary). *Nature, Lond.* **189**, 355–356.
(111) Reply to a letter of R. H. Dicke on 'Dirac's cosmology and Mach's principle'. *Nature, Lond.* **192**, 441.
(112) 1962 The energy of the gravitational field. In *Les théories relativistes de la gravitation* (*Coll. Int. du CNRS, Royaumont 1959*), pp. 385–393.
(113) Interacting gravitational and spinor fields. In *Recent developments in general relativity*, pp. 191–200. Warsaw: PWN–Polish Scientific Publications.
(114) An extensible model of the electron. *Proc. R. Soc. Lond.* A **268**, 57–67.
(115) Particles of finite size in the gravitation field. *Proc. R. Soc. Lond.* A **270**, 354–356.
(116) The conditions for a quantum field theory to be relativistic. *Rev. mod. Phys.* **34**, 592–596.
(117) 1963 The evolution of the physicist's picture of nature. *Scient. Am.* **208** (5), 45–53.
(118) A remarkable representation of the 3+2 de Sitter group. *J. math. Phys.* **4**, 901–909.
(119) 1964 Foundations of quantum mechanics. *Nature, Lond.* **203**, 115–116.
(120) Hamiltonian methods and quantum mechanics. *Proc. R. Irish Acad.* A **63**, 49–59.
(121) Equivalence of the Schrödinger and Heisenberg pictures. *Nature, Lond.* **204**, 771–772.
(122) *Lectures on Quantum Mechanics* (Belfer Grad. Sch. of Science, Yeshiva Univ., New York).
(123) 1965 Quantum electrodynamics without dead wood. *Phys. Rev.* B **139**, 684–690.
(124) 1966 *Lectures on Quantum Field Theory* (Belfer Grad. Sch. of Science, Yeshiva Univ., New York).
(125) 1967 The versatility of Niels Bohr. In *Niels Bohr, his life and work* (ed. S. Rozental), pp. 306–309. Amsterdam: North-Holland.
(126) 1968 The physical interpretation of quantum electrodynamics. *Commentarii, Pont. Acad. Sci.* **2** (13), 1–12.
(127) Methods in theoretical physics. In *From a life of physics, Evening lectures at the International Centre for Theoretical Physics, Trieste, Italy*. (Special suppl. of *IAEA Bulletin*, IAEA, Vienna, 1969.)
(128) 1969 The quantization of the gravitational field. In *Contemporary physics. Proc. Intl. Symp. Trieste, 7–28 June 1968* (ed. L. Fonda), vol. I, pp. 539–543. Vienna: IAEA.
(129) Can equations of motion be used? In *Proc. Coral Gables Conference on Fundamental interactions at high energies, Coral Gables, 22–24 Jan. 1969*, pp. 1–18. New York: Gordon and Breach.
(130) Hopes and fears. *Eureka*, no. 32, pp. 2–4.
(131) 1970 Can equations of motion be used in high energy physics? *Physics Today* **23** (4), 29–31.
(132) 1971 A positive-energy relativistic wave equation. *Proc. R. Soc. Lond.* A **322**, 435–445.
(133) *The development of quantum theory*. New York: Gordon and Breach.
(134) A positive-energy relativistic wave equation. In *Invited Papers, Tracts in mathematics and the natural sciences* (ed. M. Dal Cin, G. J. Iverson & A. Perlmutter), vol. 3, pp. 1–11. New York: Gordon & Breach.
(135) 1972 Evolutionary cosmology. *Commentarii, Pont. Acad. Sci.* **2** (46), 1–15.
(136) Discrete subgroups of the Poincaré group. In *Problemi teoreticheskoi fiziki* (ed. V. I. Ritus), pp. 45–51. Moscow: Publ. House Nauka.
(137) Zitterbewegung of the new positive-energy particle. In *Fundamental interactions in physics and astrophysics* (ed. G. Iverson, A. Perlmutter & S. Mintz), pp. 354–364. New York: Plenum.
(138) Relativity and quantum mechanics. *Fields and Quanta* **3**, 139–164.
(139) A positive-energy relativistic wave equation. II. *Proc. R. Soc. Lond.* A **328**, 1–7.

(140) The variability of the gravitational constant. In *Cosmology, fusion and other matters* (ed. F. Reines), pp. 56–59. Boulder, Colorado: Colorado Assoc. U. Press.

(141) 1973 Development of the physicist's conception of nature. In *The physicist's conception of nature* (ed. J. Mehra), pp. 1–14. Dordrecht: D. Reidel.

(142) Fundamental constants and their development in time. In *The physicist's conception of nature* (ed. J. Mehra), pp. 45–54. Dordrecht: D. Reidel.

(143) Long range forces and broken symmetries. *Proc. R. Soc. Lond.* A **333**, 403–418.

(144) The development of quantum theory. *Fiz. Szle.* **23**, 289–294.

(145) New ideas of space and time. *Naturwissenschaften* **60**, 529–531.

(146) Long range forces and broken symmetries. In *Fundamental interactions in physics* (ed. B. Kursunoglu & A. Perlmutter), pp. 1–17. New York: Plenum Press.

(147) Some of the early developments of quantum theory. In *Impact of basic research on technology* (ed. B. Kursunoglu & A. Perlmutter), pp. 1–13. New York: Plenum.

(148) 1974 Cosmological models and the large numbers hypothesis. *Proc. R. Soc. Lond.* A **338**, 439–446.

(149) *Spinors in Hilbert space.* New York: Plenum.

(150) An action principle for the motion of particles. *J. gen. Rel. Grav.* **5**, 741–748.

(151) Magnitnii monopol. In *Bolshaya Sovetskaya Entsiklopediya* (ed. A. M. Prokhorov), vol. 15, pp. 186–187 (columns 546–548). Moscow: Izdatelštvo 'Sovetskaya Entsiklopediya'.

(152) 1975 Variation of G. *Nature Lond.* **254**, 273.

(153) The large numbers hypothesis and its consequences. In *Theories and experiments in high-energy physics* (ed. A. Perlmutter & S. M. Widmayer), pp. 443–456. New York: Plenum.

(154) *General theory of relativity.* New York: Wiley-Interscience.

(155)* The scientific work of Georges Lemaitre. *Commentarii, Pont. Acad. Sci.* **2** (11), 18.

(156) James Chadwick. *Commentarii, Pont. Acad. Sci.* **3** (6), 1–5.

(157) Does the gravitational constant vary? *Commentarii, Pont. Acad. Sci.* **3** (7), 1–7.

(158) An historical perspective of spin. In *Proc. Summer Studies of High-Energy Physics with Polarized Beams, July 1974* (ed. J. B. Roberts). (Rep. ANL/HEP 75–02, ch. xxxii, pp. 1–11.) Argonne Nat. Lab.

(159) 1976 Theory of magnetic monopoles. In *New pathways in high-energy physics* (ed. A. Perlmutter), vol. I. pp. 1–14. New York: Plenum.

(160) Heisenberg's influence on physics. *Commentarii, Pont. Acad, Sci.* **3** (14), 1–15.

(161) 1977 The relativistic wave equation of electron. *Fiz. Szle* **27**, 443–450. (An abridged version, with title 'The relativistic electron wave equation', was published in *Europhysics News* **8** (10), 1–4.)

(162) Recollections of an exciting era. In *History of twentieth century physics* (ed. C. Weiner), pp. 109–146. New York: Academic.

(163) Ehrenhaft, the subelectron and the quark. In *History of twentieth century physics* (ed. C. Weiner), pp. 290–293. New York: Academic.

(164) The dynamics of streams of matter. In *Deeper pathway in high-energy physics* (ed. A. Perlmutter & L. F. Scott), pp. 1–11. New York: Plenum.

(165) Annahmen und Voreingenommenheit in der Physik. *Naturw. Rdsch., Stuttg.* **30** (12), 429–432. (Lindau lecture, 1976.)

(166) 1978 The prediction of antimatter. In *The 1st H. R. Crane Lecture, 17 April 1978*, 25 pages. Ann Arbor, Michigan: Univ. Michigan.

(167) Consequences of varying G. In *Current trends in the theory of fields* (AIP Conf. Proceedings, no. 48) (ed. J. E. Lannutti & P. K. Williams), pp. 169–174. New York: Am. Inst. Phys.

(168) The large numbers hypothesis and the cosmological variation of the gravitational constant. In *On the measurement of cosmological variations of the gravitational constant* (ed. L. Halpern), pp. 3–17. Gainesville, Florida: Univ. Press Florida.

(169) New ideas about gravitation and cosmology. *Commentarii, Pont. Acad. Sci.* **3** (24), 1–10.

(170) *Directions in physics* (ed. H. Hora & J. R. Shepanski). Lectures delivered during a visit to Australia and New Zealand August/September 1975. New York: Wiley-Interscience.

* Note added in proof: this reference is misplaced and should appear under the year 1968.

(171) The monopole concept. *Int. J. theor. Phys.* **17**, 235–247.
(172) Fundamental physical constants and their development in time. *Fiz. Szle* **28**, 201–204.
(173) The mathematical foundations of quantum theory. In *Mathematical foundations of quantum theory* (ed. A. R. Marlow), pp. 1–8. New York: Academic.
(174) A new approach to cosmology theory. In *New frontiers in high energy physics* (ed. B. Kursunoglu, A. Perlmutter, L. F. Scott, O. Kadiroglu, J. Nowakowski & F. Krausz), pp. 1–16. New York: Plenum.
(175) 1979 The large numbers hypothesis and the Einstein theory of gravitation. *Proc. R. Soc. Lond.* A **365**, 19–30.
(176) Developments of Einstein's theory of relativity. In *On the path of Albert Einstein* (ed. A. Perlmutter & L. F. Scott), pp. 1–13. New York: Plenum.
(177) The relativistic electron wave equation. *Soviet Phys. Usp.* **22**, 648–653.
(178) The excellence of Einstein's theory of gravitation. *Impact of Science in Society* **29**, 11–14.
(179) The test of time. *The UNESCO Courier*, May, pp. 17–23. (Also published in German as Der Zeittest. *Naturw. Rdsch., Stuttg.* **33**, 353–356 (1980).)
(180) Einstein's influence on physics. In *Einstein Galileo. Commemoration of Albert Einstein*, pp. 19–23. Libreria Editrice Vaticana, Citta del Vaticano: Pont. Acad. Sci.
(181) 1980 The variation of G and the problem of the Moon. In *Recent developments in high energy physics* (ed. A. Perlmutter & L. F. Scott), pp. 1–7. New York: Plenum.
(182) Why we believe in the Einstein theory. In *Symmetries in science* (ed. B. Gruber & R. S. Millman), pp. 1–11. New York: Plenum.
(183) Dirac recalls Kapitza. *Physics Today* **33** (5), 15.
(184) 1981 Does renormalization make sense? In *Conf. on Perturbative Quantum Chromodynamics, AIP Conf. Proc.* **74**, 129–130.
(185) Models of the Universe. In *Gauge theories, massive neutrinos and proton decay* (ed. A. Perlmutter), pp. 1–9. New York: Plenum.
(186) 1982 Pretty mathematics. *Int. J. theor. Phys.* **21**, 603–605.
(187) The variation of G and the quantum theory. In *Proc. Second Marcel Grossmann Meeting on General Relativity* (ed. R. Ruffini), vol. A, pp. 1–6. Amsterdam: North-Holland.
(188) The early years of relativity. In *Albert Einstein historical and cultural perspectives: the centennial symposium in Jerusalem* (ed. G. Holton & Y. Elkana), pp. 79–90. Princeton University Press.
(189) 1983 The present state of gravitational theory. In *Field theory in elementary particles* (ed. A. Perlmutter), pp. 1–10. New York: Plenum.
(190) The origin of quantum field theory. In *The birth of particle physics* (ed. L. M. Brown & L. Hoddeson), pp. 39–55. Cambridge University Press.
(191) 1984 The requirements of fundamental physical theory. *Eur. J. Phys.* **5**, 65–67. (Lindau lecture 1982.)
(192) The future of atomic physics. *Int. J. theor. Phys.* **23**, 677–681.
(193) Blackett and the positron. In *Cambridge physics in the thirties* (ed. J. Hendry), pp. 61–62. Bristol: Hilger.
(194) 1986 The inadequacies of quantum field theory. In *The Dirac memorial volume* (ed. B. Kursunoglu & E. P. Wigner). Cambridge University Press. (In the press.)
(195) — The arrival of quantum mechanics (n.d. or p.), pp. 185–190.
(196) — *Elementary particles and their interactions* (held by the Pauli Library at CERN) (n.d. or p.). 8 pages.
(197) 1974 The development of quantum mechanics. In *Contributi del Centro Linceo Interdisciplinare di scienze mathematiche e loro applicazioni*, no. 4, 11 pages. Rome: Accademia Nazionale dei Lincei.
(198) 1980 A little 'prehistory'. *The Old Cothamian 1980*, p. 9.

Photocopied lecture notes

(L1) 1931 Lectures on quantum mechanics. Reported by B. Hoffmann. Princeton. Mimeographed. (Library, Mathematical Institute, Oxford, and elsewhere.)
(L2) 1935 Lectures on quantum electrodynamics (1934–35). Notes by Dr Boris Podolsky (1st semester) and Dr Nathan Rosen (2nd semester). Institute for Advanced

Study, Princeton N.J. Mathematical publications 1934–35, vol. 2. Mimeographed. (Mathematics Department, Columbia University.)

(L3) 1946 Lectures on quantum electrodynamics. Notes by S. Kusaka. Palmer Physical Lab., Princeton University, Fall term. Mimeographed. (Library, Brookhaven National Laboratory.)

(L4) 1949 The relation of classical to quantum mechanics. 7 September. (Physics Department Library, Columbia University.)

(L5) 1955 Lectures on quantum mechanics and relativistic field theory. Notes by K. K. Gupta and G. Sudarshan. Tata Inst. Fund. Research: *Lectures on Mathematics and Physics* 1. Mimeographed.

(L6) 1969 The basic ideas of quantum mechanics. A course of lectures. January–March 1969. Notes taken by G. D. Kaiser and R. M. Williams (reproduced from typescript). Coral Gables, Florida. (Cambridge University Library.)

(L7) 1971 The evolution of our understanding of nature. (Univ. Miami, Center for Theoretical Studies.) Coral Gables, Florida. (Cambridge University Library.)

Dirac's Lindau discourses

From their inception Dirac regularly attended, and contributed to, the Nobel Prize winners' meetings at Lindau (Bodensee). His discourses were recorded and tapes of them can be obtained from the Ständiger Arbeitsausschuss für die Tagungen der Nobelpreisträger in Lindau. In general the Lindau discourses are not published, although abbreviated versions of some of Dirac's discourses have appeared in print, as we have noted in the bibliography above (93, 108, 118, 145, 165). His last discourse (191) has been published in full.

1953 *Quantenmechanik und der Äther.*
1956 *Electrons and the vacuum.*
1959 *Gravitational waves.*
1965 *The foundations of quantum mechanics.*
1968 *How far will quantum mechanics go?*
1971 *Fundamental problems of physics.*
1973 *New ideas of space and time.*
1976 *Basic beliefs and prejudices in physics.*
1979 *Does the gravitational constant vary?*
1982 *The requirements of fundamental physical theory.*

References to other authors

Casimir, H. B. G. 1985 Paul Dirac 1902–1984. *Naturw. Rdsch., Stuttg.* **38**, 219–223.

Cohen, K. P. 1951 *The theory of isotope separation as applied to the large-scale production of* U^{235}. New York: McGraw Hill.

Craigie, N. S., Goddard, P. & Nahm, W. 1982 *Monopoles in quantum field theory*, p. vii. Singapore: World Scientific.

Dalitz, R. H. 1986 Another side to Paul Dirac. In *The Dirac memorial volume* (ed. B. Kursunoglu & E. P. Wigner). (In the press.)

Dauzat, A. & Rostaing, C. 1963 *Dictionnaire étymologique des noms de lieux en France*. Paris: Larousse.

Eliezer, C. J. 1987 Some reminiscences of Professor P. A. M. Dirac. In *Tributes to Paul Dirac* (ed. J. G. Taylor). (In the press.)

Griffin, J. L. 1979 Private communication to the Hon. William Waldegrave, M.P.

Harish-Chandra 1986 My association with Professor Dirac. In *The Dirac memorial volume* (ed. B. Kursunoglu & E. P. Wigner). (In the press.)

Heisenberg, W. 1971 *Physics and beyond*, pp. 85–87. New York: Harper & Row.

Kuhn, T. S. 1962 Interview with P. A. M. Dirac, 1 April 1962. Tape 7*a*, Niels Bohr Library, American Institute of Physics, New York.

Kuhn, T. S. 1963 Interview with P. A. M. Dirac, 6 May 1963. Tape 62*b*, Niels Bohr Library, American Institute of Physics, New York.

Kursunoglu, B. & Wigner, E. P. (editors) *The Dirac memorial volume*. Cambridge University Press. (In the press.)

Longnon, A. 1920–29 *Les noms de lieu de la France*. Paris: H. Champion.
Mehra, J. 1972 The golden age of theoretical physics: P. A. M. Dirac's scientific work from 1924 to 1933. In *Aspects of quantum theory* (ed. A. Salam & E. P. Wigner), pp. 17–60.
Mehra, J. 1973 *The physicist's conception of nature*. Dordrecht, Holland: D. Reidel.
Mehra, J. 1975 *The Solvay conferences on physics, aspects of the development of physics since 1911*. Dordrecht: D. Reidel.
Mehra, J. & Rechenberg, H. 1982 *The historical development of quantum theory*, vol. 4. Heidelberg: Springer-Verlag.
Mott, N. F. 1986 Reminiscences of Paul Dirac. In *The Dirac memorial volume* (ed. B. Kursunoglu & E. P. Wigner). (In the press.)
Niblett, W. R. 1985 Private communication (via R. J. Tucker, 1985).
Phillips, L. R. 1947 Private communication (via R. J. Tucker, 1986).
Phillips, R. J. N. 1987 Some words from a former student. In *Tributes to Paul Dirac* (ed. J. G. Taylor). (In the press.)
Salam, A. & Wigner, E. P. 1972 *Aspects of quantum theory*. Cambridge University Press.
Salaman, E. & Salaman M. 1986 Remembering Paul Dirac. *Encounter*, May issue, 66–70.
Schweber, S. S. 1984 Some chapters for a history of quantum field theory: 1938–1952. In *Relativity, groups and topology II, Les Houches, Session XL, 1983* (ed. B. S. DeWitt & R. Stora). Amsterdam: North-Holland.
Shanmugadhasan, S. 1987 Dirac as research supervisor and other remembrances. In *Tributes to Paul Dirac* (ed. J. G. Taylor). (In the press.)
Taylor, J. G. (editor) 1987 *Tributes to Paul Dirac*. Bristol: Hilger. (In the press.)
Whitley, S. 1984 Review of the gas centrifuge until 1962. I. Principles of separation physics. *Rev. mod. Phys.* **56**, 41–66.
Willis, D. C. 1985 Private communication (via R. J. Tucker, 1985).

Dirac at the height of his career. Photograph from about 1930. Reproduced with permission of AIP Niels Bohr Library.

explained as a manifestation of
to the principle of unity decei
model was "rather forced upo
those general considerations, t
mathematical structure of the
the hole was the same as the ma
made by Heisenberg in his lette
at that time Dirac was too fascir
of the point.

Having concluded that the pr
hole, Dirac was faced with sev
expect that a positive-energy el
tum transition to fill a hole, un
would annihilate, that is

$$p^+$$

Dirac considered this hypotheti
1930, and there he also showed
agreement with the Klein–Nish
for the Advancement of Science

> There appears to be no reason wh
> somewhere in the world. They wou
> Nature, in particular with the law
> would have to occur only very seld
> never been observed in the laborato

In 1930, the idea of annihilatio
with astrophysical speculations
electron annihilation had been
World War I. Eddington and Je
cated the view that such proce
astrophysics, although they adm
was unknown. Annihilation and

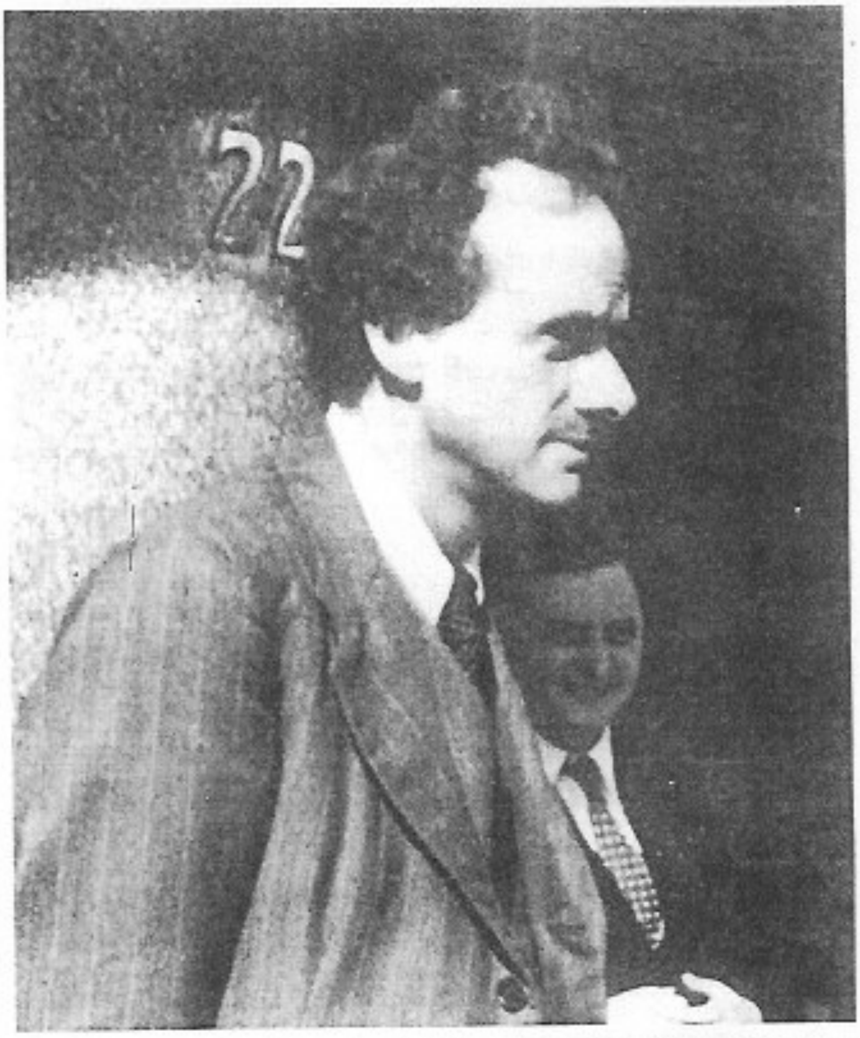
nal notation) was called a bra-
is way Dirac was able to write
$\langle b|a \rangle$. He further showed that
or bra, yielding states symbol-
he proved a number of theo-
Dirac's new notation was not
st time in 1943), but eventually
mmon, powerful, and much

it still another notational nov-
in important study of the uni-
his work he coined the word
es that appeared as coefficients
ne mathematical properties of
y might be useful in physics.
d that particles having no spin
moving. Although this work
ficance, it was of considerable
e-dimensional irreducible rep-
by Harish-Chandra, Valentin
ists.

tinued to uphold his idea of a
ion that the Schrödinger wave
ressed as a power series in the
as written as

$$e^3\psi_3 + \cdots$$

to Dirac, this was a most seri-
ted and that demanded some
ism. He considered, of course,
solutions that were not in the
this idea with the following



to the 60s, was described by H. Leutwyler. Among many solid results in this sector, the combination of chiral perturbation and lattice techniques looks a promising way to get low energy hadron physics parameters.

The transition from QCD confinement to deconfinement is still more accessible on the lattice than in laboratory heavy ions collisions. F. Karsch showed exciting hints from lattice simulations for a complicated non-perturbative structure of the quark-gluon plasma near the critical temperature.

Progress in heavy ion collisions was reviewed by H. Satz. Good probes for monitoring the QCD phase transition have now been devised by theorists, and the 'smoking gun' may be provided by the production of the J/psi and related particles.

The future will bring new challenges for QCD. G. Wolf summarized the status of HERA, where a new chapter in deep inelastic scattering is about to begin, with the nucleon structure being explored down to $2x10^{-16}$ cm.

The long awaited sixth (top) quark, where indirect evidence points to a mass around 130 GeV, should soon show up at the Fermilab Tevatron. Startling new QCD effects are predicted for top physics (J. Kühn) due to the interplay between electromagnetic and strong interactions at these energies. Weak decays of hadrons containing heavy quarks (C. Sachrajda and A. Buras) is another frontier.

John Ellis looked at how QCD could fit eventually into a larger Theory of Everything, and Harald Fritzsch summarized. While QCD has some remarkable achievements to its credit after 20 years, there are still challenging problems to be solved in the next 20 years.

By O. Nachtmann

Looking at the antiworld

A popular pastime among amateur scientific historians is tracing key concepts in twentieth century physics back to their origins. Participants at the Antihydrogen Workshop in Munich on July 30-31 were astonished to hear 1989 Nobel prizewinner Wolfgang Paul mention in his introductory remarks that W. Nernst referred to antimatter as far back as 1897.

Nernst thus 'beat' Dirac by some 30 years, (breaking by a matter of months the previous record, held by A. Schuster's 1898 letter to *Nature*). These historical footnotes enhance Dirac's achievement in demonstrating the existence of antimatter as the price paid for combining quantum mechanics and special relativity.

As every physicist knows, Dirac turned the embarrassing 'redundant' solutions of his relativistic wave equation for electrons to good effect, hypothesising that they corresponded to '...a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to the electron...'. The positron obligingly appeared in 1932, but the discovery of the antiproton had to wait a further quarter of a century, and it was only at that time that the relationship of matter and antimatter was finally seen in terms of the CPT (charge conjugation/parity/time reversal) theorem.

In 1981, CERN began to mass-produce antiprotons, stacking millions of millions of them at a time for physics experiments. A few years later Fermilab too built an antiproton factory.

In spite of all this work with antiparticles, not one atom of antihydrogen has yet been synthesized for study in the laboratory. The enormous strides now being made in cooling, trapping, storing and manipulating charged and neutral particles, as well as in ultra high precision laser spectroscopy should soon change this.

The aims of the workshop were to review progress in these areas, assess the potential of antihydrogen as a test bench for answering fundamental questions of physics, and guide current deliberations on the future of the antiproton programme at CERN.

The workshop attracted some 100 participants from all over the world. The unifying nature of anihydrogen studies was evident in the diversity of their research backgrounds, which ranged from atomic to nuclear and particle physics, from laser spectroscopy to permanent magnet design, and from accelerator physics to cosmology. There were some eleven hours of oral presentations and discussions, as well as 23 posters.

Wolfgang Paul's introduction was followed by R.J. Hughes (Los Alamos) who reviewed the physics potential of high precision atomic spectroscopy of antihydrogen. Seen simply as a probe of CPT, such measurements are capable of reaching with atomic matter the four parts in10^{18} precision given by the neutral kaon system.