I

Dirac in Cambridge

R. J. Eden and J. C. Polkinghorne

Paul Dirac held the Lucasian Chair of Mathematics at Cambridge from his appointment in 1932 at the age of 30 until his retirement in 1969. This professorship is the oldest in the Faculty of Mathematics – a faculty which includes many people working in theoretical physics as well as people working in pure mathematics – and among his predecessors was Isaac Newton.

He came to Cambridge first in 1923 as a graduate student after studying electrical engineering at Bristol, and became a Fellow of St John's College in 1927. His reputation was soon made by his paper in 1925 on the fundamental laws of quantum mechanics which gave the world a deep insight into the meaning and implications of Heisenberg's work on matrix mechanics. Heisenberg recalled later how he had visited Cambridge in 1925 to lecture on his new matrix mechanics and how much he was impressed by the penetrating questions asked by a young research student. Heisenberg added that he was even more impressed two months later when the same student sent him a thirty-page manuscript setting out the fundamental laws of quantum mechanics![†] On another occasion Heisenberg remarked that at first he had looked on the lack of commutation of coordinates and momenta as a defect in the theory until he saw from Dirac's presentation that it was one of the central features of quantum mechanics. It is characteristic of Dirac that he once introduced a lecture by Heisenberg by expressing great admiration for the speaker which was due, he said, to the fact that they had both worked on the same problem (of inventing the new quantum

† Dirac's recollection of this historic occasion is that Heisenberg's Cambridge lecture was mostly on the Zeeman effect, in which he (Dirac) was not much interested, and he did not notice the part on matrix mechanics which probably came at the end. Dirac did pay attention however a few weeks later when he received via R. H. Fowler a proof-copy of Heisenberg's paper. It was then that he realized the significance of this work and its relation to his own approach which very quickly lead to his paper on the fundamental laws of quantum mechanics.

theory) and Heisenberg had succeeded in being the first to solve it, whereas he had failed!

In Cambridge in the 1920s research students worked in their own colleges, in comparative isolation. There were no regular theoretical seminars and it was unusual to know students in other colleges even if they were working in the same field. Despite his also being a Johnian, Mott recalls how he spent most of the year 1927 reading Dirac's papers on radiation theory but does not believe that they had any discussions on physics at that time. It was apparently not the fashion in those days to ask someone to help one to do theoretical physics research.

Concerning those early days Lady Jeffreys writes:

> My memories of that time are very clear as I became a research student under the supervision of R. H. Fowler in 1925. At that time Fowler was the central figure in Theoretical Physics in Cambridge. Quantum Mechanics was just getting under way. My notebook for 1926 contains:
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> |---|---|---|
> | Lent Term | Mr. Fowler | Quantum Theory of Spectra. |
> | Easter Term | Mr. Fowler | Quantum Theory. |
> | Easter Term | Mr. Dirac | Quantum *Mechanics*. |
>
> These lectures were all attended by the same small bunch of people. At this distance of time it is difficult to be sure but I think that it included D. R. Hartree, J. M. Whittaker, A. H. Wilson, J. A. Gaunt, N. F. Mott and J. R. Oppenheimer. The Easter term lectures were complementary to each other. Fowler had a remarkable gift for rapidly digesting new work of others and imparting his own enthusiasm for it. Dirac gave us what he himself had recently done, some of it already published, some, I think, not. We did not, it is true, form a very sociable group, but for anyone who was there it is impossible to forget the sense of excitement at the new work. I stood in some awe of Dirac, but if I did pluck up courage to ask him a question I always got a direct and helpful answer, with no beating about the bush if I was getting things wrong.

Whatever their deficiencies at that time as centres of research, the colleges at Cambridge have always succeeded in providing within the large university smaller communities in which social life can flourish. The bachelor tables of the 1920s were more hearty and bibulous than they are today and it is said that Dirac once showed his disapproval of that style of life by filling all the glasses at dinner with water which consequently had to be consumed before beer could be ordered.

He has always been a great walker and used to include some mountain climbing with his walking. Tamm visited Cambridge from the Soviet Union about 1931 and invited Dirac to go climbing with him on the Soviet–Chinese border. Mott recalls how Dirac and Tamm trained for this by climbing trees on the Gog-Magog hills near Cambridge, Dirac always wearing a formal dark suit. By this time Dirac had already invented the relativistic equation for the electron, which appears at

first sight to indicate that a free electron has only two velocities namely $\pm c$, an appearance which Dirac explained at a deeper level which leads to the average velocity that arises in an actual observation. It is said that like his electron, Dirac's car in the early 1930s had only two velocities, one being full speed, the other being zero!

The extension into everyday life by Dirac of his logical approach to the problems of theoretical physics has yielded many stories. One of the earliest of these is recounted by Hulme who was Dirac's first research student. They used to go walking together in the Cambridgeshire countryside, apparently with little conversation and especially little on physics. Hulme remarks that they both used walking sticks and on one occasion he jokingly asked whether Dirac had ever tried walking with two sticks for symmetry. To his surprise, Dirac replied that he had tried once and it didn't work. He also remembers an occasion when he encountered Dirac going to London by train to the motor show. They met again on the return train to Cambridge, and Hulme was sucking some throat lozenges from a glass bottle that rattled a little in his pocket. Dirac asked if he had developed a sore throat during the day as he had not heard the lozenges rattling on the way to London. Hulme explained that the bottle had been full then so that they had not rattled, to which Dirac replied that he supposed when the bottle was half empty they would rattle most. Peierls has remarked how interesting it would have been if the story about the lozenges had preceded the hole theory of positrons; alas it is not so, the lozenges story is dated 1934 ± 2 so it was presumably derived *from* the hole theory (1931).

The penetrating directness of Dirac's papers and the relatively small number of references has sometimes been thought to suggest that he did not have wide interests in physics. Peierls remarks that during his time in Cambridge following 1933, Dirac appeared to him to be a person who could become interested in almost anything and Peierls illustrates this point by the following account of a little-known aspect of Dirac's research:

In about 1934 Dirac invented a method of isotope separation. The idea was to make a jet of gas turn a corner, past a sharp edge, so that the centrifugal force would cause separation of the components. He not only conceived the idea, but decided to verify it experimentally. Kapitza allowed him the use of a compressor in the Mond Laboratory, and the device was tried initially on a mixture, not of isotopes, but of air with a heavy organic compound. When I saw the experiment there had been as yet no evidence of a difference in composition between the two output tubes, but by feeling the tubes one could easily check that one was hot and the other cold, showing that something non-trivial was happening in the junction. When Kapitza had to stay in Moscow, and his equipment was sent on,

Dirac's experiment was interrupted. During the war, however, a group in Oxford studied the feasibility of the method for separating uranium hexafluoride. They found it worked perfectly well, but less efficiently than gaseous diffusion, and it was therefore not pursued. When this work was started, Dirac was invited to Oxford to discuss the method. I was present at the meeting in the Clarendon Laboratory (there may have been more than one meeting) and I remember that the experimentalists expected a highbrow and abstract mathematician who would know the kinetic theory of the effect, but would not know one end of an apparatus from another. They were most impressed by Dirac's eminently practical and helpful remarks.

Peierls continues,

Dirac's experiment also provided the occasion for one of my favourite episodes, though I do not know whether this is more a story about Dirac or about Wigner. Wigner and I happened to visit Cambridge on the same day, and we both called, separately, in the Mond Laboratory and saw Dirac with his apparatus. Later Wigner complained that Dirac was so secretive about his idea; he had refused to explain it. I was surprised by this, since my impression was different. I had actually guessed what the principle might be, but Dirac had shown no hesitation in confirming that my guess was right. A little more probing brought out that the relevant conversation consisted on one exchange. Wigner had said 'It must have been very difficult to make the little brass piece?' (A brass T piece, with the gas mixture entering at the stem and the fractions coming out of the arms, was clearly the heart of the device). Dirac's answer was 'No, that was fairly easy'. He had given a straight answer to a direct question. Wigner on the other hand, had asked for information and thought he had been refused. I bet Wigner that, by asking directly for the principle, he would have got the explanation. The bet could not be settled, because when we asked Dirac whether he would have responded to a direct question he said, of course, 'I do not know' – his frequent answer to a hypothetical question.

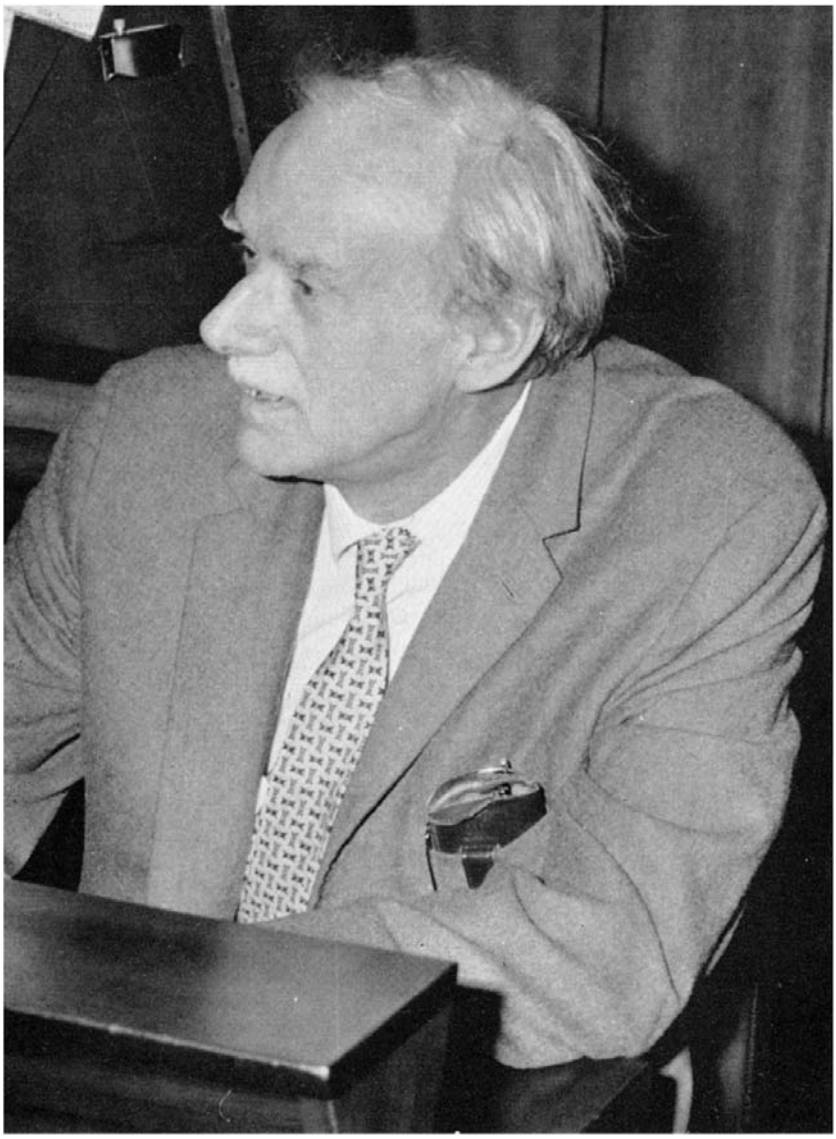
During the early and mid-1930s Dirac was 'adopted' first by the Mott family and then by the Peierls family during their successive periods of residence in Cambridge. In 1937 Dirac married Margit Wigner (sister of Eugene Wigner) and from that date the social life of a chosen few was enhanced by being 'adopted' by the Dirac family. It would be difficult to attempt to complete the list of those by whom the friendship of the Dirac family was much appreciated. Kemmer recalls, how the problems of secrecy in wartime work inhibited his scientific discussions with Dirac but he describes the friendly social contacts with the Dirac family during this time. Dirac was interested in gardening and Kemmer (like others after him) was often greeted by Mrs Dirac with 'Oh you want to see Paul? He is up a tree in the garden.'

At any given time, Dirac would not normally have more than one or two research students under his supervision. His reason was not at all related to the trouble involved, but was because his own interests were in fundamental problems and he did not think that these were suitable

for many Ph.D. students. One of us (RJE) had the privilege of being in this small band and can testify that as a supervisor Dirac was most helpful, readily available to listen to any reports on progress. A seminar by one of his students would tend to take on the character of a public supervision (or inquisition) since he asked so many questions. He could be a formidable member of the audience if he chose. On one occasion the lecturer (a post-doctoral fellow) began by listing his five basic assumptions; when he had finished writing the fifth on the blackboard Dirac said 'Your assumption number two contradicts your assumption number five.' The lecturer paused for thought and then agreed. It says much for the resourcefulness of this particular speaker that he went on to say 'Since I cannot talk on that subject, I will give a seminar on a different topic', which he then proceeded to do.

Dirac's greatest influence on students in Cambridge generally was through his course of lectures on quantum theory. For many years it was the first course in quantum theory that Cambridge students could attend and many of us have had the privilege of learning our quantum mechanics 'straight from the horse's mouth', as the saying goes. Not all the audience were novices, however, for frequently visitors of some standing would rightly judge it something not to be missed while they were in Cambridge. The material and its treatment can readily be gauged from reading his celebrated book *The Principles of Quantum Mechanics*, which the course closely followed. However, there was more to the lectures than the printed page can convey. The delivery was always exceptionally clear and one was carried along in the unfolding of an argument which seemed as majestic and inevitable as the development of a Bach fugue. Gestures were kept to a minimum, though there was a celebrated passage near the beginning where he broke a piece of chalk in half and moving one of the bits about the lecture desk said that in quantum theory we must consider states which are a linear superposition of all these different possible locations. There was absolutely no attempt to underline what had been his own contributions, though at times one felt one got a hint of his feelings about what he had done. If there was substance in these perceptions they seemed to reveal that the invention of bra and ket vectors (and the naming of them) had given him as much pleasure as anything.

Dirac's tenure of the Lucasian Chair of Mathematics has given many generations of Cambridge men the honour and pleasure of knowing him as teacher, colleague and friend and they would wish to send him their warmest good wishes on his seventieth birthday.



Dirac at a recent Solvay Conference

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Travels with Dirac in the Rockies

J. H. Van Vleck

It was in the spring of 1929 that Dirac made his first visit to the United States, in response to an invitation from the University of Wisconsin to spend a term as visiting professor there. I have always been proud of the fact that my *alma mater* was the first institution to bring him to America and that I participated in its decision to do so. Before proceeding to my reminiscences of his early visits to my country, I will digress to tell how even before then my research was influenced by his publications.

INFLUENCE OF DIRAC'S PAPERS ON MY EARLY WORK

During the first two years or so of quantum mechanics I was still at the University of Minnesota, and my 'bibles' for learning the strange new mechanics in early 1926 were the long papers by Born, Heisenberg and Jordan in the *Zeitschrift für Physik*,[1] and Dirac's articles on 'The fundamental equations of quantum mechanics', 'Quantum mechanics and a preliminary investigation of the hydrogen atom', and 'The elimination of the nodes in quantum mechanics', all in the *Proceedings of the Royal Society*.[2] In another 'reminiscing' paper[3] I have described how Dirac's work enabled me to compute the mean values of $1/r^2$ and $1/r^3$ for a hydrogen atom (needed to calculate spin-relativistic fine structure) and $1/r^4$ (needed to calculate quantum defects due to polarization in nearly hydrogenic atoms) and how when I reached Copenhagen in 1926 with manuscripts all written, I found that these mean values had just been published by Heisenberg and Jordan for $1/r^2$, $1/r^3$ and were in course of publication for $1/r^4$ by Waller.

Hill and I[4] found formulas in Dirac's early publications which I have cited also useful in connection with calculating matrix elements for fine structures involving spin coupling in molecular spectra, a field that would at first sight seem rather unrelated. I remember remarking to Hill 'You can find almost anything in Dirac's papers if you read them long enough.'

When Dirac's paper[5] on 'The quantum theory of the electron' reached me in Minneapolis in early 1928 it properly seemed to me sensational. The long article[6] on 'Magnetic susceptibilities in the new quantum mechanics' which I published in the *Physical Review* has for its second paragraph the following:

Note added in proof. A remarkable paper by Dirac (*Proc. Roy. Soc.*, Feb. 1928) has just appeared in which he shows that the requirement that the Schroedinger wave equation have the invariance demanded by relativity is adequate to give the terms ordinarily ascribed to internal spins of the electron. Thus our treatment of the electron as a spherical top to derive the Hamiltonian function inclusive of spin terms in a magnetic field suddenly loses much of its interest. However, it must at the same time be emphasized that all the essential results of the present paper are unaltered; the only difference is that our work prior to about p. 594 becomes rather antiquated, as Dirac's postulates give a Hamiltonian function such as (5) directly and elegantly. From there on everything goes as before.

In the 'antiquated pages prior to p. 594' I tried to describe the Uhlenbeck–Goudsmit spinning electron by means of a spherical top with Eulerian angles more or less as had Darwin[7] in an earlier paper written in 1927. In these pages there was a lot of nonsense relating to the electrical radius of the electron, in an attempt to show that there was no appreciable diamagnetic term due to spin and to make spin look as physical as possible in terms of classical concepts. My introductory model with Eulerian angles not merely wasted space in the *Physical Review*, but also appears to have diverted readers from the essential quantum-mechanical part of the calculation. At any rate, Sommerfeld[8] in his report to the 1930 Solvay congress appears to have overlooked the implications of my remarks added in proof, and gave the impression that my calculations, though giving the right answer, needed to be redone because of my use of Eulerian angles.

In a footnote of this same paper[6] I commented 'We use the matrix rather than wave formulation of the new quantum mechanics. The same results are, of course, obtained with either formulations in virtue of their general mathematical identity, and the popularity of susceptibility calculations by means of the wave equation seems rather surprising inasmuch as the matrix method has usually yielded the susceptibility formulas first and most directly.' As of today, this statement may appear a truism, but in the early days of quantum mechanics, things were so new that it was not easy for theoretical physicists to become thoroughly indoctrinated with both the matrix and differential equation approaches, even though they are both different aspects of general transformation theory. Unlike most theorists of the time, I belonged to a minority

group preferring the former, but many papers of that era (e.g. part of Sommerfeld's report[8]) were written which simply redid with expansion of wave functions calculations previously made with matrix algebra. For instance, a 1928 paper of Darwin's[9] began as follows:

> In a recent paper Dirac has brilliantly removed the defects before existing in the mechanics of the electron, and has shown how the phenomena usually called the 'spinning electron' fit into place in the complete theory. He applies to the problem the method of q-numbers and, using non-commutative algebra, exhibits the properties of a free electron, and of an electron in a central field of electric force. In a second paper he also discusses the rules of combination and the Zeeman effect. There are probably readers who will share the present writer's feeling that the methods of non-commutative algebra are harder to follow, and certainly much more difficult to invent, than are operations of types long familiar to analysis. Wherever it is possible to do so, it is surely better to present the theory in a mathematical form that dates from the time of Laplace and Legendre, if only because the details of the calculus have been so much more thoroughly explored.

However, Darwin's paper[9] was more than a mere transcription from matrix to wave-mechanical language; it contained the first proof that to all powers of $1/c^2$, Dirac's equations gave the same formula for the energy levels of hydrogen as that obtained by Sommerfeld in the old quantum theory with relativity but without spin. This is perhaps the most remarkable numerical coincidence in the history of physics; the physical interpretation and assignment of quantum numbers is, of course, completely different.

DIRAC IN MADISON, WISCONSIN 1929

When Dirac had been in Madison a few days I asked him what had surprised him most about the United States and he told me it was that there were so many wooden houses. I wonder whether this was responsible for his later wanting to travel extensively in the U.S.S.R., where there are also many wooden houses and where he once took the Trans-Siberian railway clear across that country.

Dirac gave a well-organized course of lectures, almost a formal course, on mainly the transformation theory of quantum mechanics which he had evolved about a year and a half earlier. It was far more abstract than the usual American courses in physics of that era. I remember my father, a Professor of Mathematics at the University of Wisconsin then in his final year before retirement, attending some of Dirac's lectures and remarking that their abstractness reminded him of the 'general analysis' which was developed by Hastings Moore, a

mathematician at the University of Chicago and which was a very abstract theory of linear spaces.

Dirac either lectured on, or told me about his now celebrated vector model[10] for handling permutation degeneracy. In particular he indicated how it could be used to obtain Heisenberg's formulas for ferromagnetism, something not mentioned in his publications. I was greatly influenced by Dirac's procedure, and subsequently capitalized on it heavily, applying it not only to magnetism,[11] but also to complex spectra[12] and chemical bonding.[13] In consequence references are sometimes improperly made in the literature to the 'Dirac–Van Vleck vector model'. All I did was apply it. Dirac had the original and essential idea.

After Dirac had been in Madison only a short time, it became apparent that he was fond of walking, especially amid nice scenery. We used to take walks in the fields overlooking Lake Mendota, and elsewhere. Another of his attributes was a very fancy watch, which told the day of the week and month, and perhaps even the phase of moon (I can't remember). Its fame had spread to the University of Iowa, in advance of his giving a lecture there. One student or faculty member at Iowa City, hoping to see Dirac's watch, said 'Dr Dirac could you please tell me the time,' whereupon Dirac looked at the clock on one of the University buildings and gave him the time. One could claim that this reply was an illustration of Dirac's facility in answering a question in the most direct possible fashion. Professor Eldridge of the Iowa physics department, hearing of his colleague's failure, used the method of direct approach, and simply said to Dirac 'Show me that watch.'

One of the mysteries connected with Dirac's visit, almost as great as how he discovered his four component equations, was what his initials 'P.A.M.' stood for. He had signed his papers and correspondence only in this terse fashion. He was reluctant to take away the mystery and let people know what they stood for. Finally, perhaps near the end of Dirac's visit, there was a dinner in his honor at the University Club, for which Professor Ingersoll had a brilliant idea. Each place card contained not merely the name of who was to sit there, but also the statement that the dinner was in honor of . . . Dirac, and on each card was inserted a different guess as to what the 'P.A.M.' stood for. For instance one place card might read 'Professor Mendenhall, dinner in honor of Peter Alfred Martin Dirac.' After studying all the cards Dirac said that by proper combinations of names entered on certain of the