BIOGRAPHICAL
MEMOIRS

Nicholas Kemmer. 7 December 1911 –– 21 October 1998

Freeman Dyson

Biogr. Mems Fell. R. Soc. 2011 **57**, 189-204, published 17 August 2011
originally published online August 17, 2011

NICHOLAS KEMMER

7 December 1911 — 21 October 1998

N. Kemmer

NICHOLAS KEMMER

7 December 1911 — 21 October 1998

Elected FRS 1956

By Freeman Dyson FRS

Institute for Advanced Study, Princeton, New Jersey, USA

Nicholas Kemmer was a theoretical physicist whose most famous contribution to science was the prediction in 1938 of the existence of three kinds of particle—one positive, one negative and one neutral—coupled to protons and neutrons in a symmetrical way so as to produce nuclear forces independent of charge. Three particle species were discovered experimentally 10 years later and found to have the nuclear couplings specified by Kemmer. They are the particles now known as π mesons or pions. Other sets of three species with the same symmetry were discovered later. The long interval between prediction and verification was caused by World War II, which interrupted the progress of particle physics in general and Kemmer's career in particular. After the war he devoted his life to teaching rather than research, and became a beloved mentor and friend to several generations of younger physicists. Among the scientists that he launched into successful research careers are Abdus Salam FRS (Nobel laureate), Paul Matthews FRS, Richard Dalitz FRS and the present writer. In the American Mathematical Society Mathematical Genealogy Project, which includes physicists as well as mathematicians, Kemmer is academic ancestor to 217 descendants.

Life

I divide the memoir into two parts, the life and the work. Kemmer's life began in St Petersburg in 1911, where he was born into a Russian family belonging to the Baltic German minority of the Russian Empire. Baltic Germans frequently occupied high positions in the Imperial government. Kemmer's father was trained as an engineer and rose rapidly to a high rank in the Ministry of Ways and Communications. In 1916, the blackest year of World War I for both Britain and Russia, Kemmer's father travelled with his family from St Petersburg to London to purchase British railway rolling stock for the disintegrating Russian army. Luckily for the family and for six-year-old Nicholas, they were safe in

http://dx.doi.org/10.1098/rsbm.2011.0008

London when the Russian revolutions occurred a year later. They stayed in London for five years. Nicholas acquired his interest in science as I did 15 years later, by hanging around the Science Museum and the Natural History Museum in South Kensington. His preparatory school and my family home were both on Queen's Gate, within easy walking distance of the museums. Besides a taste for science, Nicholas also picked up a liking for England and an accent-free command of our language.

After the bolshevik victory in Russia, the Kemmers were officially stateless and unable to obtain British nationality. To survive economically, they moved in 1921 to Germany, where Nicholas's father was employed by the British Westinghouse Brake Company. They took German citizenship, and Nicholas switched to his third language for his education. But then another disaster struck. Nicholas's father fell victim to the terrible sleeping-sickness epidemic that ravaged Europe in the early 1920s. He recovered from the encephalitis but became a permanent invalid, with body and mind deteriorating from year to year. This was the same post-encephalitic Parkinsonism that Oliver Sacks later described poignantly in his book 'Awakenings'. For Nicholas's father there was no awakening. He lived for 20 years with a small pension to support his family, and finally ended his days in a nursing home in Switzerland. Meanwhile Nicholas made his way through the German school system. He concentrated his attention on science, and avoided discussions of literature and history. He concealed his distaste for the fanatical nationalism that was taught in the German schools and embraced by many of his schoolmates. In 1930 he enrolled as a physics student at the University of Göttingen. To escape the rising tide of Hitler, Kemmer's mother moved from Germany to Switzerland in 1931 and from Switzerland to Spain in 1936. But Kemmer needed to earn a living, and the only place where he had citizenship and could legally work was Germany. He stayed in Germany until 1933, when his teachers in Göttingen, Max Born (FRS 1939), Richard Courant and Edmund Landau, were dismissed from their jobs, and the greatest concentration of mathematicians in the world was destroyed. Max Born escaped to Edinburgh, Richard Courant to New York, and Edmund Landau to Cambridge. Kemmer could see that there was no future for him in Germany, so he joined his mother in Switzerland. There he could not legally work, but he could live and study. He enrolled at the University of Zurich with Gregor Wentzel as his advisor. Within two years he completed a thesis (1)* and became a Doctor of Philosophy.

Although Kemmer was a student at the university, he quickly established friendly relations with Wolfgang Pauli (ForMemRS 1953), who was a professor at the ETH (Federal Institute of Technology). The ETH was within easy walking distance of the university, and Pauli was a livelier character than Wentzel. Pauli was notoriously rude to people whom he considered to be fools, but he took a liking to Kemmer and treated him with respect. In 1936 Kemmer was in desperate need of a job, and Pauli twice came to his rescue. First Pauli offered him a job as his own assistant. This was a coveted position, given only to people whom Pauli considered exceptionally capable. But according to Swiss law the position could only be given to a foreigner if no equally qualified Swiss candidate was available. The ETH administration told Pauli that a Swiss candidate, Guido Ludwig, was available, and therefore Kemmer was not eligible. Pauli fought hard for Kemmer, but the best deal he could negotiate was to appoint both Kemmer and Ludwig as assistants with the salary divided between them. Kemmer and Ludwig remained friends and published a paper together (4). Meanwhile Kemmer told Pauli

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

that he could not live for long on the half-salary and would have to look for another job. It happened that a research fellowship at Imperial College, London, was available, so Kemmer applied for it. He asked Pauli for a letter of recommendation, and Pauli came to his rescue a second time. According to Kemmer, the following conversation ensued (Enz & von Meyenn 1988, p. 92):

> Pauli: 'Now, Herr Kemmer, I got this letter from London. Your English is much better than mine. What should I say, "one of the more promising" or "one of the most promising"?'
>
> Kemmer: 'Well, Herr Professor, the latter is much stronger.'
>
> Pauli: 'Oh good, that's the way I'll write it.'

So Kemmer got the fellowship and moved to London in October 1936. He always said that Pauli, in spite of his reputation, was not cruel.

For Kemmer, the move to London felt like a homecoming. By a happy chance, he was working in the same area of London where he had roamed as a child. He stayed at Imperial College for three years, two years as a Beit Research Fellow and a third year as demonstrator in the mathematics department. The head of the mathematics department was Sydney Chapman FRS, a world-class geophysicist whose primary interest was the aurora borealis. Chapman was famous for bicycling to work every day through the streets of London and for leading expeditions to Alaska to observe the aurora. He did not pretend to understand the details of Kemmer's research but welcomed him as a friend and colleague. The London years were the time of Kemmer's peak activity as a research scientist. He remained in close touch with Wentzel and Pauli, and his most important work was done in response to a suggestion from Pauli. Kemmer was still a German citizen when war broke out in 1939. He became an enemy alien and had to appear before a tribunal that would decide whether he should be interned. Chapman went with him to the tribunal and testified that he was harmless and might be useful to the country. As a result, Kemmer was never interned.

In 1940 the British government ordered Kemmer to move to Cambridge. Although he was still an enemy alien, he was directed to join the French atomic energy project led by Hans von Halban and Lew Kowarski. The project had started in Paris under the leadership of Frederic Joliot (ForMemRS 1946). When France was overrun in 1940, Joliot stayed in Paris, but von Halban and Kowarski escaped to England with 180 litres of heavy water (^2H_2O), at that time almost the whole of the world supply. The French project continued in exile in Cambridge, allowed by the British authorities to share information about nuclear power but not about nuclear weapons. Kemmer stayed with the project for four years in Cambridge and for a final year in Montreal. He never became deeply involved in the engineering work of the project, either in Cambridge or in Canada. His official title was Information Officer, responsible for keeping track of reports and documents in a bureaucratic organization with arcane rules of secrecy. The project was divided into two factions, one led by von Halban and the other by Kowarsky, who barely communicated with each other. Kemmer was one of the few people who was trusted with information from both sides.

During the wartime years in Cambridge, the physics teaching staff at the university had mostly disappeared into war projects, but there were still students to be taught. Kemmer was glad to take time off from his duties at the atomic energy project, to teach regular courses for undergraduates at the Cavendish Laboratory. He acquired a taste for teaching, and became known to the university authorities as an exceptionally conscientious teacher. Before moving to Montreal he had become a British subject. When the war ended, he was invited by

Cambridge University to return to Cambridge as a university lecturer, with a Staff Fellowship at Trinity College to make him a full member of the Cambridge academic community. For the first time in his life he had a regular job and a regular home.

In 1946 I occupied Kemmer's old position as demonstrator in the mathematics department of Imperial College, with Sydney Chapman still in charge. Chapman was as helpful to me as he had been to Kemmer. He saved me from a year of postwar military service by certifying that my job in his department was essential to the nation. I was then a pure mathematician, working on problems in number theory. I told Chapman that I was intending to switch from number theory to physics and that I hoped to find a mentor who could help me to get started as a physicist. Chapman told me that he knew exactly the right person. The name of the person was Nicholas Kemmer: he had done the same job at Imperial College that I was doing seven years later, he was an expert in theoretical particle physics, and he was just then returning from Canada to Cambridge.

I arrived at Trinity College in September 1946 with the intention of learning modern physics. When I arrived there, I found that experimental physics was at a low ebb. The experimenters had been away during the war. In 1946 they were still struggling to get started on new enterprises, which were to achieve huge success within a few years: the beginnings of the new sciences of radio-astronomy and molecular biology. I understood that Martin (later Sir Martin) Ryle (FRS 1952) with his radio receivers and Max Perutz with his haemoglobin crystals were doing exciting stuff, but the stuff they were doing was not physics. I enjoyed talking with experimenters, but my more urgent need was to talk to a competent theorist.

Following Chapman's advice, I contacted Kemmer as soon as I could. I quickly found that he was the teacher I needed. He became a friend as well as a teacher. Our friendship remained alive and well for 52 years until his death. In his first year as lecturer he gave two courses of advanced lectures, one on nuclear physics and one on quantum field theory. Both courses were full of new information, not available anywhere else in England at that time. During his five years as house theoretician to the French atomic energy project, Kemmer had mastered the newly discovered facts and theories of nuclear physics, many of which had not yet been published. He gave the students a thorough understanding of the subject, which I put to good use 10 years later when I helped to design a commercial nuclear reactor.

Kemmer's quantum field theory course was like his nuclear physics course, a comprehensive and up-to-date source of information about new ideas. Nothing like it existed anywhere else in England in 1946. Quantum field theory was not a single coherent theory of elementary particles but a collection of alternative mathematical models, each attempting to combine the ideas of classical relativity with quantum mechanics. The various models were suggested by various theoretical ideas and various fragments of experimental data.

Each model was mathematically incomplete and often inconsistent. Only a few experts knew enough about the mathematical formalisms and the experimental facts to be able to give a comprehensive and comprehensible account of the subject. Kemmer was one of the few. His course was a distillation of the wisdom of continental Europe, at that time still little known in England and America. Quantum field theory had been invented in Europe by Werner Heisenberg (ForMemRS 1955), Enrico Fermi (ForMemRS 1950) and Paul Dirac FRS. Although Dirac was English, the theory was for a long time more highly regarded in continental Europe than in England and America. The great experimenter Ernest Rutherford FRS expressed the common English view of abstract theories: 'The theorists play games with their symbols, but we at the Cavendish Laboratory turn out the real facts of nature.'

In 1946 the only existing textbook on quantum field theory was by Kemmer's teacher Gregor Wentzel (Wentzel 1943), written in Zurich and published in 1943 in Vienna in the middle of the war. Kemmer possessed a copy of Wentzel's book and allowed me to borrow it. It was at that time a treasure without price. I believe there were then only two copies in England. It was later reprinted in America and translated into English. In 1946, few people in England knew of its existence and even fewer considered it important. Kemmer not only lent it to me but also explained why it was important. When I arrived in America as a graduate student one year later, I found myself, thanks to Kemmer, the only person in the Cornell physics department who knew about quantum field theory.

Kemmer's job at Cambridge gave him two heavy burdens. As a university lecturer he had a load of classroom teaching, and as a Trinity College Staff Fellow he had several undergraduates to supervise. I was appalled to see the long hours he had to spend supervising students every day. There was nobody else who supervised students as conscientiously as he did, and so they came to him in big numbers. He never complained, but he paid a heavy price for being such an excellent supervisor. He had no time left over to resume the research career so brilliantly begun 10 years previously. Kemmer and I were both living in Trinity College. He was a Staff Fellow and was treated by the college as a drudge, while I was a Research Fellow with no duties and complete freedom to do whatever I liked. This was a monstrously unfair division of labour, but Kemmer accepted it without any sign of resentment. He was as generous in spending time with me as he was with his students. He always had time to advise me, to explain the difficult points in Wentzel's book, and to share with me his vision of quantum field theory as the key to a consistent mathematical description of nature. He was the most unselfish scientist I ever knew.

In the spring vacation of 1947, the young physicists of Cambridge scraped together enough money to rent a chalet at Arosa in Switzerland for three weeks. The Swiss authorities at that time were desperate to revive their tourist industry after six years of war. The Germans who had in the past been their best customers were down and out, and so the Swiss offered enticing bargains to the British. The British pound was then a non-convertible currency, but the Swiss changed our pounds into francs at a generous rate of exchange. A dozen of us travelled to Arosa for three weeks of luxurious living and splendid skiing. By a freak of nature, a dust storm from the Sahara had drifted over Switzerland that winter and coloured the snow high up on the mountains with bright streaks of orange and pink. Our party included three physicists who later became famous, namely Nicholas Kemmer, Tommy Gold (FRS 1964) and Hermann (later Sir Hermann) Bondi (FRS 1959), together with their girlfriends Margaret Wragg, Merle Tuberg and Christine Stockman. The purpose of the trip was not to discuss physics but to celebrate the rites of spring. The celebration resulted in three weddings soon after we returned to Cambridge. All of us who had shared our lives with Nicholas and Margaret at Arosa had hoped that this would happen. Margaret had never been on skis before, and Nicholas was constantly with her on the slopes giving her help and encouragement. They obviously cared more for each other than for themselves. They were married in the summer of 1947 and remained inseparable until the death of Nicholas 51 years later. Together they raised two sons and a daughter, and enjoyed watching eight grandchildren grow up.

During his years as a stateless person and later as an enemy alien in Britain, Kemmer suffered more than his fair share of inconveniences caused by inflexible immigration laws. In 1942, when he became a British subject, he supposed that chapter of his life to be over. But worse was still to come. In 1951 he was invited to be a visiting member at the Institute for

Advanced Study in Princeton. He looked forward to spending a year at the institute, which would have given him a good chance of getting back into productive research. He could not accept the invitation because the US government refused to give him a visa. At that time the US visa authorities were at their most capricious. Kemmer could never find out why his visa was denied. He conjectured that the denial resulted from the fact that he had been a friend of Alan Nunn May, a member of the Montreal atomic energy project who was arrested and convicted of spying for the Soviet Union. In fact, Nunn May had never been working on nuclear weapons, and the information that he gave to the Russians was militarily harmless. Kemmer was known to be unfriendly to the Soviet government, which had declared his father to be a traitor and robbed him of his property. But in the US visa department, logic did not prevail. The denial of a visa in 1951, when Kemmer was young enough to learn as well as to teach, was damaging both to him and to the Institute for Advanced Study. Many years later, he obtained a visa without difficulty and visited America several times.

After seven years of intensive teaching at Cambridge, Kemmer moved to Edinburgh in 1953 to succeed Max Born as Tait Professor of Natural Philosophy. Max Born had always felt isolated in Edinburgh and returned to Germany as soon as he retired. Kemmer made sure he would not be so isolated. He continued to live gregariously, making friends with a heterogeneous crowd of students and colleagues, as he had done at Cambridge. He took pride in the fact that students came to Edinburgh from a wide variety of countries. He served as professor until his retirement in 1979, and then stayed in Edinburgh as Professor Emeritus for the rest of his life.

During his years as professor he devoted much of his time to teaching and mentoring students, and also learned to be an effective administrator. He attracted many capable young scientists to Edinburgh, among them Peter Higgs (FRS 1983), who became a close friend and colleague. Higgs first came to Edinburgh in 1954 to spend two years with Kemmer as a postdoctoral student. He returned to Edinburgh permanently as a lecturer in 1960, became Professor of Theoretical Physics in 1980, and remains there 30 years later. Higgs and Kemmer had similar interests, not only in theoretical physics but also in politics. Both of them were for a while active participants in the Campaign for Nuclear Disarmament.

The following paragraphs were contributed by Peter Higgs in a personal letter to me:

> The status of theoretical physics at Edinburgh University was one of Nick's major preoccupations between 1953 and 1970. When he was appointed in 1953, the theoretical physics group occupied part of the basement of the Natural Philosophy building, but they were not considered to be real physicists by the occupant of the ancient (1583) Chair of Natural Philosophy, Norman Feather [FRS] (one of Rutherford's disciples). Feather's dictum was 'Physics is an experimental subject: there is no such thing as theoretical physics.' On his arrival in Edinburgh, Feather had decreed that Max Born and his two colleagues were not suitable people to teach physics students.
>
> Nick's first move was to set up a B.Sc. Honours degree in Mathematical Physics. Meanwhile, a University-owned building in a neighbouring street had become vacant, and in 1955 we moved into it, with the title 'Tait Institute of Mathematical Physics'. As a tidying up operation, in 1966 Nick persuaded the Senatus to change the title of the Tait Chair from Natural Philosophy to Mathematical Physics. But his long-term aim was to become part of Physics.
>
> In the late sixties the British universities were undergoing the Robbins expansion, so new chairs were being created. By 1970 Feather had two colleagues designated Professors of Physics, and the University Grants Committee was pressing him to accept in his department another professorial appointment, preferably a theoretical physicist. Out of the blue, during a discussion of this he turned to Kemmer and said (roughly) 'It doesn't make sense to have a theoretical physicist

in my department while there is a separate Tait Institute of Mathematical Physics across the street. Why don't we unify our departments to accommodate this new chair?' Nick was quite shattered, because he had been advocating this for years without any success.

The Department of Physics came into existence (replacing Natural Philosophy and Mathematical Physics) in 1971 when the first phase of the James Clerk Maxwell Building was ready for us. The new chair, which had triggered the unification, was never filled, and disappeared during a subsequent funding squeeze.

In 1960 Kemmer took the lead in founding and organizing the Scottish Universities Summer School in Physics. This is an annual event, with students and lecturers from many countries meeting to study topics in theoretical physics. Another useful hobby that he pursued in Edinburgh was the translation of Russian books. At that time the leading Russian physics journals were available in English translation, but many important Russian books were not. Because Kemmer was fluent in Russian, and some of the leading Russian physicists wrote excellent books, he was happy to exercise his skill as a translator. He published four translated books, the best known being *The theory of space, time and gravitation* by Vladimir Fock, a text-book of general relativity, presenting the subject in an unusual and illuminating way (9). Fock was a highly original physicist, whose contributions to many areas of physics are not well known outside Russia. Another book translated by Kemmer was *What is relativity?* by Lev Landau and Yuri Rumer (11). Landau was a justly famous physicist, Rumer a less famous friend and colleague. Landau and Rumer were arrested on the same day in 1938, victims of Stalin's purges. Landau was released after a year, but Rumer spent a large fraction of his life in the Gulag.

Kemmer received the Hughes Medal from the Royal Society in 1966 and the Planck Medal from the German Physical Society in 1983. He enjoyed travelling to many countries and making new friends. But he did not enjoy sitting in committees. After his retirement as professor, he was happy to escape from academic committees and spend time cultivating his garden. He was a passionate gardener and knew how to make the best of the Scottish climate.

The last time I saw Nicholas and Margaret was at the celebration of the 200th birthday of George Green at the University of Nottingham in the summer of 1993. This was a happy reunion, in which Julian Schwinger and his wife, Clarice, also took part. George Green was a self-taught genius who grew up as the child of a miller in Nottingham, spending long nights in the workroom at the top of his father's windmill, where he had the responsibility for rotating the mill so as to keep the blades properly oriented to the wind. When the wind was steady, he spent the nights reading mathematical books by candle-light. He mastered the theory of partial differential equations and discovered that the most general solutions of wave equations are linear combinations of special solutions that are now known as Green functions. Both Kemmer and Schwinger used Green functions extensively in their reconstructions of quantum field theory more than 100 years later. We were invited to Nottingham to talk about the history of George Green and his functions. Schwinger was the star of the show and gave two brilliant talks. In Schwinger's talks and in Green's papers I could recognize the same functions that I had encountered 45 years earlier in Wentzel's book and in Kemmer's lectures.

Participants at the Nottingham conference spent an afternoon visiting the Green family windmill, which is still preserved and now used as an educational centre with science programmes for schoolchildren. Nicholas and Margaret were chatting happily with the children about windmills past and future. Five years after the Nottingham outing, Nicholas died peacefully at his home in Edinburgh.

Work

Kemmer's style as a scientist was formed during the two years that he spent as a student in Göttingen, absorbing ideas from the great mathematicians Weyl, Courant and Landau. Göttingen was then the mathematical capital of the world, upholding the great traditions of Gauss and Klein, with the aged but still active Hilbert presiding. Kemmer was lucky to arrive at this mathematical paradise two years before Hitler put an end to it. He knew that he had no time to lose, and plunged immediately into the most advanced courses. What he learned in these courses was mostly formal mathematics, and formal mathematics remained the basis of his thinking for the rest of his life. At the same time, he took a course in experimental physics from Robert Pohl, one of the founding fathers of condensed-matter physics, and enjoyed the practical demonstrations. He never intended to be an experimenter himself, but he learned enough about experiments to be able to talk and work with experimenters. He could read the experimental literature and tell the sense from the nonsense.

When Kemmer started as a graduate student in Zurich, Wentzel immediately gave him a research problem on the frontier of existing knowledge, to calculate the behaviour of a point electron interacting with the quantized electromagnetic field. This was a formidable problem, and Kemmer did not solve it. It was solved 15 years later by Tomonaga, Schwinger and Feynman, in three different ways. To solve it required a reconstruction of the theory of the electron as well as of the electromagnetic field. All that Kemmer could do in 1933 was to take the first step towards a solution, calculating the self-energy and the self-force of a point electron with the existing formalism and finding that both were infinite. The appearance of these infinite quantities proved that the existing formalism was inconsistent, and pointed the way towards the discovery of a better formalism. Kemmer's calculation was published in *Annalen der Physik* (1) and earned him a doctorate in 1935. In the same year, Hideki Yukawa (ForMemRS 1963) in Japan started a new era in particle physics with his proposal that a new particle, the meson, was responsible for nuclear forces, just as the photon was responsible for electromagnetic forces (Yukawa 1935). Kemmer, like other physicists at that time, began to think seriously about mesons. Great confusion was caused by the fact that the cosmic-ray particles known at that time did not behave like Yukawa mesons. Yukawa mesons were expected to interact strongly with any kind of matter, whereas the cosmic-ray particles seemed hardly to interact at all. The confusion was only resolved 12 years later when Cecil Powell FRS and his colleagues at Bristol discovered that there are two kinds of mesons, with the primary π mesons decaying into the secondary μ mesons (Lattes *et al*. 1947). Then it became clear that the cosmic-ray particles were mostly μ mesons, whereas the π mesons might be the particles postulated by Yukawa to explain nuclear forces.

In his last year in Zurich, Kemmer made friends with Viktor Weisskopf, who had been Pauli's assistant at the ETH. Weisskopf was working on the same problem as Kemmer with a similar lack of success, and so they joined forces. Together they did an improved calculation of the electron self-energy, describing the electron by a field theory including positive as well as negative charges. The self-energy was still infinite, but less infinite than before. This was another useful step towards an improved theory. Kemmer also collaborated with Weisskopf on a calculation of Delbrück scattering. This was the scattering of photons by a static electric field, caused by the quantum fluctuations of electron–positron pairs in the vacuum excited by the field. The scattering process was named after Hans Delbrück (ForMemRS 1967), who was then a young physicist at Copenhagen and later became a famous biologist. To calculate the scattering was a strenuous exercise in quantum field theory, stretching the limits of what could be done with the clumsy formalism of quantum electrodynamics as it then existed. The details of the calculation were published in three papers (2–4). The results were later confirmed by accurate measurements of the scattering of γ-rays by lead atoms. The collaboration with Weisskopf ended with Kemmer's move to England in 1936.

Kemmer found nobody at Imperial College with whom he wished to collaborate, but he continued to share his ideas by correspondence with Pauli. A large part of Kemmer's work as a research scientist is exceptionally well documented because he discussed it in detail with Pauli and most of their letters are preserved. In the collected correspondence of Pauli edited by Karl von Meyenn (von Meyenn 1985, 1993), there are 17 letters and 5 postcards from Pauli to Kemmer, 9 letters and 1 postcard from Kemmer to Pauli, written during the four years, 1936–40, when Kemmer was in London and Pauli was in Zurich. They discuss Kemmer's ideas as they were taking shape, and they are often more illuminating than the published papers, which present only the finished product. As long as Kemmer was in London he continued to confide in Pauli, and Pauli continued to give him generous help. All the letters from this period were written by hand. They must have cost Pauli many hours of his precious time. In all this correspondence there is no trace of the harsh judgment and rude manner for which Pauli was famous. In 1940 Kemmer moved to Cambridge, and Pauli to Princeton. There is one more substantial letter about physics, written by Pauli from Princeton in 1941 (von Meyenn 1993, pp. 95–97). After that there are only a few brief exchanges, still friendly but not discussing physics.

The most dramatic item in the Pauli–Kemmer correspondence is a postcard from Pauli to Kemmer dated 15 December 1936, soon after Kemmer's arrival in London (von Meyenn 1985, p. 491). Here is an excerpt from the postcard [my translation]:

> Take a more careful look at the new American papers on nuclear forces in the November 1 *Physical Review*. They discuss the possibility that all the non-electromagnetic forces are independent of charge, meaning that the proton–proton and proton–neutron forces are the same. The idea has a certain inner logic. There may be reasonable explanations for this that you could calculate.

Pauli knew Kemmer's strengths and weaknesses. Kemmer had mastered the abstract mathematics of quantum field theory, but knew little about the phenomenology of nuclear physics. The American papers to which Pauli refers are reporting and interpreting experimental results, and Kemmer would probably not have paid attention to them if Pauli had not sent the postcard.

The most important of the American papers is 'The scattering of protons by protons' (Tuve *et al*. 1936). This paper is 20 pages long and reports a revolution in experimental particle physics. Tuve and his colleagues at the Carnegie Institution in Washington had taken enormous trouble to measure the differential cross-section for the scattering of protons by protons as a function of energy and angle, with a resolution of the order of 1% in both energy and angle, and with an absolute accuracy of the order of 1%. The energy of the incident protons was varied over the range 600–900 kV. To obtain 1% accuracy of the cross-sections, many thousands of scattering events were measured at each energy and each angle. This was the culmination of an engineering programme that had taken them 10 years to complete. The source of their protons was a newly constructed Van de Graaff accelerator, which gave more precise control of the particle energy and beam quality than any other accelerator at that time. Their paper ends on a triumphant note: 'It thus appears that a real beginning has been made toward an accurate and intimate knowledge of the forces which bind together the primary particles into the heavier nuclei so important in the structure and energetics of the material universe.' The measurements were accurate enough to disentangle the nuclear and electromagnetic contributions to the proton–proton interaction. At the highest energy and the largest angle, the measured scattering cross-section was about four times larger than the cross-section resulting from electromagnetic interaction.

The second paper that Pauli called to Kemmer's attention was 'Theory of scattering of protons by protons' (Breit *et al*. 1936), which immediately followed the experimental paper in *Physical Review*. Gregory Breit, then at the University of Wisconsin in Madison, had been advising and encouraging the Carnegie Institution team for 10 years. The Breit paper analysed the experimental results by using accurate Coulomb wavefunctions for the protons, and found the results to be consistent with a simple short-range attractive nuclear force between protons with oppositely oriented spins. The main conclusion of the analysis is as follows:

> The interaction between protons as derived from the scattering experiments is found to be very nearly equal to that between a proton and a neutron in the corresponding condition of relative spin orientation and angular momentum (singlet S state). The proton–neutron values which come closest to being equal to the proton–proton values are those obtained by Fermi and Amaldi from the scattering and absorption of slow neutrons.

Two further papers in the same issue of *Physical Review* (Breit & Feenberg 1936; Cassen & Condon 1936) discussed the implications of the scattering measurements for the binding energies of heavy nuclei, and concluded that all the available evidence was consistent with the hypothesis of charge-independent nuclear forces.

In response to Pauli's suggestion, Kemmer embarked on a two-year programme to explore the various possibilities for a quantum field theory describing mesons interacting with protons and neutrons. For each choice of meson theory and meson–nucleon interaction he calculated the nuclear forces and compared them with experimental results. The details of this work were published in three papers, two by Kemmer alone and one with Hans Fröhlich and Walter Heitler (FRS 1948) of Bristol University as collaborators. There were at that time two versions of meson theory, one (Pauli & Weisskopf 1934) with mesons of spin 0, and the other (Proca 1936) with mesons of spin 1. For each choice of meson spin there were two choices of meson–nucleon interaction. In Kemmer's first paper (5) he concluded that only one of the four possibilities was consistent with experiment, and that the mesons must have spin 1. In the second paper (6), with Fröhlich and Heitler, the spin-1 meson field theory was used to calculate the magnetic moments of proton and neutron. The magnetic moments were found to agree in sign and in order of magnitude with the measured values, but the calculation required an arbitrary cutoff of integrals at high frequencies, and the agreement with experiment was not impressive. The paper also included a calculation of the proton–proton force going to fourth order in the meson–nucleon interaction. The proton–proton force was found to be repulsive, contrary to the Tuve measurements. The conclusions of (6) were discouraging. None of the existing theories showed a convincing agreement with experiment. There was only one ray of hope. The existing theories followed Yukawa in postulating only charged mesons as the carriers of nuclear forces. If neutral mesons also existed, they might give rise to additional nuclear forces that would agree better with experiment. Several other papers (for example Yukawa *et al*. 1938; Bhabha 1938), published at the same time as (6), also suggested the introduction of neutral mesons as a possible way forward.

The last of the three Kemmer papers (7) broke the impasse. Kemmer did not merely add neutral mesons to the existing mixture. He understood that a particular combination of neutral with charged mesons would create a new three-fold symmetry of the entire formalism.

The new symmetry would be an extension of the old 'isotopic spin' symmetry discovered by Heisenberg, but now including mesons as well as protons and neutrons. In a theory with Kemmer's new symmetry, nuclear forces would automatically be charge-symmetric, not only when calculated by perturbation theory but also when calculated exactly. The proton–proton and proton–neutron forces would be equal, as Tuve had found. So, after two years, Pauli's suggestion to Kemmer bore fruit. Kemmer's symmetric meson theory was the first example of a field theory with an abstract symmetry going beyond the usual symmetries of space and time.

In his paper (7), Kemmer gave careful attention to the question of whether the neutral meson should be identical to its antiparticle. Because the positively and negatively charged mesons are antiparticles of each other, the threefold symmetry demands that the neutral meson should be the antiparticle of itself. Kemmer then concluded the paper by comparing the predicted charge-independent nuclear forces with experiment. He found that with spin-1 mesons the agreement was acceptable but not compelling. His model remained for many years a mathematical speculation, notable for its formal beauty rather than for practical use. Eleven years later, when neutral mesons were finally discovered, they turned out to have spin 0 and not 1. There was, as Kemmer had predicted, a symmetrical triplet of mesons, but they did not explain nuclear forces as he had hoped. As time went on, the picture became more complicated as several kinds of meson were discovered with spin 1. A combination of ρ mesons with spin 1 and π mesons with spin 0 could explain nuclear forces with some degree of accuracy, but the explanation proved inadequate when experiments measuring deep inelastic scattering became possible. The Kemmer model of the meson fields turned out in the end to explain only a small part of the nuclear forces, but it pushed our thinking in the right direction to find better models. Pauli's basic insight, that a combination of phenomenological nuclear physics with abstract mathematical field theory would be the key to understanding, was correct.

Now, with 70 years of hindsight, we know that protons and neutrons and mesons are little bags of quarks, and that theories representing them as point particles can only be rough approximations. The proton–meson interaction is a rearrangement of five quarks, and the proton–proton interaction is a rearrangement of six quarks. The idea of describing such rearrangements by simple point interactions turned out to be an illusion. Now we have a standard model of particle interactions based on quarks and gauge fields instead of on protons and mesons. The standard model agrees magnificently with experiment. Not much is left of the old meson-theory models with which Kemmer struggled in the 1930s. The one thing that survives from the 1930s is the threefold symmetry that he discovered, now hugely extended and incorporated within a hierarchy of larger symmetries. Kemmer's main contribution to modern physics is not the threefold symmetry itself but the general idea of looking for larger abstract symmetries in nature. The threefold symmetry turned out to be the seed from which a majestic tree of symmetries grew, to become the basis of particle physics in the twenty-first century.

In his last year in London, Kemmer worked on a new formal approach to meson theory based on matrix algebra. Two versions of matrix algebra had been spectacularly successful in describing the behaviour of particles with spin $\frac{1}{2}$. First Pauli had used matrix algebra as the basis for his non-relativistic theory of electron spin, and then Dirac had used another matrix algebra as the basis for his theory of relativistic electrons. The Pauli and Dirac algebras were essential components of the quantum revolution that transformed physics during the 1920s.

To Kemmer and Pauli in 1939 it seemed reasonable to hope that similar matrix algebras could be equally successful in providing a new foundation for theories of particles with spin 0 and 1. Matrix algebras for meson theories had already been suggested by R. J. Duffin (Duffin 1938).

Kemmer investigated these algebras much more thoroughly, so that they are now known as Duffin–Kemmer algebras. The idea was to express the field equations for particles of spin 0 and 1 as matrix equations, just as Dirac had done for particles of spin $\frac{1}{2}$. Kemmer proved that the most general algebra consistent with the physical requirements would have dimension 126, which happens to be the sum of three squares: $126 = 1^2 + 5^2 + 10^2$. The algebra will then have three irreducible representations, a matrix algebra of rank 10 representing particles with spin 1, a matrix algebra of rank 5 representing particles with spin 0, and a trivial algebra of rank 1 representing nothing.

Pauli became enthusiastically involved in these algebraic speculations. During the spring of 1939 he exchanged letters and postcards with Kemmer at a rate of two per week (von Meyenn 1985, p. 613). They wrote long letters filled with equations, punctuated with question marks and exclamation marks. Their enthusiasm began to wane in June 1939, when it became clear that an extension of the matrix algebra to describe particles of higher spin, $\frac{3}{2}$ and 2, would not work. Pauli lost interest quickly, and Kemmer more slowly. Their matrix algebra remained a mathematical toy, amusing to play with but leading to no new physical insight. Kemmer did not bother to write up his results for publication (8) until four years later. For reasons that are still not clear, matrix algebras are enormously useful and fruitful in the theory of particles of spin $\frac{1}{2}$, but they fail dismally to fulfil Kemmer's hopes for particles of integer spin. Dirac's matrix algebra provides a leitmotiv for one half of nature, the world of protons and electrons, while the other half of nature, the world of photons and gauge fields, marches to a different drum.

Kemmer's last technical research paper (10) was written after his move to Edinburgh, where he continued to think about spin-1 fields. During his Edinburgh years, evidence accumulated that spin-1 fields have a dominant role in nature. C. N. Yang (ForMemRS 1992) and R. Mills (Yang & Mills 1954) discovered their non-Abelian gauge field model, based on a triplet of spin-1 fields with the symmetry of the isotopic spin group. Then S. L. Glashow published a paper (Glashow 1959) pointing the way towards the later Glashow–Weinberg–Salam unification of the weak interactions with electromagnetism, basing his argument on another triplet of spin-1 fields. Kemmer paid close attention to these developments. He knew more than the authors of the new theories about the intricacies of spin-1 fields. He knew that only a restricted class of interactions of spin-1 fields was mathematically legitimate. The discoverers—Yang, Mills, Weinberg, Salam and Glashow—did not bother about mathematical legitimacy. They assumed that if spin-1 fields existed in nature, a consistent mathematical formalism must also exist to describe them. Kemmer's paper confirmed that the discoverers were right. Guided by nature and by physical intuition, the discoverers chose interactions that Kemmer showed to be mathematically consistent.

In December 1958 Pauli died of cancer of the pancreas after a short illness. Kemmer published his paper (10) in the issue of the journal *Helvetica Physica Acta* dedicated to the memory of Pauli. Because *Helvetica Physica Acta* was not a widely read journal (it is now defunct, having ceased publication in 1998), Kemmer's paper received little attention. Kemmer said that the only reference to it in the literature was a favourable comment by his friend Salam. For Kemmer, the purpose of publication was not to seek public acclaim: he chose to publish his paper in *Helvetica Physica Acta* because this allowed him to put a final sentence at the end of it: 'The writer is sincerely grateful to be able to join in this tribute to the memory of Wolfgang Pauli, one of the greatest physicists of this century, who was also so good a friend and wise a counsellor.'

Kemmer was wise enough to be aware that in 1960 he had come to the end of his career as a research scientist. Theoretical physicists usually do their best work as creative scientists before the age of 40 years, and after that age they need to find another line of work. Kemmer's other line of work was teaching, which kept him busy and happy for the second half of his life. I was lucky to encounter him at a critical moment in both our lives, when he was beginning to teach and I was beginning to learn modern physics. I was only one of the many young people who came to him as students, learned their trade in his classes and tutorials, and ended by becoming his lifelong friends.

Acknowledgements

In writing this brief account of Kemmer's life and work, I have been greatly helped by the unpublished autobiography that he wrote for the Royal Society. I am indebted to Keith Moore for a copy of the autobiography. I am also indebted to Peter Higgs FRS for contributing a description of Kemmer's adventures as an administrator at Edinburgh University.

The frontispiece photograph was taken in 1946 by Lotte Meitner-Graf and is reproduced with permission.

References to other authors

Bhabha, H. 1938 On the theory of heavy electrons and nuclear forces. *Proc. R. Soc. Lond.* A **166**, 501–528.

Breit, G. & Feenberg, E. 1936 The possibility of the same form of specific interaction for all nuclear particles. *Phys. Rev.* **50**, 850–856.

Breit, G., Condon, E. U. & Present, R. D. 1936 Theory of scattering of protons by protons. *Phys. Rev.* **50**, 825–845.

Cassen, B. & Condon, E. U. 1936 On nuclear forces. *Phys. Rev.* **50**, 846–849.

Duffin, R. J. 1938. On the characteristic matrices of covariant systems. *Phys. Rev.* **54**, 1114.

Enz, C. & von Meyenn, K. (eds) 1988 *Wolfgang Pauli, das Gewissen der Physik*. Braunschweig: Friedrich Wieweg u. Sohn.

Glashow, S. L. 1959 The renormalizability of vector meson interactions. *Nucl. Phys.* **10**, 107–117.

Lattes, C. M. G., Occhialini, G. P. S. & Powell, C. F. 1947 Observations on the tracks of slow mesons in photographic emulsions. *Nature* **160**, 453–456.

Pauli, W. and Weisskopf, V. 1934 Über die Quantisierung der scalaren relativistischen Wellengleichung. *Helv. Phys. Acta* **7**, 709–731.

Proca, A. 1936 Sur la théorie ondulatoire des électrons positifs et négatifs. *J. Phys. Radium* **7**, 347–353.

Tuve, M. A., Heidenburg, N. P. & Hafstad, L. R. 1936 The scattering of protons by protons. *Phys. Rev.* **50**, 806–825.

Von Meyenn, K. (ed.) 1985 *Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.*, vol. 2 (1930–1939). Berlin: Springer-Verlag.

Von Meyenn, K. (ed.) 1993 *Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.*, vol. 3 (1940–1949). Berlin: Springer-Verlag.

Wentzel, G. 1943 *Einführung in die Quantentheorie der Wellenfelder*. Wien: Franz Deuticke. (Reprinted in 1946 by Edwards Brothers, Ann Arbor, MI.)

Yang, C. N. & Mills, R. 1954 Conservation of isotopic spin and isotopic gauge invariance. *Phys. Rev.* **96**, 191–195.

Yukawa, H. 1935 On the interactions of elementary particles. I. *Proc. Phys. Math. Soc. Jpn* **17**, 48–57.

Yukawa, H., Sakata, S. & Taketani, M. 1938 Interactions of elementary particles. III. *Proc. Phys. Math. Soc. Jpn* **20**, 319–340.

BIBLIOGRAPHY

The following publications are those referred to directly in the text. A full bibliography is available as electronic supplementary material at http://dx.doi.org/10.1098/rsbm.2011.0008 or via http://rsbm.royalsocietypublishing.org.

(1) 1935 Über die Elektromagnetische Masse des Dirac-Elektrons. *Annln Phys.* **22**, 674–712.

(2) 1936 (With Viktor Weisskopf) Deviations from the Maxwell equations resulting from the theory of the positron. *Nature* **137**, 659.

(3) 1937 Über die Lichtstreuung an Elektrischen Feldern nach der Theorie des Positrons. I. *Helv. Phys. Acta* **10**, 112–122.

(4) (With G. Ludwig) Über die Lichtstreuung an Elektrischen Feldern nach der Theorie des Positrons. II. *Helv. Phys. Acta* **10**, 182–184.

(5) 1938 Quantum theory of Einstein–Bose particles and nuclear interaction. *Proc. R. Soc. Lond.* A **166**, 127–153.

(6) (With H. Fröhlich & W. Heitler) On the nuclear forces and the magnetic moments of the neutron and the proton. *Proc. R. Soc. Lond.* A **166**, 154–177.

(7) The charge-dependence of nuclear forces. *Proc. Camb. Phil. Soc.* **34**, 354–364.

(8) 1943 The algebra of meson matrices. *Proc. Camb. Phil. Soc.* **39**, 189–196.

(9) 1959 (Translation) *The theory of space, time and gravitation*, by V. A. Fock. Oxford: Pergamon Press.

(10) 1960 On the theory of particles of spin one. *Helv. Phys. Acta* **33**, 829–838.

(11) (Translation) *What is Relativity?*, by Lev Landau & Yuri Rumer. Edinburgh: Oliver & Boyd.