# RICHARD PHILLIPS FEYNMAN

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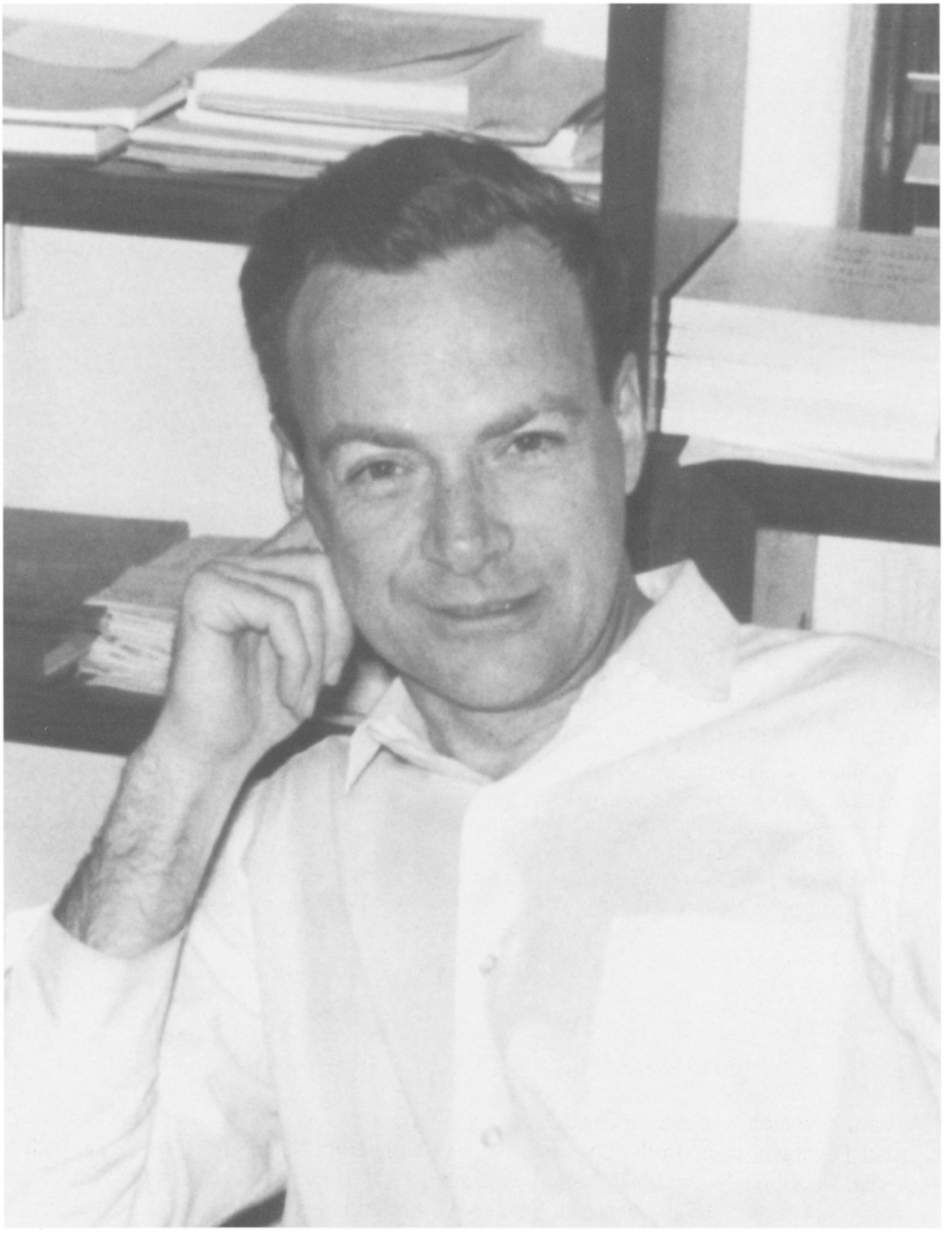

R. P. Feynman

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## Elected For.Mem.R.S. 1965


By Jagdish Mehra

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Just as Josiah Willard Gibbs was assuredly the greatest American physicist of the nineteenth century, Richard Phillips Feynman was arguably the greatest American-born theoretical physicist of the twentieth century. Feynman was among the truly great physicists of the world.

Mark Kac, the eminent Polish-American mathematician, wrote (Kac 1985, p. xxv):

> In science, as well as in other fields of human endeavor, there are two kinds of geniuses: the 'ordinary' and the 'magicians'. An ordinary genius is a fellow that you and I would be just as good as, if we were only many times better. There is no mystery as to how his mind works. Once we understand what he has done, we feel certain that we, too, could have done it. It is different with magicians. They are, to use a mathematical jargon, in the orthogonal complement of where we are and the working of their minds is for all intents and purposes incomprehensible. They seldom, if ever, have students because they cannot be emulated and it must be terribly frustrating for a brilliant young mind to cope with the mysterious ways in which the magician's mind works. Richard Feynman [was] a magician of the highest caliber.

### Family background, early life and education

Richard Feynman was born in Far Rockaway, Queens County, New York. His father, Melville Arthur Feynman, came to the USA in 1895 as a child from Minsk, in Byelorussia, with his parents. Melville's father, Jakob (Louis) Feynman, a Lithuanian Jew, had settled in Minsk with his family. Many of the Russian and Polish Jews at that time had a great faith in the new science that was developing, and Jakob Feynman had fallen in with a group of rationalists. Times were hard in Minsk, especially for the Jews, and Jakob—with his wife and son Melville—emigrated to America, and upon arrival he settled in Patchogue, Long Island, New York. It was in Patchogue that Melville grew up. He was taught at home, first by his father and then by special tutors. He attended the regular high school in Patchogue, but never went to college. His ambition was to go to a medical school, but not having much money, he enrolled in a homeopathic institute. He lived in a house where people were very poor, and he became involved in helping them. Because of lack of funds, he discontinued his medical education at the homeopathic institute. However, he was eager to learn and absorbed a great deal of knowledge, especially in the sciences; he learned from his father and other people, and taught himself. Like his father Jakob, Melville had a rational mind and liked those things that could be understood by thinking.

Melville Feynman had different jobs and occupations; apparently he never became a very successful businessman. He lived through the Depression, and was forced to make a living in different ways, such as running dry-cleaning stores and shirt manufacturing. Ultimately, he got his most permanent job as sales manager for a large uniform company called Wender and Goldstein. From then on he remained in the uniform business for most of his life.

Melville Feynman married Lucille Phillips. Her parents had come from Germany at a young age. Her father, Henry Phillips, became a very successful businessman; he manufactured shirts as a partner in the Phillips–Jones–Van Heusen Company, and was called 'the father of the trimmed-hat business'. (This quotation, like most others in this memoir, is taken from interviews and conversations between Feynman and myself in Pasadena, California, in January 1988.) Lucille attended the Ethical Culture School in New York City, which had a considerable influence upon her. (Interestingly, J. Robert Oppenheimer (For.Mem.R.S. 1962) also attended this school as a young boy.) She had an older sister named Pearl, who was a woman of superior intellect and liked plays and reading. She married a man called Harold Levine, whom she thought was compatible intellectually, 'but he was rather dull, uninteresting, and not a very able man'. Henry Phillips and his wife lived with their two daughters and their families in their large house with a big garden. The house, situated in rural surroundings in Far Rockaway, was a couple of miles from the beach.

Richard Phillips Feynman, the first child of Melville and Lucille, was born on 11 May 1918. Before Richard was born, Melville told Lucille, 'If it's a boy, he'll be a scientist', and he guided Richard in that direction during his childhood. Richard's sister, Joan, was born when he was nine years old. As Richard grew up, he had no brothers or sisters to play with. 'But we lived in this house where there were cousins, the children of my mother's sister, and we lived together, so it was very much as if there were brothers and sisters; there was a cousin three years younger than I, a girl called Frances, and a boy, Robert, three years older than I. We all played together.'

Melville played a lot with Richard, who went to kindergarten at about six years of age and became interested in mathematics and science quite early on. 'My father talked to me not only about mathematics, but about the whole range of things he was interested in. He was always telling me things.' By constantly bringing new concepts about nature to Richard's attention, he helped to develop his sense of curiosity. There were many occasions when Feynman's father taught him how to notice new things and explore the world around him. The Feynmans had the *Encyclopaedia Britannica* at home, and even when Richard was small his father used to seat him on his lap and read to him from the encyclopaedia, going from one subject to another. Melville would often take Richard to the Museum of Natural History in Manhattan, which—for Richard—became the most exciting place in the world. They would look at the dinosaur bones and great rocks, which had long cuts and grooves from the glaciers, and Melville would explain to the little boy how they were formed. Similarly, Melville filled Richard's childhood with other exciting experiences. One of the things that Melville taught his young son, besides science and nature, was a disrespect for the seemingly respectable—irreverence for certain kinds of authority—like the exaggerated reverence for the Pope and high military officials. 'The difference is the epaulettes', he would say in the case of a general, for example. 'It was always the uniform that gave the person authority; otherwise he was like anyone else. He, by the way, was in the uniform business. So he knew what the difference was with the uniform off and the uniform on; it was the same man for him.'

Although his father had been paramount in Feynman's growing up, his mother also exerted an important influence.

> My mother had a great sense of humor, much better than my father's. My father looked at things in a screwy way, a strange way. You look at it funny and it looks quite different, and that is always a little humorous because of the surprise. So that combined with my mother's sense of humor makes me have a lot of fun looking at things the wrong way around and having it come right. But my mother was a compassionate person whom everybody liked and with whom everybody could talk because she was a good listener. I did not learn that. My mother was one of those wonderful people that many others seek out to discuss their innermost problems with, but I did not learn from her how it works. I can't do it.

However, Feynman always remained very proud to admit that he had been trained by his father and that he had had such a wonderful mother.

When Richard was 10 or 11 years old, Melville took the family to live in the nearby town of Cedarhurst, where he was enrolled in the elementary school. As a boy in Cedarhurst, Richard used to read science fiction, which, because of his love for science, he found interesting. As time went on, he became upset with it, because it became more and more ridiculous that anything could happen in science fiction. After he reached high school, he did not read science fiction any more. At that time, Richard became interested in electrical devices. He had a laboratory at home, and he would repair radios in the neighbourhood to make a little money to buy things for his laboratory. He had a photoelectric cell. He made amplifiers, although not good ones. Nothing really worked for him, but he struggled with it. He had a laboratory bench, and batteries and motors, and so on.

In kindergarten and elementary school, Richard used to attend the equivalent of the Sunday School at the synagogue. There he would learn about the stories of the Old Testament and would apply his imagination to understand rationally the various accounts of the miracles. Even before he finished elementary school he had decided that he did not want to have anything to do with religion; for him science was much more important, as it reflected his own temperament and questioning attitude to every phenomenon. The Cedarhurst period had been very fruitful, but then the family again returned to Far Rockaway. This had to do with the precarious financial situation of the families who lived in the big house there.

> We weren't too well off. At that time, my father was making about $5000 a year. This was in the 1930s. It was a period of Depression. My father was in trouble; that was why we moved back to Far Rockaway. But I never felt poor. My father used to ask me to take the check to the bank; it was about $100 per week, and I remember thinking that it was a very nice amount, that everything was all right, we lived fine, and my ambition was to make that much money. So I knew that I wanted about $5000 a year, that's all I needed!

While he was attending the elementary school in Cedarhurst, Richard made the acquaintance of the science teachers at the Far Rockaway High School through his dentist Dr Marx, and they allowed him to come and play and work with instruments in the laboratory at the school one day a week, which he greatly enjoyed. Loved dearly by his parents, having the security of a good home life, he had a very cheerful and pleasant childhood. He had already formed a number of attitudes that would follow him throughout life, foremost among them being a rationalistic outlook, disrespect for authority, disdain for ceremony, a love of intellectual achievement, and a very happy and cheerful disposition. Both of his parents, and his growing success at school and cultivation of a creative outlook, had begun to contribute greatly to an enlarging world view.

In the autumn of 1931, at the age of $13\frac{1}{2}$, Richard began to attend the Far Rockaway High School. He had already known a number of his teachers there since his elementary school and all of them encouraged him a great deal, and he excelled in most of his subjects, especially science and mathematics; he had already learned calculus by private reading. In 1934, Richard's senior year in high school, another physics teacher, Abram Bader, joined the school as a physics teacher. Bader had wanted to become a trained physicist and was trying to obtain his PhD under Isidor Isaac Rabi at Columbia University, but the lack of financial support made him abandon his goal and he was forced to seek employment to teach high-school physics. As Bader recalled in a letter to me dated 8 December 1988,

> Richard became my student in the fourth and final year of high school. I had heard about the unusually bright boy from his chemistry teacher. I was about to start teaching a class in honor's physics and looked forward to having a group of students in which all the students would be bright. After my first day I realized that Feynman was *sui generis*. In only one day he stood out as the top student in a class of top students.

Bader found that Feynman already knew everything that was being taught, which was all too easy for him, and he was therefore noisy and disruptive. As Feynman recalled:

> So he gave me a book; he knew that I had learned calculus from other books. I was a senior then. He gave me a book entitled Woods's *Advanced calculus* [Woods 1932], which had Fourier series, Bessel functions, gamma functions, wondrous things that came out of the calculus, which I learned to play with! So that was a wonderful thing he did to me! I worked very hard on that, and I learned a great deal of mathematics in high school, mathematics of an advanced kind.

Feynman went through that entire book in about a month; it had taken Bader more than a year to master the subject.

On another occasion, Bader explained to Richard the principle of least action: there is a number, the kinetic energy minus the potential energy—the action—that, when averaged over the path, is least for the true path. 'This is philosophically a delightful thing. It's a different kind of way of expressing the [dynamical] laws. Instead of differential equations, it tells the property of the whole path. And this fascinated me. The rest of my life I have played with action, one way or another, in all my work, and have loved it always.' The principle of least action became a kind of mantra for him; in all the problems of theoretical physics that he treated, he invoked this principle wherever he could. This principle, together with the principle of least time, became his own in every way.

At his graduation, Feynman won many medals, for best in physics, best in chemistry, best in mathematics and, most surprisingly, honours in English, in which he had been relatively poor. Thus laden with honours, Richard Feynman graduated from high school and got ready for college. He applied for admission to various colleges, among them Massachusetts Institute of Technology (MIT), Columbia University, and the City College of New York. At Columbia, admission was denied him; Feynman related that it had something to do with the Jewish quota for the freshman class at Columbia. At MIT there were fewer scholarships, which he applied for but did not get. However, he was admitted as a regular student there with a small scholarship (about $100 per annum), but he did not receive the big scholarship he had hoped for. At

MIT there were fraternities, and two of them were Jewish; Richard joined the Phi Beta Delta fraternity. By taking examinations he received advanced placement and opted for studies in physics, mathematics, chemistry, Reserve Officers Training Corps and English, from which he derived pleasure in decreasing order. He found mathematics too abstract and electrical engineering too practical, so he decided to pursue something in between, like physics—which turned out to be the right thing—and he was allowed to make the change without penalty. Because of his advanced placement, he obtained permission to take higher-level courses in theoretical physics. Another young man who was allowed to do that, and one who would become his friend and discussion partner at MIT, was Theodore (Ted) Welton, and the two of them used to teach quantum mechanics and relativity to each other. Phillip Morse, who taught theoretical physics, gave a special course on quantum mechanics to Feynman, Welton and another student, Al Clogston (who was a year ahead of them), and assigned them actual problems. Feynman studied analytical chemistry, optics, and electricity and magnetism. Feynman and Welton took a graduate course on theoretical nuclear physics, which was taught by Morse, for which they used the material from the famous articles by H.A. Bethe (For.Mem.R.S. 1957) and R.F. Bacher in *Reviews of Modern Physics*. Near the senior year, Feynman did not have much to do; he had already taken all the necessary senior courses as well as graduate courses. So he took a course in metallography, about which he wanted to learn something; Feynman and Welton also took courses on X-rays. There were also courses in the humanities, but Feynman had little patience with them. He did take a course in philosophy from an old professor (Robinson), in which—surprisingly—he did very well. In another course in the humanities on the history of thought, the students were supposed to read Goethe's *Faust* and write about it. So he sat down and wrote a theme on 'the limitation of reason'; he developed his ideas about the limitations of the methods of science, and wrote up the theme; at the end of it he connected it with the views of Faust and Mephistopheles (Mehra 1994; see pp. 61–63).

Because Feynman had only a small scholarship at MIT, and his parents had worked very hard to send him there, he tried to augment his finances by working with a professor on research projects which were financed by a body called the National Youth Administration; for a while he worked in the laboratory of Professor Stockbarger, who sought to grow large single crystals of lithium fluoride and other alkali halides. He tried to obtain employment during the long summer vacations, and he even worked as the 'chief chemist' in a newly established business, the Metaplast Corporation, of his friend Bernard Walker from high school, who had started the enterprise for metal-plating plastics. Feynman was very successful at this work, but after the long vacation he left and the business collapsed. While Feynman was still an undergraduate student at MIT, he published two papers in *Physical Review*. One of these was with Manuel Sandoval Vallarta, who was interested in cosmic rays. Vallarta posed the following problem to Feynman: 'I want you to figure out that if cosmic rays were isotropic at infinity outside the galaxy, how the stars in the galaxy would change the distribution so that we would be able to know how much more we would be able to detect'. Feynman worked on it a bit and proved the theorem that if cosmic rays are isotropic outside the galaxy then they would be isotropic inside as well. Vallarta and Feynman published their note, entitled 'The scattering of cosmic rays by the stars of a galaxy' (1)*, which Vallarta wrote up. Feynman felt good about the proof he had found, although all he was proving was Liouville's theorem. Vallarta thought that this was very interesting, and it was the solution he had in mind. There was no effect due

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

to the scattering by the stars of the galaxy; the net effect was zero, the only assumption being that the stars do not absorb cosmic rays.

When the time came for Feynman to write a senior thesis as a requirement for graduation, he went to John Clarke Slater to get a problem. Slater told Feynman that quartz was a remarkable substance, because when you heat it up it does not expand too much and it is very stable, and he suggested that Feynman should work on this problem. As he recalled, 'My mind was wild about the problem, and I worked out the theory by myself, and it felt good to compute the properties of substances from fundamentals'. He wrote his senior thesis on 'Forces in molecules', and it was published in *Physical Review* (2). This paper contained an important result, a general quantum-mechanical theorem, that has had a fundamental role in theoretical chemistry and is frequently cited as the Hellmann–Feynman theorem (see Hellmann (1937); this reference was unknown to Feynman and Slater). As Feynman noted in the abstract, 'The force on a nucleus in an atomic system is shown to be just the classical electrostatic force that would be exerted on this nucleus by other nuclei and electrons' charge distribution'. He used quantum mechanics to calculate the charge distribution as the absolute square of the Schrödinger wavefunction, and it turned out to be an important result. (The history of the Hellmann–Feynman theorem is given in Musher (1966) and in Slater (1975).)

## Princeton University: the making of a theoretical physicist

John Slater was impressed by Feynman's work on his senior thesis. When the time came to choose a graduate school, Feynman wanted to stay on at MIT, but Slater would have none of it, suggesting that he should go to a different school for graduate work. At Princeton, Feynman was awarded a research assistantship, and it was suggested that he should work with Eugene Wigner (For.Mem.R.S. 1970), but when he got to Princeton it turned out that he had to work with John Archibald Wheeler (a young faculty member) (For.Mem.R.S. 1995) instead, which was fine with him. At Princeton, Feynman was installed in the Graduate College, where all the graduate students lived. It was an imitation Oxford or Cambridge college, complete with accents; Feynman was a bit daunted by its formality, which did not sit well with him. However, he was greatly looking forward to seeing the cyclotron, from whence many important new discoveries were always being reported, and he was directed to go down to the room at the end of the basement where the cyclotron was located; he understood immediately 'why so many new results were being reported from the Princeton cyclotron' and why John Slater had told him to go to another school for graduate work. The cyclotron was in the middle of the room and wires were hanging in the air all over the place, just strung up together. 'The whole place was completely different from MIT; it was a place where somebody was working!' Feynman realized immediately that this was a place for research; it had the atmosphere of a laboratory, with all the necessary tools lying around. For Feynman, 'the whole idealism that MIT was the greatest school collapsed, because I recognized in that room the same kind of atmosphere I had in my laboratory at home. I loved it and felt that I had come to the right place, and I realized right away that Slater had been right.'

When Feynman first met Wheeler he was very surprised to see how young he was and looked. Both of them immediately struck up a close and pleasant rapport. Princeton had no requirement about how many or what courses one had to take for working towards a PhD. One had, of course, to pass a preliminary or qualifying examination—which was partly written and partly oral—to become a candidate for the doctorate. These examinations were far-ranging and rigorous, and one had to prepare well for them in order to pass them. When Feynman had to take the written and oral examinations, he prepared for them by taking several weeks off from Princeton and going back to MIT, where he was now unknown and nobody bothered him; he could organize his knowledge of physics away from the bustle of activities in Princeton.

Because there was no compulsion to take this or that course, Feynman took only a few courses. Being Wheeler's assistant, he had to attend his course on nuclear physics and write the notes of his lectures. At Princeton, many people, foremost among them Eugene Wigner, were closer to the forefront of nuclear physics. Feynman also took Wigner's course on solid-state physics, from which he 'learned many things, including such deep questions as to why a solid is a solid'. Most of all, Feynman's graduate education was completed by intense and incessant discussions with fellow graduate students and by working on research problems. Sometimes he interacted with people at the Institute for Advanced Study; he talked to H.P. Robertson from time to time, and also went to see Albert Einstein, For.Mem.R.S., with John Wheeler once or twice to discuss some questions. Wheeler would give him the necessary problems, which arose in the course of his research work, and Feynman would solve them. Once Wheeler gave him a problem and he got stuck and found that he could not do it. So, for a change, he began to think about certain ideas that had occurred to him at MIT. 'At MIT, I had learned that quantum electrodynamics gave infinities, and so this was a problem I wanted to work on.'

Feynman had become aware that physical quantities that should be calculable by the theory, such as the self-mass of the electron (that is, the effect of the action of the electron's own electromagnetic field on its mass), were predicted to give an absurd infinite result. Feynman knew that a similar result was predicted classically, namely that the energy contained in the Coulomb field of a point charge is theoretically infinite. In his Nobel Lecture (35) he recalled that his 'general plan was first to solve the classical problem, to get rid of the infinite self-energies and hope that when I made a quantum theory of it, everything would be just fine'. The idea 'which [he] deeply fell in love with' was to replace the field itself by 'delayed action-at-a-distance'. According to this view, the electron would act only on *other charges*, not on itself, and the field would act as a useful invention for representing the delayed interaction. He would later abandon this idea after the precise experimental value had been obtained for the Lamb shift in hydrogen in 1947, which showed the presence of 'vacuum polarization', which could be obtained only by using the full field concept. Wheeler and Feynman had intense discussions with each other about these problems, and two papers emerged from their joint collaboration, but they were written by Wheeler. In the paper entitled 'Interaction with the absorber as a mechanism of radiation' (4), they introduced the absorber theory, according to which half the electromagnetic field propagates before the electron emitting it accelerates (advanced) and half as it accelerates (retarded). The advanced field was assumed to be absorbed in distant matter, where it would re-radiate and arrive at the accelerating electron at the right time and in the right amount to produce 'radiation reaction' that was needed to reduce the radiating electron's kinetic energy by the amount of energy that it radiates; there are no *observable* 'advanced effects'. As Wheeler pointed out to Feynman, that was the solution to the problem of lack of energy conservation that would result if the electron's radiated field did not act back upon the electron.

In their paper 'Classical electrodynamics in terms of direct interparticle interaction', which was published in 1949, at a time when Feynman was deeply involved in his work on quantum electrodynamics, which was a scholarly paper (8), they continued the critique of classical electrodynamics begun in the previous paper; it was based on the following idea: the field of a charge is determined by its motion; its field is sensed only by its action on other charges, whose motions act back upon the first charge. This would make it possible to eliminate the field and discuss directly how the motion of one charge affects the motion of another. This can be done by writing the relativistic expression for the principle of least action, which determines the equations of the charges (Fokker's action principle, which requires the use of half-advanced and half-retarded four-vector potentials, and leads to the paradox of advanced effects).

Feynman's paper 'A relativistic cutoff for classical electrodynamics' (6) was a further step towards modifying classical electrodynamics as a forerunner to attacking the problems of quantum electrodynamics, and Feynman pointed out that his modification of the classical 'point-like' interaction could be applied to conventional electrodynamics. In this paper Feynman also discussed the least-action solution to the problem of an electron striking a barrier and penetrating it—either directly, or indirectly by a process involving the production of a virtual positron–electron pair—and introduced the forerunner of the Feynman diagrams, containing an electron moving 'backward in time' to represent the positron.

I have referred to and discussed three of Feynman's papers ((4), (8) and (6)) out of sequence because they formed part of his pursuit of his MIT programme, but they were not actually completed in Princeton. In Princeton, Wheeler asked Feynman to give a colloquium on their joint work on the classical time-symmetric electrodynamics in the Physics Department at Princeton, and made arrangements for Feynman's colloquium with Eugene Wigner, who was at that time the chairman for arranging it. Wigner invited Henry Norris Russell, For.Mem.R.S., the great astronomer, John von Neumann, the famous mathematician, Wolfgang Pauli (For.Mem.R.S. 1953), who was spending the war years in Princeton, and Albert Einstein to attend Feynman's colloquium. Feynman felt very nervous about all these 'monster minds' coming to attend his lecture, but when everybody came in, he gave the lecture; according to prior arrangement, Wheeler answered the questions that were raised, and the event went off successfully. At the colloquium, Wheeler announced his intention to give a lecture on the quantum theory of the half-advanced, half-retarded potentials. On the way back to the Palmer Physical Laboratory, Feynman walked with Pauli, who asked him about what Wheeler was going to say about the quantum theory of the phenomenon, and Feynman told him that he did not know. 'Oh', Pauli said, 'the man works and doesn't tell his assistant what he is doing on the quantum theory!' He came closer to Feynman and said in a low, secretive voice, 'Wheeler will never give that seminar!', which turned out to be true.

Feynman had taken his qualifying examinations to become a candidate for a PhD in the fall of 1940. After passing the stiff qualifying examination, Feynman continued to work on his thesis, 'The principle of least action in quantum mechanics' (3), from September 1940 to the end of November 1941, and then postponed it after Robert R. Wilson recruited him for work on the atomic bomb project as a junior scientist at the Office of Scientific Research and Development (OSRD). The experimentalists at Princeton were busy building the apparatus right there to verify the theoretical ideas. After Feynman had been working on the atomic bomb project for a few months, he took six weeks off to finish his thesis; he quit the project to take his degree. At first he felt tired from his work at the atomic bomb project, and then could not turn his attention back intensely to something else. So, at first, he lazed around, but all of a sudden ideas began to come to him and he wrote them all up. Wheeler, who was then working at the Metallurgical Laboratory in Chicago with Eugene Wigner, also kept on insisting that Feynman had enough material for his thesis even if all the problems had not been solved and even if he never applied his theory to electrodynamics, which was the main purpose of it. At the commencement in about the middle of June 1942, Feynman received his PhD from Princeton University with his dissertation on the path-integral method.

The essential parts of his thesis were published in 1948 in his paper 'Space-time approach to non-relativistic quantum mechanics' (5). The Feynman path-integral approach to quantum mechanics was on a par with the formulations of M. Born, F.R.S., Werner Heisenberg (For.Mem.R.S. 1955) and P. Jordan, P.A.M. Dirac, F.R.S., and E. Schrödinger (For.Mem.R.S. 1949) of 1925–26. Feynman had developed the path-integral formulation of quantum mechanics to quantize the action-at-a-distance theory of the classical electron and thus to avoid problems arising in field theory from the self-interaction of the electron. The methods of quantization associated with the work of Heisenberg, Schrödinger and Dirac all began with the Hamiltonian function as the generator of the evolution of the system with time, whereas the classical action-at-a-distance theory employed a classical action principle based on the Lagrangian. This involved the interaction of two currents, each a function of an independent space-time variable. In his Nobel Lecture (36), Feynman described how he discovered (with the help of Herbert Jehle) an infinitesimal time development operator of Dirac involving the classical Lagrangian (Dirac 1933). Successive applications of this operator to the initial wavefunction generated the wavefunction at any later time, and the wavefunction was equivalent to finding the solution of the Schrödinger equation. To obtain the wavefunction after a *finite* time has elapsed, however, one had to integrate over all possible paths containing two arbitrary space-time points. This, in fact, was the path-integral approach of Feynman.

Feynman's book with Albert R. Hibbs (34) used path integrals to treat problems other than quantum mechanics and quantum electrodynamics, including statistical mechanics, the variational principle, the polaron problem, Brownian motion, and noise. Other applications have been made to quantum liquids and solids, to macromolecules and polymers, and to problems of propagation in dissipative media. Feynman's approach is important to various forms of semiclassical approximations in chemical, atomic, and nuclear problems and is fundamental to the instanton problem (barrier penetration between different vacuum ground states). It can be extended to optics and even to the motion of particles in the strong gravitational fields near a black hole. The path-integral formulation of quantum gauge theories that lie at the heart of the Standard Model of elementary particle interactions had a fundamental role in the Veltman–'t Hooft proof that these theories were renormalizable.

## Los Alamos

Richard Feynman had not yet completed his doctorate when, in April, 1942, he became a member of the group working with Robert R. Wilson on the electromagnetic separation of uranium-235 and uranium-238 by using the 'isotron', a device that the experimentalists at Princeton had developed for accelerating beams of ionized uranium, and trying to separate the isotopes by bunching them by applying a high-frequency voltage to a set of grids partway down a linear tube. He had been engaged upon this task since the previous December, when Wilson had invited him to join him in the OSRD project on the initial studies for building the atomic bomb. Towards the end of April, Feynman took several weeks off to complete his dissertation, receive his PhD and get married to his boyhood sweetheart Arline Greenbaum, after which he again returned to full-time work on war-related research.

Feynman recalled that 'one of the first interesting experiences I had in this project was meeting great men. I had not met very many great men before.' There, in the evaluation committee that he was invited to attend, he met the regular members: A.H. Compton, Karl Compton, R.C. Tolman, Harry Smyth, Harold Urey (For.Mem.R.S. 1947), I.I. Rabi and J. Robert Oppenheimer, with Tolman as Chairman. He was astonished to discover how smoothly this committee functioned, without having anything repeated. It was ultimately decided that the project in Princeton would not be the one that was going to separate uranium isotopes, and that they would actually be starting the project to build the atomic bomb at Los Alamos, New Mexico, and all of them from Princeton would go there.

At Los Alamos the theoretical physicists started their work immediately. Feynman worked very hard in those days, studying, reading and calculating. As he recalled,

> All the big shots except Hans Bethe [who was the Head of the Theory Division] happened to be away at the time, and what Bethe needed was someone to talk to, to push his ideas against. Well, he came to me in an office and started to argue, explaining his idea, and I said, 'No, no, you're crazy. It'll go like this.' And he said, 'Just a moment,' and explained to me how he's not crazy. I was crazy. And we kept on going like this. But it turned out to be exactly what he needed. I got a notch up on account of that and I ended up as a group leader [of the Technical Computations Group] under Bethe with four guys under me [Julius Ashkin, Frederick Reines, Richard Ehrlich and Theodore Welton].

It was at this encounter that Richard Feynman first met Hans Bethe. Feynman was deeply impressed by Bethe's analytical powers, physical intuition, stamina and erudition, his unaffected and forthright simplicity of manner and, above all, his integrity. Bethe and Feynman developed a profound mutual admiration and affection.

At Los Alamos (the atomic bomb laboratory, of which Oppenheimer was the Director), Feynman had many diversions apart from hard and intense work on the project. For example, he had great fun in fooling the censor officials who would scrutinize the outgoing and incoming mail and, in general, guard the security of the premises of the laboratory. At every turn, Feynman would seek to outwit them. He mastered the technique of picking locks and opening safes, by inferring the lock combination numbers in an uncanny fashion, and leaving behind notes for his victims that their security had been breached. For physicists he had a hunch that for the combinations of their locks they would use the numerical values of e, the base of the natural logarithms, or the value of $\pi$. Feynman would try various combinations of the six-digit numbers and often the combinations would work. In this way he had actually access to all the secrets of the atomic bomb at his disposal.

Before Feynman took charge of the Theoretical Computations Group, the people who were doing the various steps did not know what all this work was for. Then Feynman obtained permission from Oppenheimer to explain to the crew the goal of the project—how important and necessary their contribution was for war work—and he gave them a lecture about what was being done and what it was for. After that the technical people worked with great excitement. There was a complete transformation: they did not need much supervising any more, and would even work at night and in overtime.

At weekends, Feynman would visit his wife Arline at her clinic in Albuquerque, where she was laid up with tuberculosis; at times, some of his friends would visit her, too, and they would all try to cheer her up. Feynman went to see her mostly alone, either taking a bus or hitching rides. Then Arline died in Albuquerque, and Feynman had to get down there, for which he borrowed his colleague Klaus Fuchs's car. When he returned after three days he found that his group was extremely busy, fully occupied with the problem of juggling index cards; they had found some means of making corrections in the calculation that had been done wrongly earlier. The calculations were very elaborate and difficult to determine how much energy would be released from various designs of the bomb, and then from the specific designs that were going to be used in the actual bombs, and how much material was needed in each case. It was a big problem and the time allowed for computation was too short. Feynman's leadership of the Technical Computations Group was universally admired.

Apart from his great love and admiration for Hans Bethe, which the latter fully reciprocated, Feynman had other notable encounters and influences at Los Alamos. There was the great mathematician John von Neumann. Feynman would go for walks on Sundays with Bethe, von Neumann and Robert Bacher. As Feynman recalled, 'von Neumann gave me an interesting idea: that you don't have to be responsible for the world you are in. So I have developed a very powerful sense of social irresponsibility as a result of von Neumann's advice, which has made me a very happy man ever since; it was von Neumann who put in the seed that grew into my *active* irresponsibility.'

At Los Alamos, Feynman also met Niels Bohr, For.Mem.R.S. Feynman concluded that, even to the 'big guys', 'Bohr was a great god'. At meetings, Bohr had observed that Feynman always spoke his mind, without being weighed down by the importance and authority of the people present. So, the next time Bohr came to attend an important meeting about the bomb at Los Alamos, he decided to talk to Feynman about his ideas before calling in other people.

After Arline's death, Feynman spent a few days of rest and vacation at home in Far Rockaway, where he received a message from Hans Bethe that said, 'The baby is expected on such and such a day'. Feynman immediately flew back to New Mexico, and arrived just in time, as the buses were leaving for the test site. There he waited at a distance of 20 miles, while others waited six miles away. They had been issued with dark glasses to witness the sight of the explosion of the atomic bomb, but Feynman knew that the only thing that could hurt the eyes was ultraviolet radiation, and he took a place behind the windscreen of a lorry; ultraviolet light would be absorbed by the glass of the windshield. Then the explosion took place and Feynman saw it:

> A tremendous flash out there…, white light changing into yellow and then into orange. The clouds formed and disappeared again; the compression and expansion formed and made the clouds disappear. I am about the only guy who actually looked at the damned thing—the first Trinity test. Everybody else had dark glasses, and people at six miles couldn't see it because they were told to lie on the floor. Finally, after about a minute and a half, there was a sudden tremendous noise—BANG, and then a remarkable thunder.

After the bomb went off and the test had been successfully completed, there was huge excitement at Los Alamos. All of them ran around, everybody had parties. Feynman sat on the hood of a jeep and played drums by beating metal rubbish bins. But one man, Robert Wilson, was just sitting around and moping. Feynman asked him, 'What are you moping about?' He replied, 'It's a terrible thing that we made', to which Feynman replied, 'But you started it. You got us into it.'

From Los Alamos, Feynman went to teach at Cornell University, where Hans Bethe had already arranged an appointment for him as an assistant professor. His first impressions after returning to normal life were very strange. For instance, he sat in a restaurant in New York and started to look at the buildings; he began to think about how large the radius of the Hiroshima bomb damage was and the consequences of this. All those buildings, all smashed and so forth.

As he walked along and saw people building a bridge or a new road, he would think that they were *crazy*, that they just did not *understand*. 'Why are they building new things? It's so useless. But, fortunately, it has been useless for [so many] years, hasn't it? So I have been wrong about it, being wrong to build bridges and roads, and I'm glad that those other people had the sense to go ahead.'

## Cornell University, path-integral method, and quantum electrodynamics

On 30 October 1943, a few months after Feynman had arrived at Los Alamos, Hans Bethe had already proposed to R.C. Gibbs, Chairman of the Physics Department at Cornell University, and recommended in the highest terms that Feynman be hired at Cornell. Robert F. Bacher, who also worked in the Physics Department at Cornell and was then at Los Alamos, also wrote to Gibbs supporting Feynman's appointment. Gibbs acted on their proposals favourably; all that Feynman had to do was to send him a brief (one-page) note about his background. This he did, and he was appointed. His appointment at Cornell finally began on 1 November, but he arrived in Ithaca on 31 October. Feynman had received offers from other prestigious universities as well, but he wanted to be close to Bethe and was glad to be at Cornell. In his first year he was asked to teach a course on the mathematical methods of physics and another one on electricity and magnetism, which he started to prepare on the train to Ithaca. Upon arrival there, he faced the problem of finding hotel accommodation, which was not to be had at all, and he had to spend the night on a couch in the lobby of the Willard Straight Hall. However, the next morning he showed up at the Physics Department office in Rockefeller Hall, and learned that he had been asked to come to Ithaca one week before the classes started so as to give him time to get settled, which he did at the Telluride House.

With his wife Arline dead, he was a bachelor again; he looked very young and could pass for a freshman at the university. At the dances he attended, the girls would not believe a word about his background that he had worked at the Manhattan Project—and thought that he was a phoney and a faker, so he gave it up and began to attend to preparing and teaching his courses. Other than teaching his classes, he would rest. He was burned out from war work at Los Alamos. He would go to the library and sit there for hours reading *Arabian nights*. He found that preparing his courses was a full-time job, and he devoted himself fully to that. He did not believe that it was very important work, and he thought he should be doing research. The more he taught courses and stayed away from research, the more depressed he became. He believed that he was completely burned out and would not accomplish anything any more.

Feynman continued to get increases in salary. He was still in demand from other places outside; they wanted to lure him away to departments in other universities. When another place offered him more money and would invite him to take a new job, he would just refuse it, but the departmental secretary knew what was going on and she would inform the Chairman, and Feynman would receive increases in salary that were intended to keep him at Cornell. Most attractive of all, he was informed by H.D. Smyth from Princeton that the university and the Institute for Advanced Study would like to offer him a joint position at a substantial salary, making it possible for him to spend half the time at the university and the other half at the Institute (where Albert Einstein was). From Princeton, Eugene Wigner congratulated Feynman and expressed the hope that he would accept. The offer from Princeton resulted in his being promoted to an associate professorship at Cornell. He received other offers from the University of California at Los Angeles (UCLA) and Berkeley, which he routinely refused. To Feynman, given his psychological situation, all these invitations seemed so strange, so mistaken, that he concluded that they must be crazy to think that he was worth all that, and that he did not have to live up to 'other people's idiotic impression of my abilities'.

Then, one day, something interesting happened. Feynman was in the students' cafeteria for lunch. While he was eating and watching, he noticed that some guy, fooling around, threw up a plate in the air; the plate went up and then started to come down. The plate had emblazoned upon it the emblem of Ezra Cornell, the founder of Cornell University. 'As the plate went up in the air, I saw it wobble. It was pretty obvious to me that the medallion went around faster than the wobbling. I began to figure out the motion of the rotating plate, and discovered that when the angle [of the wobble with the horizontal] is [very small], the medallion rotates twice as fast as the wobble rate—two to one. It's a cute relationship.' Feynman worked out the equation of motion of the rotating–wobbling plate, which was fun for him. This episode rekindled his love of 'playing around with physics'. Then he thought about the problem of rotations of the spinning electron in relativity, of how to represent them by path integrals in quantum mechanics, and worked on the Dirac equation in electrodynamics. And before he knew it, he was playing around with the same problem that he had loved so much, which he had stopped working on when he went to Los Alamos: his thesis type of problems. 'It was like uncorking a bottle, everything flowed out effortlessly. I almost tried to resist it! There was no importance to what I was doing, but ultimately there was. The diagrams that I got the Nobel Prize for came out of piddling around with the wobbling plate.' Feynman again became interested in physics as play.

His father, Melville Feynman, suffered a stroke on 7 October 1946 and died the next day. Richard signed his father's death certificate, his second in less than two years—the first had been upon the death of Arline in the summer of 1945. Melville was interred at the Bayside Cemetery in Queens.

Feynman became seriously involved in the space-time (path-integral) approach to non-relativistic quantum mechanics and other problems, and in summer 1947 he completed his third way of formulating quantum mechanics, which was based on the new physical interpretation of the method that he had developed in his thesis (3), and which has already been discussed (5). Soon after completing the latter paper, Feynman moved on to develop his space-time approach to quantum electrodynamics.

The main new initiative for his work was the Shelter Island Conference, which took place on 2–4 June 1947 at Ram's Head Inn on Shelter Island, at the tip of Long Island, and the general theme of the conference was 'Problems of quantum mechanics and the electron'. At this conference, Willis E. Lamb, Jr, presented the result of his precise measurement of what came to be called the 'Lamb shift', and I.I. Rabi reported on the measurement of the anomalous magnetic moment of the electron by Polykarp Kusch and his collaborators at Columbia University. Both of these measurements indicated the small but profound discrepancy between these precise experimental results and Dirac's relativistic theory of the electron. Immediately after the Shelter Island Conference, Hans Bethe made his famous non-relativistic calculation of the Lamb shift (Bethe 1947), which greatly inspired Feynman, who became absorbed in developing the relativistic theory and outlined his views at the Pocono Conference, which took place on 30 March to 1 April 1948.

Feynman's unconventional approach to quantum electrodynamics did not convince those present at the meeting, and he was roundly criticized for it. In fact, he had too much material, and his methods were unique and original; he decided that it would be best if he published his results first. He began this process by publishing his paper 'Relativistic cut-off for quantum electrodynamics' (7), which included the quantum-mechanical version of the relativistic high-frequency cut-off that he had already introduced for classical electrodynamics (6). This method, which would be called 'regularization', made the divergent integrals of quantum electrodynamics finite, other than the vacuum polarization integrals, as was shown by Pauli & Villars (1949). By using the 'old-fashioned' perturbation theory of Dirac, Feynman showed that his cut-off procedure led to the same results for the electromagnetic shift of energy levels of hydrogen (Lamb shift) and radiative corrections to potential scattering as had been published by others, including Bethe, J. Schwinger and V.S. Weisskopf. However, Feynman's results avoided the subtraction of infinite integrals, using only finite ones that he showed were very insensitive to the value chosen for the cut-off.

Feynman's substantial papers on quantum electrodynamics appeared in 1949. These were 'The theory of positrons' (9), received by *Physical Review* on 8 April 1949, and 'Space-time approach to quantum electrodynamics' (10), received a month later. The validity of the rules given in these two papers was demonstrated in a third paper, 'Mathematical formulation of the quantum theory of electromagnetic interaction' (11), which arrived at *Physical Review* on 8 June 1950. In the first of these papers, Feynman discussed the behaviour of positrons and electrons in given external potentials, neglecting their mutual interaction by replacing the theory of holes by a reinterpretation of the solution of the Dirac equation. He wrote down the complete solution of the problem in terms of the boundary conditions on the wavefunction, and this solution automatically contained all the possibilities of virtual (and real) pair formation and annihilation together with the ordinary scattering processes, including the correct relative signs of the various terms.

In this solution, the 'negative energy states' appeared in a form that could be pictured (as had been done earlier by Stückelberg) in space-time as waves travelling away from the external potential backwards in time. Experimentally, such a wave corresponded to a positron approaching the potential and annihilating the electron. A particle moving forwards in time (electron) in a potential may be scattered forwards in time (ordinary scattering) or backwards (pair annihilation). When moving backwards (positron) it may be scattered backwards in time (positron scattering) or forwards (pair production). For such a particle the amplitude for transition from an initial to a final state was analysed to any order in the potential by Feynman by considering it to undergo a sequence of such scatterings.

The amplitude for a process involving many such particles is the product of the transition amplitudes for each particle. The exclusion principle requires that antisymmetric combinations of amplitudes be chosen for those complete processes that differ only by the exchange of particles. It seemed that a consistent interpretation was possible only if the exclusion principle were adopted. The exclusion principle did not need to be taken into account in intermediate states. Vacuum problems did not arise for charges that did not interact with one another, but Feynman analysed them anyway in anticipation of an application to quantum electrodynamics. He also expressed the results in momentum–energy variables, and in an appendix he proved the equivalence to the second quantization theory of holes.

Quantum electrodynamics proper was the subject of the second paper, 'Space-time approach to quantum electrodynamics' (10). In this paper, Feynman did two things. First, he showed that a considerable simplification could be attained by writing down matrix elements for complex processes in electrodynamics. Further, a physical point of view was available to

Feynman that permitted him to write them down for any specific problem. As this was simply a restatement of conventional electrodynamics, however, the matrix elements diverged for complex processes. Second, he showed that electrodynamics is modified by altering the interaction of electrons at short distances. All matrix elements were now finite, with the exception of those relating to problems of vacuum polarization. Feynman evaluated the latter in a manner suggested by Pauli and Bethe, and obtained finite results for these matrices too. He found that the only effects sensitive to the modification were changes in mass and charge of the electrons. Such changes could not be observed directly. Phenomena directly observable are insensitive to details of the modification used (except at extreme energies). For such phenomena, a limit could be taken as the range of modification went to zero. Feynman found that his results then agreed with those of Schwinger, and he showed that a complete, unambiguous and consistent method was therefore available for the calculation of all processes involving electrons and photons. Feynman then gave the famous Feynman rules and Feynman diagrams (10).

The next paper (11) was designed to justify the space-time procedure given in the previous papers and to supply the 'proof of equivalence of these results to conventional electrodynamics'. In fact, the first four sections of this paper had been written in 1947, much of which duplicated the work of Feynman's thesis (7). It was followed a year later by 'An operator calculus having applications in quantum electrodynamics' (12), which was completed while Feynman was on leave of absence in Brazil before taking up his appointment as a professor of theoretical physics at the California Institute of Technology (Caltech). As he remarked later, 'With this paper I had completed the project on quantum electrodynamics. I didn't have anything remaining that required publishing. In these two papers [(11, 12)], I put everything I had done and thought should be published on the subject. And that was the end of my published work in the field.' An advantage of the ordered operator calculus over the path-integral method was that his new method of dealing with operators would have a wider application, but that—at least so far—has not turned out to be so.*

In a paper with Laurie M. Brown on 'Radiative correction to Compton scattering' (13), the authors calculated the radiation corrections to the Klein–Nishina formula for Compton scattering, which proved to be an appropriate testing ground for quantum electrodynamics as well as being an important observable effect at higher energies.

In a review paper in 1961, 'The present status of quantum electrodynamics' (26), Feynman stated his 'long-held strong prejudice that [quantum electrodynamics] *must* fail significantly (other than being incomplete) at around 1 GeV virtual energy'. He further remarked, 'I shall hold this belief, and not subscribe to the philosophy of renormalization'.

After the completion of his fundamental papers on quantum electrodynamics (9–12), Feynman left the field in triumph, but personally he always remained dissatisfied, as he expressed at the 1961 Solvay Conference. He thought that he would solve the problem of divergences in the theory, that he would 'fix' the problem, but he did not. He believed that he had invented a better way to make the calculations, but had not succeeded. He had kept the relativistic invariance under control and everything was nice, but he had not fixed anything: 'I was not satisfied at all'. Feynman was also disappointed that his space-time picture of electrodynamics was not really new: it was, in fact, equivalent to the conventional field theory of

* Feynman's work on quantum electrodynamics was very extensive. See Mehra (1994), chapters 4–6 and 10–15. Laurie M. Brown has given brief commentaries on groups of Feynman's papers, and is referred to in the acknowledgements to this memoir.

Schwinger and Tomonaga. He had hoped to eliminate fields entirely as fundamental entities in favour of particles, but field theory had triumphed in the end.

## From Cornell to Caltech and new ventures

What bothered Feynman the most at Cornell, in upstate New York, were the weather and the fact that while humanities, such as history, philosophy, literature, psychology and sociology, were fully represented at the university, so were animal husbandry, home economics and hotel management, which he did not believe were intellectual pursuits. In the autumn of 1949, Robert F. Bacher—who, with Hans Bethe, had been instrumental in bringing Feynman and had meanwhile gone to Caltech as Head of the Division of Physical Sciences and Director of the Norman Bridge Laboratory of Physics—invited Feynman to give a series of lectures on quantum electrodynamics, meson theory and nuclear forces. Feynman greatly enjoyed his stay at Caltech. Bacher discussed with him the possibility of a permanent move to Caltech, and offered him sufficient inducements to leave Cornell to come to California. Above all else, two things attracted Feynman at the prospect of such a move: first, of course, was the pleasant climate; second, but more important, was the character of Caltech as an institute of technology. There were many science departments with really active people, and they had the same way of thinking as Feynman had. He fell in love with Caltech and decided that it was 'my kind of place'. Actually, his close association with and fondness for Bethe made his choice to leave Cornell for Caltech very difficult, but Bacher persuaded Bethe that this was the best course for Feynman. Soon after his return to Cornell from Caltech in the spring of 1950, Bacher offered Feynman a professorship of theoretical physics. At Cornell, Feynman was entitled to a sabbatical leave during the academic year 1951–52, and Caltech offered to give it to him at their expense in the beginning of his duties there.

From 24 to 30 April 1950 there took place the International Colloquium on Fundamental Particles and Nuclei in Paris, to which Feynman was invited, and he was asked to give a report on the current status of the theories of elementary particles at the opening day of the conference. Almost all the great names in modern particle physics and quantum field theory were present at the Paris Conference, including Bohr, Dirac, Pauli, Hendrik Kramers, Rudolf (later Sir Rudolf) Peierls, F.R.S., Markus Fierz and Walter Heitler, F.R.S. Pauli, whom Feynman had known since his Princeton days, was deeply impressed with him and his work, and he invited Feynman to visit Zurich for a few days after the conference in Paris. In Zurich, Feynman addressed the joint University–ETH (Federal Institute of Technology) colloquium and gave several other lectures on his path-integral formulation of quantum mechanics and on his work on quantum electrodynamics. This was Feynman's first visit to Europe and he enjoyed it thoroughly; he would later return to Europe time and again, for both business and pleasure.

Feynman had arranged with Robert Bacher that his appointment at Caltech for the academic year 1950–51 would begin on 1 July; it was also arranged that he would stay at the Athenaeum, Caltech's faculty club, as a long-term guest for the entire year. Before joining Caltech, Feynman spent a sabbatical year at the Centro Brasiliero de Pasquisas Fisicas at Rio de Janeiro in Brazil and learned to speak Portuguese. He greatly enjoyed his stay in Rio, where he taught his classes in Portuguese, and fell in love with the local music, especially at Carnival time. Because he was not able to form any personal attachments in Brazil, Feynman felt very lonely; recalling his friendship with Mary Louise Bell (Mary Lou), whom he had known at

Cornell, where they had often had unpleasant arguments, he now reflected on those times with nostalgia and proposed marriage to her by letter, which she accepted. After a stay of 10 months in Brazil, Feynman decided to return to California and decided that he would stay there permanently. Soon after his return from Brazil, he married Mary Lou in June 1952; however, things did not work out between them and the marriage lasted only a few years until they were divorced in 1956.

By the summer of 1953, Feynman had already been thinking almost constantly about the problem of the superfluidity of liquid helium for a couple of years. During his stay in Rio de Janeiro in 1951–52, several ideas came to him concerning the solution of various aspects of the problem. In September 1953 he went to Japan to attend the International Conference on Theoretical Physics, which took place partly in Tokyo and partly in Kyoto, where he gave a lecture on the atomic theory of liquid helium and had his first encounter with Lars Onsager (For.Mem.R.S. 1975). Upon his return to Caltech, Feynman continued to work on the theory of the superfluidity of liquid helium as well as the problem of superconductivity.

## Liquid helium and other problems

Between 1953 and 1958, Feynman published numerous papers on the atomic theory of superfluid helium. A significant part of Feynman's central contribution was the demonstration that the phenomenological concepts arose directly from the fundamental quantum mechanics of interacting bosonic systems with strong repulsive cores. In his first substantial paper on liquid helium, dealing with the 'Atomic theory of the lambda transition in liquid helium' (14), Feynman showed in detail how the symmetric character of the many-body wavefunction severely restricts the allowed class of low-lying excited states, which he established in detail in his paper 'Atomic theory of liquid helium near absolute zero' (15). In the subsequent series of four papers, Feynman discussed all the details of his various conceptions concerning the superfluidity of liquid helium (16, 17, 20, 23); the last two of these papers were authored jointly with Michael Cohen, who wrote his doctoral thesis on this problem with Feynman. Cohen and Feynman (23) used their trial function to study the inelastic neutron scattering from superfluid helium, predicting that the scattering would be dominated by the excitation of a single quasiparticle, allowing a direct measurement of the energy spectrum; subsequent measurements fully confirmed this behaviour. (For details of the entire history of the problem of superfluidity and Feynman's extensive work on superfluid helium, see Mehra (1994), chapter 17.)

As mentioned earlier, among the applications of the path-integral method treated by Feynman, he developed a new variational principle and applied it to the polaron problem, in which he had become accidentally interested by reading a paper by Herbert Fröhlich, F.R.S. As Feynman noted, 'An electron in an ionic crystal polarizes the lattice in its neighborhood. This interaction changes the energy of the electron. Furthermore, when the electron moves the polarization state must move with it. An electron moving with its accompanying distortion of the lattice has sometimes been called a polaron. It has an effective mass higher than that of an electron. We wish to compute the effective mass of such an electron' (18). From this work on the problem of the polaron, 'Slow electrons in a polar crystal' (18), Fröhlich had conveyed to Feynman that the next step would be the solution of the problem of superconductivity. Feynman used his variational principle to obtain a lower bound for the polaron's self-energy that was lower than that obtained in five other papers published in the 1950s. In his paper with Robert

W. Hellwarth and two Caltech graduate students, Carl K. Iddings and Phillip M. Platzman, Feynman employed a similar method to calculate the polaron's mobility (28), but whatever he tried Feynman was not led to the correct formulation of the problem of superconductivity.

Feynman's paper with F.L. Vernon (which became Vernon's doctoral thesis) dealt with a general quantum-mechanical system (such as an atom or molecule) interacting with a linear dissipative system, represented by a collection of harmonic oscillators. The oscillator coordinates could be integrated out so that their total effect was replaced by that of an 'influence functional'. In addition to the fluctuation–dissipation theorem, they derived the 5relating temperature and dissipation of the linear system to a fluctuating classical potential acting on the system of interest, which reduced to the Nyquist–Johnson relation for electric circuits. Feynman did not always publish papers with his graduate students, but in this paper with Vernon, 'The theory of a general quantum-mechanical system interacting with a linear dissipative system' (30), and just a few others he agreed to do so.

During his sabbatical year, 1959–60, Feynman devoted himself to an active pursuit of his interest in biological problems, which had attracted him since the time he arrived at Caltech, and began to interact with Max Delbrück (For.Mem.R.S. 1967) and other members of the biology department. When he decided to devote himself to the pursuit of biological problems for a while, he mentioned it to Delbrück, who sent him to see Robert S. Edgar, who at that time was a postdoctoral fellow responsible for bacteriophage research in Delbrück's laboratory. Edgar advised Feynman to work like an active graduate student, and formally assigned him a problem to work on back-mutations and on ribosomes. His paper 'Mapping experiments with *r* mutants of bacteriophage T4D1', published with his colleagues (29), was very successful. Feynman also worked on ribosomes with Matt Meselson (For.Mem.R.S. 1984), but this work did not go very well and he was not able to reproduce his results; however, he was extremely successful as a 'graduate teaching assistant' and all the graduate students liked him.

## 'The only law of nature I could lay a claim to': the theory of weak interactions

Since Henri Becquerel's discovery of radioactivity in 1896, before the discovery of the electron, the proton or the atomic nucleus, the weak interactions were observed and studied in the form of beta-decay. Enrico Fermi (For.Mem.R.S. 1950) formulated a theory of this process at the end of 1933, just after the seventh Solvay Conference on Physics at which Pauli talked about his conception of the neutrino and it gave a good measure of agreement with experiment. Like quantum electrodynamics and the atomic and nuclear theories of the time, Fermi's four-fermion weak interaction theory obeyed the left–right symmetry known as parity (which, when switching from a left-handed to a right-handed spatial coordinate system, left the physics unchanged). It was a great shock, therefore, when a suspected violation of parity in weak interactions (such as beta-decay and particle decays) was experimentally confirmed in 1957 that led in a short interval to theories and experiments that became known as the 'parity revolution'. Feynman had a very important role in that revolution by publicly questioning the necessity for the left–right symmetry at the 1956 Rochester Conference, at which he repeated a question asked of him by Martin Block about whether parity might be violated in the decay of the K-meson. (For all the relevant details of Feynman's work on the V–A theory of weak interactions, see Mehra (1994), chapter 21, as well as Laurie M. Brown's brief commentary.)

At the 1957 Rochester Conference, Feynman informally suggested an early version of the vector–axial vector (V–A) theory of the weak interaction. Soon thereafter, he worked out the details of the V–A theory, and felt very happy with the thought that this 'was the only law of nature I could lay a claim to'; however, a few others had also worked on the theory of weak interactions, including Murray Gell-Mann (For.Mem.R.S. 1978), Robert E. Marshak and E.C.G. Sudarshan, as well as J.J. Sakurai. Feynman and Gell-Mann co-authored a paper on the V–A interaction (24), which led to controversy about its collaboration, priority and other questions. However, in comparison with the rival work, the contribution of Feynman and Gell-Mann uniquely emphasized certain features: the heuristic value of the two-component relativistic electron equation of the second order in time, current–current interaction possibly mediated by heavy intermediate bosons, and the conserved vector current.

## PARTONS—QUARKS AND GLUONS

After his work on the V–A theory of weak interactions, Feynman surveyed the situation in the physics of strong interactions (structure of nucleons) and found it to be too complex; not enough experimental data were available. The quantum theory of gravitation and the *Feynman lectures on Physics* (32) fully occupied him for a long time, and it was only after receiving the Nobel Prize for Physics in 1965 (which he shared with Julian Schwinger and Sin-Itiro Tomonaga) that he was able to turn his attention to high-energy physics. At that time, the theoretical situation in physics abounded with approaches to the problem of strong interactions: dispersion relations, Regge asymptotics, current algebra, the S-matrix approach, nuclear democracy, bootstrap theory and, of course, symmetries. Feynman thought that 'there was an awful lot of theory in those days'.

In the spring of 1968, Feynman began by examining the collisions of two hadrons—say two protons or a pion and a proton—at high energies. He believed that at high collision energy the dynamical details of the strong force between them might become quite simple and amenable to calculation. Because of their high relative velocity, each proton sees the other as relativistically contracted along the direction of motion to a flat disc or pancake. In addition, because the strong interactions are of short range, the two flat discs would only have a very short time to interact with each other. Feynman began to think of each hadron as a collection of small parts, of unspecified quantum numbers, which he christened 'partons'. He thought of partons as arbitrary, 'bare', ideal particles—the 'quanta of some underlying field, their exact number in any hadron being indeterminate'. Feynman worked out the consequences of the parton model and looked for evidence of its validity in the scanty data then available on high-energy proton and pion–proton collisions. During the second week of August 1968, Feynman went to the Stanford Linear Accelerator Center (SLAC), where he learned about the experiments on deep inelastic lepton–nucleon scattering that were designed to probe the structure of hadrons; they showed that the hadron is not a point-like object (particle) but has a finite size, and the results of the collision would be interpreted as the scattering of the lepton by a constituent. Feynman's parton model bore out and explained the results of the SLAC experiments.

At Caltech, Murray Gell-Mann and George Zweig had proposed the existence of fractionally charged hadronic constitutents which they, respectively, called 'quarks' and 'aces'. Partons quickly became identified with quarks by most physicists (33, 37–39, 43, 44). Later (45, 46), the language of partons was replaced by that of quarks and gluons. The theory that is used is quantum chromodynamics (QCD), and the fundamental collision processes included not only quark scattering, but also quark–gluon, and even gluon–gluon, scattering.

In the mid-1970s Richard Feynman became interested in a phenomenon known as 'quark jets'. In a paper with Rick D. Field (46) and another with Field and Geoffey C. Fox (45), he published theoretical studies of the production of quark and gluon jets, made before the accumulation of relevant data, and these were verified by the experimental studies both at the SLAC and DESY. These jets arise from cascade processes in which each quark and gluon produced in a collision turns into a rapid stream (i.e. a jet) of hadrons. The analyses of Feynman, Field and Fox exploited the important property of QCD called asymptotic freedom (the vanishing of interaction between quarks having high relative momentum).

## The quantum theory of gravitation

In January 1957 Richard Feynman attended one of the first conferences organized to discuss the role of gravitation in physics. 'I knew that gravity must be part of nature and nature can't be half classical and half quantum mechanical.' It needed to be verified that the general theory of relativity must be subject to the same rules as every other 'classical' theory. It had to be shown that otherwise there would be inconsistencies in the laws of nature. In several lectures on gravitation, Feynman gave lengthy reasons why he thought there must be quantum corrections to the gravitational force, even though the gravitational phenomena all took place at the macroscopic scale (51).

In the autumn of 1960 Victor Weisskopf asked Feynman a question about the radiation of gravitational waves. On 11 February 1961, Feynman gave Weisskopf a detailed answer and discussed gravitational radiation and the lowest order of quantum description; he patiently explained to Weisskopf how to calculate gravitational waves, that he thought they must be quantized just like electromagnetic waves have to be, and that '*gravitons*' (quanta of the gravitational field), particles with spin 2, must be real physical particles carrying real, physical energy like all the others.

In the last week of July 1962, Feynman attended the international conference on relativity and gravitation in Warsaw, Poland, where he gave lectures on the quantum theory of gravitation (31). There he began with many of the same things he had explained to Weisskopf over a year before the conference. He still felt the need to justify his interest in the subject along lines that would not be popular today; for instance he claimed that it must be possible in principle to compute such a thing as the Lamb shift correction to the energy levels of an atom held together exclusively by the gravitational force. Apparently the argument satisfied him more than the idea that at very high energies the gravitational force would eventually dominate over all other forces.

In his papers (31, 40, 41), Feynman addressed the question of renormalizing quantum gravity. He considered the quantum gravity questions by using the tools of quantum field theory, many of which he had himself developed. Feynman started by computing tree diagrams (i.e. Feynman diagrams without closed loops) of the theory (which are equivalent to classical theory) and showed that they were gauge-invariant. However, he then looked at some of the loop diagrams and discovered that they were not gauge-invariant; that started the long saga of the 'ghosts', which most people associate with the names of L.D. Faddeev and V.N. Popov, who derived the extra terms that must be added to the Lagrangian by considering a full path-integral quantization, but all this goes back to Feynman. Feynman made use of the Yang–Mills theory to obtain the corresponding expressions for gravitation (41). Although Feynman did not succeed in going beyond the level of one-loop diagrams, his methods, including 'ghost-particle' loops and path integrals, were indispensable for the eventual success of this endeavour. Of his effort to obtain a consistent quantum theory of gravitation, Feynman remarked: 'I can't say that I'm seeking the Holy Grail; there may not be a Holy Grail [of one 'unified' theory]. What I'm trying to do is to find out more and understand more about nature, and what I'll find—I have no preconception about what nature is like or ought to be!'

## The fundamental limits of computation

Richard Feynman had been interested in the problem of computing and the nature of computers since his earliest days in high school. At Princeton he had the opportunity of learning about what John von Neumann was doing with computers, but at the same time Feynman himself became involved in developing a mechanical director for shooting down aeroplanes at the Frankfort Arsenal in Pennsylvania in the summer of 1941, and did not take any direct interest in what von Neumann was doing. At Los Alamos, as leader of the Technical Computations Group in the Theoretical Division under Hans Bethe, Feynman used Marchant and Monroe calculators and learned how to repair them. Later, when computers started to come out, he did not do a great deal with them but played around. During the last 10 years of his life he became fascinated by the theory and application of computers and he gave a joint course in computation at Caltech, together with his colleagues John Hopfield and Carver Mead, in which they also invited guest lecturers to talk about special topics. Feynman also published several papers on computers (47–50), of which two (48, 50) were the same.

Of these papers, the most important was (47), in which Feynman proposed the idea of a quantum computer and discussed the limitations on computers imposed by the laws of physics. In some of his ideas, Feynman had been paralleled (and sometimes anticipated) independently by others, especially by Paul Benioff and Rolf Landauer. Feynman discussed the reversible computer (48), first considered by C.H. Bennet, and the limitations that could arise from the second law of thermodynamics (the increase of entropy with time). However, he concluded, 'it seems that the laws of physics present no barrier to reducing the size of the computers until bits are of the size of the atoms, and quantum behavior holds sway'.

Paper (49) was a lecture delivered in Japan in 1985 as a memorial to Yoshio Nishina, the real founder of the Japanese school of theoretical physics, in which such luminaries as Hideki Yukawa, For.Mem.R.S., and Sin-Itiro Tomonaga were trained, both of whom received the Nobel Prize for Physics. It was a 'popular' presentation in which Feynman discussed parallel computation and the possibilities of reducing energy and computation size.

## 'There's plenty of room at the bottom'

On 29 December 1959, at the Annual Meeting of the American Physical Society, held at Caltech, Richard Feynman gave a talk entitled 'There's plenty of room at the bottom' (25). In this talk Feynman sought to 'describe a field in which little has been done, but in which an enormous amount can be done in principle'. What Feynman wanted to talk about was 'the problem of manipulating and controlling things on a small scale'. He recalled that whenever he mentioned this problem to people, they would tell him about miniaturization, and how far it had progressed today. For instance, Feynman recalled that 'they tell me about electric motors that are the size of a nail on your small finger. And there's a device on the market, they tell me, by which you can write the Lord's Prayer on the head of a pin. But that's nothing; that's the most primitive, halting step in the direction I intend to discuss. It is a staggeringly small world that is below' (25). He presented a vision in which the entire *Encyclopaedia Britannica* could be put on the head of a pin, and all the 24 million volumes of interest in all the libraries of the world could be put in a pamphlet of about 35 pages of the *Encylopaedia*; thus 'all the information which mankind has ever recorded in books can be carried around in a pamphlet in your hand'.

The fact that enormous amounts of information can be carried in an exceedingly small space is, of course, known to biologists. It resolves the mystery that existed, before it was understood clearly, of how it could be that, in the tiniest cell, all the information for the organization of structures such as human beings can be stored. The biological example of information on a small scale inspired Feynman to imagine what should be possible. Feynman surmised that there were economic possibilities of making things very small, and considered the problem of making computing machines smaller and smaller, for there is nothing in the physical laws to say that computer elements cannot be made vastly smaller. By pursuing these ideas in detail, Feynman started the field of nanotechnology. To begin with he announced a prize of $1000 to the first person who could take information on the page of a book and put it on an area 1/25 000 smaller on a linear scale in such a manner that it could be read by an electron microscope; the second prize of $1000 would go to the first person who could make an operating motor—a rotating motor that could be controlled from the outside and, not counting the lead-in wires, be only 1/64 inch cubed. Feynman's second prize was won in November 1960 by William McLellan, a senior engineer at Electro-Optical Systems in Pasadena, California. In 1990, researchers at IBM spelled 'IBM' by lifting and depositing individual, supercooled xenon atoms onto a nickel substrate with a scanning tunnelling microscope (STM). Less than a year later, an electronics engineer at Hitachi Central Research Laboratory (HCRL) in Tokyo, carved 'PEACE '91 HCRL' into a sulphur medium using the STM—but, unlike the IBM team, he did it at room temperature, without the need for a massive cooling system. Soon, other electronic equipment manufacturers moved in and began to realize more and more Feynman's vision about there being 'plenty of room at the bottom', and thereby the potential of nanotechnology (25, 27).

## Science, religion, culture and modern society

Beginning in the mid-1950s Richard Feynman began to give invited talks on more philosophical reflections on science, its relation to religion and its role in the modern world. In these talks, Feynman celebrated the joys and wonders of thinking about nature, championed a boldly logical and rational point of view against mystical and irrational beliefs and practices, and expressed some of his deep-seated convictions and viewpoints. He turned the full force of his derision on superstition, magic, witch doctors, UFOs, extrasensory perception, and psychology and psychiatry. About psychiatry, he would mockingly say that 'anyone who wants to visit a psychiatrist should have his head examined'. Feynman's strong views on the importance of a scientific, rational and objective outlook on life and human endeavour were formed early in life; he lived by them and unfailingly expressed them, to the extent that in his pursuit of total objectivity he could sound unfeeling and rather belligerent. (For details see Mehra (1994), chapter 25.) In a series of talks spread over the years, Feynman discussed the value of science (19), the relation of science and religion (21), the role of science in the world today (22), the role of scientific culture in modern society*, and cargo cult science (42). Feynman expressed his views on the importance of rational and logical thinking and the expression of doubt about popular beliefs, as well as the continual re-examination of cherished convictions that affect our thoughts and lives.

## Aspects of Feynman's life and personality

Richard Feynman and Mary Lou were divorced in 1956, after which Feynman again lived the life of a bachelor, seeking to have fun, but he was on the lookout for a suitable partner with whom to settle down. In the late summer of 1958, Feynman was in Geneva, Switzerland, to attend the Second International Conference on the Peaceful Uses of Atomic Energy (1–13 September 1958, United Nations, Geneva), at which he presented a joint paper on his own behalf and that of Murray Gell-Mann on the theoretical ideas used in analysing strange particles, a survey of the then current status of elementary particle physics (United Nations 1958, pp. 38–49). In his spare time he went to the beach at Lake Geneva, and there encountered Gweneth Howarth, an attractive young Englishwoman, who had gone to school and started to become a school librarian at home in Yorkshire; in her vacations she would visit mainland Europe. Then she became dissatisfied with her life and wanted to see far off places. She bought a one-way ticket to Switzerland, where, in Geneva, she obtained a job as an *au pair* with an English family, spending most of her time in her duties at home. At the beach, Feynman eyed this beautiful young girl in her polka-dot bikini and, after the start of a rather casual conversation, he moved closer to her; he soon became engaged in an exchange of information about her life and work, and told her about his own life back in California. In due course he persuaded her to give up her plans in Geneva and going on to Australia, in order to come to Pasadena and keep house for him. Feynman took care of the formalities of bringing her over with the help and guarantee of his friend and colleague Matthew Sands at Caltech. After Gweneth had arrived in Pasadena to take up her position to take care of Feynman in his duplex home, their relationship remained formal in the beginning, and she would go out on dates in the company of other young men; soon, however, she and Richard discovered each other; he made a proposal of marriage to her, which she accepted, and they were married in a private ceremony.

From the beginning Gweneth sought to make for Richard a peaceful and happy home. Richard would not really discuss his work with her but he was wonderful at explaining complicated things simply, and he would feel that she really understood it. She always had the feeling that he loved teaching. Their son Carl was born in 1962 and named after the Caltech physicist, Feynman's friend, and discoverer of the positron, Carl Anderson. Feynman developed a very close relationship with his son, and sought to help him develop in the same way

* See Feynman (1966); this article is not included in Feynman's bibliography, which is based on Laurie M. Brown's almost complete list of his important published papers, reproduced by permission in this memoir.

as his father, Melville, had helped him, and Carl caught the spirit, too. Gweneth thought that 'Carl's mind is very much like Richard's' (Mehra 1994, chapter 20). Six years later, in 1968, Richard and Gweneth adopted a two-month old infant daughter and named her Michelle. Richard did not have as close a relationship with her as he did with Carl, because she would not let him, but he adored her.

Richard and Gweneth loved travelling; they often went backpacking or camping, or just visiting unusual places. They did not like to stay in big hotels; they therefore acquired a large van in which the four of them could sleep comfortably together. Feynman had his quantum electrodynamics diagrams painted on the outside of the van. Occasionally, the Feynman family would visit Yorkshire to spend some time with Gweneth's family, relatives and friends.

In Pasadena, Richard and Gweneth would go to costume parties at the house of Albert Hibbs, Feynman's former student and collaborator, for which he would dress up elaborately each time in some unique outfit; he derived much fun from this activity. Gweneth enjoyed her life with him; she was an excellent housekeeper and a wonderful wife. She took a load off his shoulders by looking after things he did not care about, such as household expenses. Feynman was very happy with his wife and family, including the dogs, and very proud of his son.

By 1960, it had become evident, even in Feynman's mind, that he might win the Nobel Prize for one of the several original things he had done: the theory of quantum electrodynamics, the theory of the superfluidity of liquid helium, or the theory of weak interactions. For Feynman, the pleasure that he had received from his work had been enough, and he derived much satisfaction from the fact that other people found his work useful and employed it in their calculations; he no longer looked for any recognition of his work beyond what the scientific community had already conferred on him. Yet he did win the Nobel Prize in Physics for 1965, jointly with Julian Schwinger of Harvard and Sin-Itiro Tomonaga of Kyoto, Japan, for their respective contributions to the theory of quantum electrodynamics and the calculation of the Lamb shift. When the announcement of the Nobel Prize did come and was announced to Feynman very early one morning, he was a trifle annoyed, and resented the limelight and publicity it cast on him. He would have liked to refuse it but was advised against doing so because that would engulf him in greater publicity. So he did accept the prize, and Richard and Gweneth did travel to Stockholm to receive it; surprisingly, they—including Feynman—enjoyed the trip very much.

After the Nobel award ceremonies in Stockholm, Feynman went to Geneva, where, at the invitation of Victor Weisskopf (then Director-General of CERN), he gave a lecture. His lecture was attended by Ernst C.G. Stückelberg, who had done important work in quantum electrodynamics, some of which had preceded and overlapped with Feynman's. After the lecture, Feynman had a tinge of regret that he himself was covered with glory while Stückelberg walked quietly alone.

Two years after Feynman was awarded the Nobel Prize, George Beadle, President of the University of Chicago, informed him that on the recommendation of the Board of Trustees, the University wished to confer upon him the honorary degree of Doctor of Science at a special convocation to mark the 75th anniversary of the founding of the University of Chicago. However, Feynman refused to accept Beadle's offer and, after that, he would decline every such offer of an honorary degree. Quite early on, he had even resigned his membership of the US National Academy of Sciences; however, in 1962 he was awarded the Ernest Orlando Lawrence Memorial Award by the Atomic Energy Commission 'for significant contributions to nuclear science'. Early in 1965, before he received the Nobel Prize, Feynman was elected a Foreign Member of The Royal Society of London in honour of his contributions to quantum field theory and the theory of liquid helium. In 1971 Feynman received the Oersted Medal of the American Association of Physics Teachers for excellence in physics teaching, and in 1973 Queen Margarethe of Denmark bestowed upon him the Niels Bohr International Gold Medal in Copenhagen. Feynman did accept these awards and honours. However, he would routinely refuse to accept invitations to visit and lecture at prestigious institutions, always giving the excuse that his duties at Caltech did not allow him the time or the opportunity to accept them.

At Caltech, Feynman's office was always open to students to walk in and discuss scientific problems with him; however, various faculty members and administrators who wished to see him had to make appointments through his secretary. He also avoided attending committee meetings and other such things, which he regarded as 'chores' that distracted him from his work.

Feynman used to complain that he never had any good students and felt that in spite of the fact that he put a lot of energy into students, he would somehow wreck them, and he used to wonder how great physicists like Arnold Sommerfeld and J. Robert Oppenheimer had established such great schools of theoretical physics with brilliant disciples. However, in fact, Feynman appeared brusque and abrasive; although he did not choose to be curt to his students, he did not take much personal interest in them, and treated one and all in the same way. He was a born showman, who liked to perform to a large gathering and did so engagingly, but in one-to-one encounters with students he was not so successful. During all his years at Caltech, Feynman's colleagues were afraid of him. If, at faculty meetings, one said something with which he disagreed he would put them in their place with a sharp tongue; he did not suffer fools at all, and apparently no one on the faculty ever got close to him.

At seminars and colloquia, Feynman would ask terrible questions of the speaker, always showing that he knew the topic better than the speaker. He used to torment the speakers who came to give the theory seminars if he thought they were not good. As one of his prominent colleagues, William Fowler, thought, 'Feynman was a very wise man, who set very high standards for everyone. He motivated you to achieve them. Just the fact that he was around, all of us at Caltech thought that we had to live up to his standards. In this indirect way he influenced us all' (Mehra 1994, pp. 589–590).

Feynman found some close friendships in the community of artists and amateur actors. Jiryar (Jerry) Zorthian and Tom Van Sant, the artists, became his close friends. Feynman took to sketching and painting himself, and became quite good at it. He found that his drawing had a certain kind of strength—a funny, semi-Picasso-like strength, which appealed to him. By learning and practising to draw himself, he began to appreciate the difference between good and bad paintings. In 1964, he was in Italy to attend a conference celebrating the 400th anniversary of Galileo's birth, and he went to see the Sistine Chapel in the Vatican, getting there before the crowds came in, and started to look around. After looking at the ceiling for a while, his eyes came down a little bit and he saw some big framed pictures, which he did not recognize and decided that they weren't any good. Then he looked at another one and thought that was a *good one*. He had never heard of these panels, but decided that they were all good except the two; he saw the same thing in the Raphael Room. Upon returning to his hotel, he looked at the guide book and learned that below the panels of Michelangelo there were fourteen panels by Botticelli, Perugino, and so on, all great artists, and the two by so and so (lesser artists) which were of no significance. In the Raphael Room, the secret was that only some of the paintings were done by the great master himself and the others by students; Feynman liked the ones by Raphael, and he was able to seek them out, which boosted his self-confidence.

Feynman finally understood what art is for really, in certain aspects, that it gives somebody, individually, pleasure. He concluded that in his own case to sell a drawing the reason was not to make money, but to be sure that it would be in the home of someone who really wanted it. So he decided to sell his paintings. However, he did not want people to buy them because he, Richard Feynman, was a famous physicist who could draw. He made up a pseudonym and called himself 'Au Fait', French for 'thoroughly conversant', but he changed the spelling to 'Ofey'.

Richard Feynman had always been drawn to the rhythmic sounds of the beating of a drum, which he had practised regularly since his stay at Cornell and, in due course, he had become quite accomplished in playing percussion instruments, which he preferred to any type of classical music. At Caltech, Feynman would regularly take part in the annual productions of the Drama Department; he took part in the musicals *Guys and dolls*, *Kismet*, *Fiorello*, *The mad woman of Chaillot* and, in 1982, the musical *South Pacific*. In the latter, they had a lot of Caltech students dressed up like natives, and then Feynman appeared with this gigantic feather plume, all covered with feathers, naked from the waist up, and the whole scene started out with a wild piece of drumming. The last show in which he took part was in 1987; it was the musical comedy called *How to succeed in business without really trying*, in which all the action takes place in the business office of some fictitious company in New York. Feynman played the part of a janitor; it was a walk-on part, with which he actually stole the show and brought the house down. This was typical of Feynman; he would do it whenever he got the chance, to turn a walk-on part into stealing scenes.

With his second wife, Mary Lou, Feynman had gone to Mexico and Guatemala on their honeymoon. In some little town in Guatemala, they went to a museum that had a case displaying a manuscript full of strange symbols, pictures, bars and dots. It was a copy—made by a man called Villacorta—of the Dresden codex, an original book prepared by the Mayans and found in a museum in Dresden in Germany. Feynman recognized the bars and dots as numbers; he remembered how the Mayans had invented zero—his father had told him that*—and had done many interesting things. The museum had copies of the codex for sale and Feynman bought one. On each page on the left was the codex copy, and on the right a description and partial translation of it in Spanish. Feynman loved puzzles and codes, so when he saw the bars and dots he thought he was going to have some fun. He covered the Spanish with a sheet of yellow paper and began to play the game of deciphering the Mayan bars and dots, sitting in his hotel room. Upon his return to California, Feynman continued to work on the codex and deciphered it. After that, he began to read a lot about the Mayans, especially the books of the great Mayan expert Eric Thompson, and he himself became an expert on deciphering Mayan hieroglyphics.

Richard Feynman maintained that physics was his only hobby, his primary joy and entertainment. That it was also his work was beside the point. He constantly thought about it and played with it. There was nothing in his life compared with thinking about physics and nature, everything centred upon it. Feynman believed that there were two ways of doing physics: the

* The concept of zero, '*Shunya*' in Sanskrit, meaning 'nothing' was invented by ancient Hindu mathematicians in India around 3000 B.C. Melville Feynman had no knowledge of Hindu mathematics or mathematicians, and neither did Feynman until I told him in our last recorded interview.

Babylonian way and the Greek way. The Greeks were very logical and worked on things from first principles, from axioms, where one thing depended on the other. The Babylonians just related one thing to another.* Feynman always thought that he was a Babylonian and that, in his view, people like Julian Schwinger and Sin-Itiro Tomonaga were Greeks in that they tended to approach problems in a more logical fashion. This was his approach not only to quantum electrodynamics but to physics as a whole. When he saw a problem, he always tried to relate one thing to another. Feynman always said, 'I'm like a little Jewish boy in the marketplace trying not to be cheated by nature'.

In the spring of 1983, a reusable space shuttle made its maiden voyage. From the beginning there had been problems of leaks in one valve or the other, and a variety of other difficulties inherent to the flight of space vehicles, but they were usually overcome. The space shuttle *Challenger* had an accident on 28 January 1986, in which all the astronauts perished. A few days after the accident, William Graham, Head of the National Aeronautics and Space Administration (NASA), invited Feynman to become a member of the commission to investigate what went wrong with the shuttle and a whole lot of related questions; there took place extensive hearings with the NASA administrators, engineers and designers, after which the investigation was taken over by a Presidential Commission headed by William Rogers, the former Secretary of State. Soon Feynman focused on the key problem that hot gas had burnt through the O-rings (used to form a seal between the sections of the main fuel tank) on several occasions. He also learned that the zinc chromate putty in which the O-rings were embedded had bubbles or holes, and that the gas came through these holes to erode the O-rings. Feynman obtained a piece of the O-ring and, during a now famous televised public hearing, he demonstrated that at freezing temperatures (such as were present when the shuttle had been launched), the O-ring did not maintain its resiliency; he did the experiment with the piece of the O-ring and a glass of ice-cold water, which was shown on television and reported in the public press. After all the various investigative hearings, Feynman concluded that the loss of common interest between the scientists and engineers on the one hand and management on the other was the cause of the deterioration in cooperation that led to the *Challenger* disaster. Feynman reached the profound but obvious conclusion that 'for a successful technology, reality must take precedence over public relations, for Nature cannot be fooled'. (For the relevant details of the *Challenger* disaster and Feynman's role in its investigation, see Mehra (1994), chapter 26.)

## Final stay in the hospital

Towards the end of 1978, Feynman developed abdominal cancer; during the following 10 years he had to have four major operations, in which each time further growths of the tumour had to be removed, until whole sections of his inner organs and tissues had to be sacrificed. Finally, Feynman walked with a stooped and drooping gait, and was often in pain.

* In this connection, I told Feynman the true story about the following incident between Wolfgang Pauli and John von Neumann: in a conversation between them, Pauli told von Neumann the result of a difficult physical problem and how he had arrived at it. Von Neumann replied that the same result could be derived from just one axiom. Pauli responded, 'Herr von Neumann! If physics could be done with axioms, you'd be a great physicist!' I made a note of this remark as I was present at that meeting and heard it myself.

In 1987, the cancer reappeared, but this time further surgery was ruled out. He entered the UCLA Medical Center in Los Angeles on 3 February 1988, knowing that his 10-year battle with cancer would soon be over. His friends Albert Hibbs, Jerry Zorthian and Tom Van Sant would visit him in the hospital and try to cheer him up; instead they found that he would try to be jovial with them and to lift *their* spirits in the face of his own grave condition. His wife Gweneth and sister Joan stayed constantly by his bedside.

Feynman was a most courageous man who faced the end with full knowledge of what was happening to him. He had the kind of courage that one usually associates with someone who has strong religious convictions and expects rewards in an afterlife, yet he had no religious conviction or expectation. He was absolutely sure that the evolutionary process terminates existence in the same way as a leaf falling from a tree, and that life rolls over through genetic endowments. The concept of consciousness seemed too parochial to him. He could conceive of these things, but could not fit them into his conception of an expanding universe and an evolutionary design. Feynman died peacefully on 15 February 1988, just three months short of his 70th birthday.

## Acknowledgements

For the details of information about the various aspects of Feynman's life, work and personality, I have drawn on my book *The beat of a different drum: the life and science of Richard Feynman* (Mehra 1994); all of Feynman's technical-scientific and popular papers are analysed in detail in that book. *The selected papers of Richard Feynman* (Brown 2000), with Brown's brief but invaluable commentaries on Feynman's papers, has been of great use and help to me, for which I am deeply grateful and indebted. Feynman's bibliography, in the form edited by Brown, has been used in this memoir and is reproduced by permission of World Scientific.

I wish to thank the Council of The Royal Society for inviting me to write the biographical memoir of Richard Feynman, whom I had the pleasure of knowing and being friends with since the autumn of 1958; during the intervening years until his death I had the opportunity of having many conversations and numerous taped interviews with Feynman. The last set of such conversations and interviews about his life and work took place in Pasadena, California, during a period of two and a half weeks in January 1988; Feynman died almost two weeks after the last set of our taped discussions.

The frontispiece photograph was taken by me on 10 May 1962, for which Feynman posed in his office in the Norman Bridge Laboratory at Caltech.

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# Richard Phillips Feynman

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