To Have Been a Student of Richard Feynman

Laurie M Brown

It was my good fortune to study physics as a graduate student at Cornell University during 1946–50 when Richard Feynman was on the faculty. I attended his course in electrodynamics, mathematical methods of physics, quantum mechanics, elementary and advanced, and took part in the theoretical physics seminar. In the latter there were spirited discussions involving Feynman and the other elementary particle and field theorists at Cornell. These included Professors Hans Bethe and Philip Morrison, and postdocs Edwin Salpeter, who became a well-known astrophysicist, Ning Hu, who had worked with Wolfgang Pauli (and who soon returned to China where he played an important role after the Communist revolution), Fritz Rohrlich, and Robert Gluckstern. Among the graduate students was the very young-looking and prodigious Freeman Dyson. The theorists also participated actively at the experimental seminars, notably those on cosmic rays and high energy physics, with the experimentalists led by Robert R. (Bob) Wilson, the future founding director of Fermilab, and including Robert Bacher, Boyce McDaniel, Kenneth Greisen, Dale Corson, John DeWire, William Woodward, and Giuseppe and Vana Cocconi.

In 1948 I became Hans Bethe's assistant, and soon afterwards persuaded Feynman to become my thesis advisor. After the war, government funding for research at universities became widespread, but academic scientists were not sure how to handle it without incurring criticism, either from without or within the university. At Cornell, the Laboratory of Nuclear Studies, where the nuclear, cosmic ray, and high energy physics groups were located, was largely supported by the Office of Naval Research. The faculty decided that since the money was explicitly awarded for the support of research, not education, graduate students should not be paid for working on their own thesis. (This policy was changed in a few years).

Feynman had no paid assistant and, in any case, if I worked with him on my thesis, I could not be *his* research assistant, and so I had to be content with Bethe. (Of course, being able to work for both of these great physicists was an unbelievable stroke of luck for me!) Bethe had many thesis students at a time, perhaps as many as eight, as he seemed to have an inexhaustible supply of problems, but Feynman never had more than two students. Throughout his career, at Cornell and later at Cal Tech, he accepted few doctoral students. In part, as he explained, that was because he liked to concentrate on one, or at most two problems at any given time, and because he would assign only those problems for which he genuinely wished to know the answer.

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Feynman was a very popular teacher, and his advanced lecture courses were well attended by both theorists and experimentalists. The students met in small groups afterwards to compare their lecture notes and to work through them again. Although the lectures were thoughtfully prepared, they were not always easy to follow. We soon learned that Feynman's methods were anything but trivial, not to be found in any books, and that some of his views were highly unconventional. Feynman stressed creativity—which to him meant working things out from the beginning. He urged each of us to create his or her own universe of ideas, so that our products, even if only answers to assigned classwork problems, would have their own original character—just as his own work carried the unique stamp of *his* personality. Obviously, that kind of teaching extends well beyond physics, or even science in general. It was excitingly different from what most of us had been taught earlier.

Some of us, unfortunately, also learned something that could handicap us for years to come: Feynman urged us not to read very widely, but to try to work out everything by ourselves from first principles. That method could serve an outstanding mind, already well-prepared, like that of Feynman, but while it inspired us to try for originality after we left Cornell, it also lowered our productivity to a point that at times was dangerous to our academic careers. In truth, a good deal of Dick's supposed naiveté, his apparent ignorance of book learning, his disdain for mathematical rigor, etc., was feigned (although his poor pronunciation and spelling was not). It was all in the spirit of his *"Surely You're Joking …"* stories—but he conveyed it seriously enough to influence some of his admiring students, including me. (Working as Bethe's assistant did not encourage a lot of reading either, since Bethe seemed to carry all the essential physics in his head, and was able to calculate quickly any result that he wanted.) Feynman's informality was, in part, an act put on to command attention to impress his young audience. On the whole, however, he was a careful, responsive, and caring teacher and we students felt that he was serving as our guide, not our judge.

In 1947–48, I had two semesters of quantum mechanics, first with Feynman and then with Bethe; the following year, I took Feynman's course in advanced quantum mechanics. At that time, he was working actively, indeed one might say compulsively, on his diagrammatic approach to renormalized quantum electrodynamics (QED), the work for which he shared the Nobel Prize in 1965. His course made use of the new methods that he was developing, and we students became expert in the use of Feynman diagrams before they were written up for publication. Some of Bethe's doctoral students used the Feynman diagram methods to do calculations based upon the weak-coupling meson theories that were very popular then. Unfortunately, the meson theory results turned out to have little relevance to nature, or to future developments of theoretical physics.

Since Feynman was only twenty-eight years old when he came to Cornell, and because many of the graduate students' studies had been interrupted by war service, it turned out that most of the graduate students were only a few years younger than Feynman. Some of them had served at Los Alamos during the war and were on familiar terms with Cornell faculty members who had worked there, including Feynman, Bethe, Morrison, Wilson, and others. Socially, things tended to be informal, partly because of the relatively small age spread, and partly as a carryover from the spirit of wartime collaboration, which had created a kind of meritocracy and a disregard of seniority, although to some extent the Los Alamos pecking order was maintained.

Some of the theoretical students resuming their studies had learned earlier what came to be called (after the "manifestly covariant" version became established in the later 1940s) the "old-fashioned" field theory. This formulation of quantum electrodynamics was due mainly to Heisenberg and Pauli, and its applications to the meson theory of nuclear forces were to be found in Gregor Wentzel's book, *The Quantum Theory of Fields*. Feynman was always looking for a contest of some sort, and he liked to challenge the more knowledgeable students with this sort of query: "According to Pauli, how many virtual photon polarizations must be exchanged?" Then after receiving the conventional answer (namely, two), he would say, "But that's wrong, it should be four because …" A student working on a thesis that made use of the accepted field theory methods would develop anxiety, becoming upset at being asked to carry the burden of defending a theory for which he was not really responsible. (I know I should say "he or she" — but there were no female theory graduate students, although there were several excellent female experimenters.) Their concern was this: If this adventurer, Feynman, bent on destroying conventional field theory, were to be successful, might the student's thesis turn out to be wrong, and possibly not be accepted?

On the other hand, students who took Feynman's courses and learned his methods tended to disregard the Heisenberg-Pauli methods—and indeed had a totally unjustified contempt for them that would have to be unlearned later. That was one example of Feynman's intensity being able to carry everything before him. He drove himself, working literally day and night. When he felt challenged to prove a conjecture or to complete a calculation, he would work all through the night. Then, for some weeks he might do no physics at all. Of his thesis students he demanded a comparable intensity, and he assigned them problems that were extensions of his own research and for which he was eager for results. Consequently he had few students, for usually he was concentrating on a single problem. One very good student changed from being a theoretical to an experimental physicist in an instant, when Feynman came in one Monday and told him that during the weekend he had solved the thesis problem that Feynman had assigned to him six months earlier.

At Cornell, Feynman was noted for unusual social behavior, the sort featured in his popular sketches, "*Surely You're Joking, Mr. Feynman.*" For example, he like to attend dances for undergraduates, where he was usually thought to be a student, based upon his youthful appearance and exuberance. However, his devil-may-care pose may have been a concealed reaction to grief and loneliness. Readers of the second of the two autobiographical collections, "*What do you care what other people think?,*" will recall from his moving memoir on the subject that Feynman's first wife, Arline, had died of a lingering illness in 1946, while he was at Los Alamos. In spite of the extroverted *persona* that he liked to present, he was actually a very private person, who reacted angrily whenever anyone tried to penetrate this façade, as I have observed faculty wives and others try. Once he was persuaded by a psychology graduate student to take a Rohrschach test, but during the test he would discuss only the physical production of the inkblot. For example, he would say, "This one was made with thumb and middle finger pressed together," and so forth. Afterwards, he told me about it, stating proudly that he had outwitted the analyst!

During the time that I was his doctoral student, he was writing his famous papers on the theory of positrons (with the backward-in-time paths) and on QED, which appeared together in the same issue of the Physical Review. He asked me to read them, to correct his terrible spelling and grammar, and to serve as a guinea pig. Anything that was not clear to me, he said he would rewrite, as he wanted it to be readable by someone at my level of ability and experience. Thus I had a marvelous opportunity to discuss these important works with him at length. He assigned to me at first a thesis topic that bore fruit only in later years, but which allowed me to have a taste of being creative in physics. (However, my actual thesis topic turned out to be a different one.)

Pauli had noted that a term could be added to Dirac's relativistic Hamiltonian for the electron that would represent an anomalous magnetic moment interaction without violating any accepted invariance requirements. This could, it appeared, be used to represent the neutron's electromagnetic interaction, for example. Feynman noted that a term of the same form as Pauli's appears automatically upon iterating the Dirac equation. [More explicitly, if the Dirac equation is written $(D - m)\,\Phi = 0$, the iterated equation reads $(D + m)\,(D - m)\,\Phi = 0$.] Assume now that the new Pauli term has a variable coefficient σ. For $\sigma = 1$, the resulting magnetic moment is exactly the Dirac moment, for $\sigma = 0$, it is zero. Feynman asked me to apply the "Feynman rules", i.e., his diagrammatic algorithm, to calculate various well-known processes for arbitrary σ. I found interesting and unexpected results: some differential "cross sections" had negative regions, although for $\sigma = 1$ the Dirac results and for $\sigma = 0$ the usual spin 0 results were obtained. Most interestingly, the equation had too many solutions, but the physical ones (of positive mass) could be selected if the Dirac wave function were restricted to have only two components, rather than the conventional four components.

Feynman was excited about the possibility of a two-component Dirac theory and he reported my results to Bethe. However, the latter persuaded him that the topic was unsuitable for a thesis, being too speculative and "original." Bethe felt that a standard calculation was more appropriate for an apprentice theorist. (I heard the discussion through the wall of my room, which adjoined Bethe's office. Feynman, as usual, spoke very loudly.) Accordingly, the next day I began work on the radiative corrections to the Klein-Nishina formula for Compton scattering, which became my actual doctoral thesis and which I later published with Feynman as coauthor.

The two-component Dirac equation, as I discovered later, had already been considered by Hendrik Kramers (and in its massless form by Hermann Weyl). Although the QED based upon it conserved reflection symmetry, the equation itself was not *manifestly* parity invariant, and it had been rejected by Pauli on those grounds. In 1957, when Feynman was working on a parity-nonconserving theory of the weak interactions, I met him at that year's Rochester Conference on High Energy Nuclear Physics, and he asked me to send to him all my notes on the two-component theory. I did so, and also wrote up the two-component version of QED as a paper, for which I asked Feynman to be a coauthor. He declined, saying that he thought I deserved to be the sole author, and so I published it alone in the *Physical Review.*

After that I saw Feynman infrequently, as he did not travel very much, but I continued to follow his splendid career with great interest. Whenever I wrote anything, and whenever I had to make a scientific judgement or decision, I always asked myself, consciously or unconsciously, "What would Feynman think of this?"—and then I tried to decide accordingly. There is no doubt in my mind that Feynman's spirit and approach to physics lives on in my mind, as it does in many other minds.

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