MEMORIES OF RICHARD FEYNMAN

A 'new' set of lectures—on computation—by one of the more colorful characters in modern physics, gives rise to these reminiscences by an Englishman in Richard's court.

Anthony J. G. Hey

I well remember my arrival at Caltech on a sunny October morning in 1970. Fresh from the University of Oxford where even graduate students at that time wore ties and shirts, I was unsure what to wear for my first meeting with Murray Gell-Mann. I gambled, wrongly, on a suit, and arrived at the office of the theory group secretary, Julie Curcio, feeling more and more overdressed and as if I had a large label dangling from my collar saying "New PhD from Oxford." I had seen Gell-Mann once before in England but was unsure if the bearded individual dressed in an open-necked shirt and sitting in Julie's office was indeed the eminent professor. A moment after I had introduced myself, my doubts were dispelled by the man putting out his hand and saying "Hi, I'm Murray." This episode illustrates only a small part of the healthy culture shock I experienced in California. Six years in Oxford had left me used to calling my professor "Professor Dalitz, sir." At that time, I would certainly not have dared to address Richard Dalitz as "Dick."

One of my first tasks on arrival in Pasadena was to buy a car. That was not as easy as it sounds. The used car lots in Pasadena are sprinkled down Colorado Boulevard for several miles in typical US fashion, and getting to them in the days when public transport in Los Angeles was probably at its lowest ebb was not straightforward. It was only after my wife and I were stopped by the police and asked why we were walking on the streets of Pasadena that I understood the paradox that, in California, you had to have a car to buy a car. Another chicken-and-egg problem arose in connection with "ID," a term we had not encountered before. As a matter of routine, the police demanded to see our ID and of course the only acceptable ID in deepest Pasadena at that time was a California driver's license. A British driving license without a photograph of the bearer was clearly inadequate, and even our passports were looked on with suspicion.

An introduction to America via used car salesmen is not the introduction I would recommend to my worst enemy, and it is not surprising that I sought advice from the Caltech grad students. I was pointed in the direction of Steve Ellis, whose advice was valued because he came from Detroit and was believed to be worldly-wise. I tracked Steve down to the seminar room, where I saw he was engaged in a debate with a character who looked mildly reminiscent of the used car salesmen I had recently encountered. That was, of course, my first introduction to Dick Feynman. At first, I did not recognize him from the much earlier photograph I knew from the three red books of the *Feynman Lectures on Physics* (Addison-Wesley, 1963). Curiously enough, even after ten years or more, I always felt more comfortable addressing him as Feynman rather than Dick.

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No doodling in science

Compared to my previous life as a graduate student in Oxford, life at Caltech was like changing to the fast lane on a freeway. First, instead of Oxford being the center of the universe, it was evident that, to a first approximation, Europe and the UK did not exist. Second, I rapidly discovered that the ethos of the theory group of Feynman and Gell-Mann was that physics was all about attacking the outstanding fundamental problems of the day: It was not about getting the phase conventions right in a difficult but ultimately well understood area. I remember asking George Zweig, a coinventor of the whole quark picture of matter, for his comments on a paper of mine. It was the not-about-to-be-very-famous SLAC-PUB 1000, a paper I had written with an experimenter friend at the Stanford Linear Accelerator Center (SLAC) about the analysis of three-body final states. George's uncharacteristically gentle comment to me was, "We do, after all, understand rotational invariance." In fact, the paper was both useful and correct but, on the Caltech scale of things, it amounted to doodling in the margins of science. In those days, I aspired to be as good a physicist as Zweig: This ambition strikes me now as similar to wanting to emulate the achievements of Jordan in the early days of quantum mechanics, rather than those of his collaborators, Heisenberg and Born.

One of the nicest things about Caltech was the sheer excitement of being around Feynman and Gell-Mann. As a postdoc from England, where one gains a rapid but narrow exposure to research, my wife and I were contemporary in age with the final-year grad students, and a lot of our social life was spent with them. Feynman was actively working with two of them, Finn Ravndal and Mark Kislinger, who had just been awarded his PhD for

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LIFE AT CALTECH WAS NEVER DULL with Murray Gell-Mann (left) and Dick Feynman giving seminars on linguistics and heiroglyphics, as well as probing the deepest levels of theoretical physics, all the while striking sparks off each other. (Photo courtesy of AIP Emilio Segrè Visual Archives.)

his own version of the quark model. Perhaps because of his work with Ravndal and Kislinger, Feynman was very involved with the final-year grad students, and we all—finishing grad students and postdocs—had lunch with him most days at the "Greasy," as the Caltech self-service cafeteria was universally known. Needless to say, our table was always the center of attraction. One frequent topic for discussion was Feynman's explanation of some new experimental results obtained at SLAC on electron–proton scattering. Feynman's "parton model," an intuitively appealing picture of the proton made up of pointlike constituents, was sweeping all before it, much to Murray's annoyance.

It was not surprising that I had left Oxford full of enthusiasm for working on the parton model and looking forward to hearing Feynman on the subject he had invented. Curiously, Feynman's only publication on partons was applied to proton–proton scattering. It was when he was visiting SLAC, and the experimenters told him of their surprising results with electrons and protons, that Feynman realized that this could provide a much simpler application of his parton model. There and then, Feynman gave a seminar in which he explained their results using partons. Nothing was written down by him after the seminar, however, and it was left to James Bjorken, who had been away from SLAC at the time of Feynman's visit, and Emmanuel Paschos, a postdoc at SLAC, to write up the analysis of the experimental results in terms of Feynman's parton model.

My first encounter with Feynman on a technical level was intimidating. Two Caltech experimenters, Barry Barish and Frank Sciulli, had just had a proposal for a neutrino–proton experiment accepted. Since I liked to work with experimenters, they asked me to give an informal lunchtime seminar to their group explaining the application of the parton model to their experiment. Imagine my surprise when I turned up to talk to the experimental group and found Feynman sitting in the audience. Still, I started out and even managed to score a point off Feynman. At an early stage in the seminar, he asked how I derived a particular relation. I replied, with what now seems like foolhardy temerity: "I used conserved vector current theory; you should know, you invented it!" In fact, all went well until I had nearly reached the end of the seminar. I was just outlining what further predictions could be made when Feynman said: "Stop. Draw a line. Everything above the line is the parton model. Below the line are just some guesses of Bjorken and Paschos." As I rapidly became aware, the reason for Feynman's sensitivity on this point was that Murray was going round the fourth floor of Lauritsen, the physics and astronomy building at Caltech, growling that "Partons are stupid" and that "Anyone who wants to know what the parton model predicts needs to consult Feynman's entrails!" In fact, all the results above Feynman's line in my seminar were identical to predictions that Murray had been able to derive using much more sophisticated algebraic techniques. Feynman wanted to dissociate himself from some of the wilder parton-model predictions of others and to stress that his simple intuitive parton approach gave predictions identical to Gell-Mann's much fancier methods. Unfortunately, my seminar just happened to be a handy vehicle for him to make this point!

The awkward Feynman notebooks

There were, of course, drawbacks to being in the same group as Feynman and Gell-Mann. I had come to Caltech with the firm intention of pursuing research on Feynman's parton model. What I had not realized was that Caltech was the one place where one could not publish research

on partons! Why was this? There was the obvious distaste of Gell-Mann for the whole approach, but that would not have mattered if it had not been for the awkward existence of Feynman's notebooks.

I used to go to Feynman with some idea and proudly display my analysis on his blackboard. Each time, Feynman listened, commented and corrected, and then proceeded to derive my "new" results several different ways, pulling in thermodynamics, rotational invariance or what have you, and using all sorts of alternative approaches. He explained to me that, once he could derive the same result by a number of different physical approaches, he had more confidence in its correctness. Although his explanation was very educational and stimulating, it was also somewhat dispiriting and frustrating. After all, one could hardly publish a result that Feynman already knew about and had written down in his famous working notebooks but had not bothered to publish.

So it was somewhat in desperation that I turned to Gell-Mann's algebraic approach for a more formal framework within which to work. With Jeff Mandula, an assistant professor, I looked at electron–proton scattering when both the electron and proton are polarized, with their spins lined up in the same direction. We found a new prediction whose parton equivalent was obscure. Roughly speaking, at high energies the spin direction of the parton is unchanged by collision with an electron. Our result concerned the probability of the parton spin changing its direction in the collision, which was related to the so-called spin-flip amplitudes normally neglected in the parton model. Armed with this new result, I went to Feynman and challenged him to produce it with his parton approach. In the lectures he gave at Caltech the next term, published as the book *Photon–Hadron Interactions* (W. A. Benjamin, 1972), you will find how Feynman rose to this challenge.

Exploits of Dick and Murray

Life at Caltech with Feynman and Gell-Mann was never boring. Stories of their exploits abounded, many of Feynman's subsequently having been preserved for posterity by his friend Ralph Leighton in *Surely You're Joking, Mr. Feynman!* (Norton, 1985). There were many other stories. A friend told me of the time he was about to enter a lecture class and Gell-Mann arrived at the door to give the class. My friend was about to open the door but was stopped by Murray, who said, "Wait!" There was a storm raging outside the building, and at the appearance of a particularly violent flash of lightning, he said, "Now!"—and entered the class accompanied by a duly impressive peal of thunder.

Another story that circulated was of Feynman giving a talk about the discovery, with Gell-Mann, of the $V-A$ model of weak interactions. After the talk, a person in the audience came up to him and said, "Excuse me, Professor Feynman, but isn't it usual in giving a talk about joint research to mention the name of your collaborator?" Feynman reportedly came back with: "Yes—but it's usual for your collaborator to have done something!" Obviously these stories get inflated in the telling, but I did ask Feynman about this one since it seemed so out of character for the Feynman I knew. He smiled and said, "Surely you don't believe I would do a thing like that!" I only knew Feynman after he had received the Nobel Prize and found happiness in his marriage to Gweneth. Before this time, a somewhat more abrasive and aggressive picture emerges from biographies of him, so I am still not sure!

Certainly Feynman enjoyed making a quick and amusing response. This trait was often in evidence in seminars given by visiting speakers. On one memorable occasion, the speaker started out by writing the title of his talk on the board: "Pomeron Bootstrap." Feynman shouted out, "Two absurdities," and the room dissolved into laughter. Alas for the speaker, he was deriving theoretical results supposedly valid in one energy regime but going on to apply them in another. That was just the kind of academic dishonesty that Feynman hated, and on that particular occasion the speaker had a very uncomfortable time fielding brickbats thrown by the entire audience. Nevertheless, Feynman could be restrained: During another seminar, he leaned over to me and whispered, "If this guy wasn't a regular visitor, I would destroy him!"

It was during this time at Caltech that Feynman gave his celebrated lecture on deciphering Mayan hieroglyphics. His account is contained in *Surely You're Joking, Mr. Feynman!* The story illustrates perfectly Feynman's approach to tackling a new subject. Rather than look at a translation of the codex, Feynman made believe he was the first to get hold of it. Struggling with the Mayan bars and dots in the tables, he figured out that the Dresden Codex predicted both the different phases of Venus and lunar eclipses. With a typical down-to-earth analogy, Feynman likened the Mayans' fascination with "magic" numbers to our childish delight in watching the odometer of a car pass 10 000, 20 000, 30 000 miles and so on.

As Feynman says, "Murray Gell-Mann countered in the following weeks by giving a beautiful set of six lectures concerning the linguistic relations of all the languages of the world." For these lectures, Murray used to arrive clutching armfuls of books and proceed to tell his audience about the classification of languages into "superfamilies" with a common origin. He was always fond of drawing attention to the similarities between English and German and, for example, delighted in calling George Zweig "George Twig." Even though it seemed a bit strange for professional particle physicists to be attending lectures on comparative linguistics, life at Caltech was always interesting!

Other recollections of Feynman are still fresh in my memory. One time I went to get the coffee at lunch in the Greasy and returned to the table to find that Feynman had invited my wife to his house in Mexico for the weekend—with his family, I hasten to add. As an afterthought he invited me too, and we found ourselves strolling along the beach in Mexico, talking physics with Feynman late into the night. Feynman's advice to me on that occasion was, "You read too many novels." He had started out very narrow and focused; only later in life had his interests broadened out. Good advice perhaps, but during the years I knew Feynman, I also learned how impossible he was for anyone to emulate—in his disregard for the "unimportant" things of life, like committees and administration, and in his unique ability to attack physics problems from many different angles.

On another visit to Caltech many years later, sitting with him in the garden of his house in Altadena, I watched Feynman take off his belt and demonstrate his new understanding of the spin–statistics rule. He later wrote this up in a memorial lecture to his hero in physics, Paul Dirac, discoverer of antimatter. This was some twenty

A MASTERFUL LECTURER, Feynman used body language, plain words, keen insight, drama, humor and boundless enthusiasm to captivate his audiences. (Photo taken from Feynman lecture film "Distinction of Past and Future Pt. I," Department of Physics, Eastern Nazarene College, courtesy AIP Emilio Segrè Visual Archives.)

years after the publication of *The Feynman Lectures on Physics* in which he had apologized for not being able to give an elementary explanation of this rule. As he said then: "This probably means we do not have a complete understanding of the fundamental principle involved."

One-of-a-kind lectures

What made Feynman's lectures unique? In a review in *Science* (20 July 1973), N. David Mermin, himself noted for thoughtful and penetrating analyses of supposedly well understood problems in physics, was moved to say: "I would drop everything to hear him lecture on the municipal drainage system." On 14 March 1967 the *Los Angeles Times* science editor, Irving Bengelsdorf, wrote: "A lecture by Dr. Feynman is a rare treat indeed. For humor and drama, suspense and interest it often rivals Broadway stage plays. And above all, it crackles with clarity. If physics is the underlying 'melody' of science, then Dr. Feynman is its most lucid troubador." In the same article, Bengelsdorf summed up the essence of Feynman's approach: "No matter how difficult the subject—from gravity through quantum mechanics to relativity—the words are sharp and clear. No stuffed shirt phrases, no 'snow jobs,' no obfuscation." Later that year (8 October), a *New York Times Magazine* article said that Feynman "uses hand gestures and intonations the way Billy Rose used beautiful women on the stage, spectacularly but with grace."

For me, it was Feynman's choice of words that made a Feynman lecture such a unique experience. The same 1967 *New York Times* article went on to say that "his lectures are couched in pithy often rough-cut phrases." There are innumerable examples to choose from, even in his published lectures. For example, in the middle of pages of complicated mathematics, Feynman deliberately lightened the text by introducing phrases like "you can cook up two new states . . ." or by personalizing the account with imagined conversations of physicists, as in " 'Now,' said Gell-Mann and Pais, 'here is an interesting situation.' " In his 1971 invited lecture, when he received the Oersted medal for his services to the teaching of physics, Feynman began disarmingly by saying, "I don't know anything about teaching," and then proceeded to give a fascinating account of the research problem he was working on: "What is the proton made out of? Nobody knows but that's what we're going to find out." In the talk, he likened smashing two protons together to smashing two watches together: One could look at the gear wheels and all the other bits and pieces that resulted and try and figure out what was happening. In that way, he was able to explain that smashing a simple point particle like an electron into a proton was much simpler because there was only one watch to look at. At the 1964 summer school in Erice, Italy, he was asked a question about conservation laws. Feynman replied: "If a cat were to disappear in Pasadena and at the same time appear in Erice, that would be an example of global conservation of cats. This is not the way cats are conserved. Cats or charge or baryons are conserved in a much more continuous way."

Required Reading

Feynman's Nobel Prize lecture (published in PHYSICS TODAY, August 1966, page 31) should be required reading for all aspiring scientists. In it, Feynman forewent the customary habit of removing the scaffolding that was used to construct the new theory. Instead, he described all the blind alleys and wrong ideas he had encountered on the way to his great discoveries. The article also reveals more of Feynman's lecture technique, as when he said: "I shall include details of anecdotes which are of no value scientifically nor for understanding the development of the ideas. They are included only to make the lecture more entertaining." In the lecture, we find out how Feynman first started on his attempt to answer the challenge of Dirac concerning the troublesome infinities that plagued relativistic quantum mechanics. In the last sentence of his famous book (*The Principles of Quantum Mechanics*, 2nd edition, Oxford, 1935) Dirac said: "It seems that some essentially new physical ideas are here needed." Of his own youthful and essentially new idea for solving the problem, Feynman said: "The idea seemed so obvious to me and so elegant that I fell deeply in love with it. And, like falling in love with a woman, it is only possible if you

'HOW ARE YOU DOING?' Whether cracking a safe or answering the 'big questions' of physics, Feynman's response was the same: You can't tell until you're finished; until then, you only know what doesn't work. (Photo courtesy of Michelle Feynman.)

do not know too much about her, so you cannot see her faults. The faults will become apparent later, but after the love is strong enough to hold you to her. So, I was held to this theory, in spite of all difficulties, by my youthful enthusiasm."

Later in the lecture, Feynman said: "I suddenly realized what a stupid fellow I am; for what I had described and calculated was just ordinary reflected light, not radiation reaction." This refreshing honesty from one of the greatest physicists of the 20th century reminds me of another of my heroes, Johannes Kepler, who was first to write down laws of physics as precise, verifiable statements expressed in mathematical terms. Unlike Copernicus and Newton, Kepler wrote down all the twists and turns in his thought processes as he was forced to the shocking conclusion that the orbit of Mars was not a circle but an ellipse. Kepler summed up his struggle with the words, "Ah, what a foolish old bird I have been!"

One of the best anecdotes told by Feynman in his lecture concerned a physicist named Murray Slotnick and his encounter with "Case's theorem." The story described the moment when Feynman realized that his "diagrams" really were something new. In its full form the story runs as follows. At the January 1949 meeting of the American Physical Society in New York, Slotnick presented a paper comparing two different forms for the electron–neutron coupling. After a long and complicated calculation, Slotnick concluded that the two forms gave different results. At that point, Robert Oppenheimer rose from the audience and remarked that Slotnick's calculation must be wrong since it violated Case's theorem. Poor Slotnick had to admit he had never heard of this theorem, so Oppenheimer kindly told him he could remedy his ignorance by listening to Kenneth Case presenting his result the next day. That evening, in his hotel, Feynman could not sleep so he decided to use his new methods to repeat Slotnick's calculations. Feynman then goes on to say:

> The next day at the meeting, I saw Slotnick and said, "Slotnick, I worked it out last night; I wanted to see if I got the same answers you do. I got a different answer for each coupling—but, I would like to check in detail with you because I want to make sure of my methods." And he said, "What do you mean you worked it out last night, it took me six months!" And, when we compared the answers he looked at mine, and he asked, "What is that Q in there, that variable Q?" I said, "That's the momentum transferred by the electron, the electron deflected by different angles." "Oh," he said, "no, I only have the limiting value as Q approaches zero, the forward scattering." Well, it was easy enough to just substitute Q equals zero in my form and I then got the same answers as he did. But it took him six months to do the case of zero momentum transfer, whereas during one evening I had done the finite and arbitrary momentum transfer. That was a thrilling moment for me, like receiving the Nobel Prize, because that convinced me, at last, I did have some kind of method and technique and understood how to do something that other people did not know how to do. That was my moment of triumph in which I realized I really had succeeded in working out something worthwhile.

What Feynman did not include in his lecture was that he had stood up at the end of Case's talk and said, "Your theorem must be wrong. I checked Slotnick's calculation last night and I agree with his results." In the days when calculations like Slotnick's could take as much as six months, the Feynman–Slotnick–Case encounter was the incident that put Feynman's diagrams on the map.

The other piece of required reading for students of all science disciplines is Feynman's article on "Cargo Cult Science," which is somewhat modified in *Surely You're Joking, Mr. Feynman!* It was originally Feynman's commencement address to new Caltech graduates in 1974; in it Feynman discussed science, pseudoscience and learning how not to fool yourself. The unifying theme of the talk was Feynman's passionate belief in the necessity for "utter scientific integrity"—in not misleading funding agencies about likely applications of your research, in publishing results of experiments even if they do not support your pet theory, in giving government advice it may rather not hear, in designing unambiguous rat-running experiments and so on. As he said, "Learning how to not fool ourselves is, I'm sorry to say, something that we haven't specifically included in any particular course that I know of. We just hope you've caught on by osmosis." He concluded with one wish for the new graduates: "The good luck to be somewhere where you are free to maintain the kind of integrity I have described, and where you do not feel forced by a need to maintain your position in the organization, or financial support, or so on, to lose your integrity." At the risk of sounding pompous, I think the world owes a vote of thanks to Caltech for providing just such an environment for Richard Feynman.

Two More Stories

It seems appropriate to end these reminiscences with two more Feynman stories. The first one harks back to his safecracking days at Los Alamos. At a 1971 particle physics conference in Irvine, California (AIP Conf. Proc. 6, 1972), Feynman agreed to be on a discussion panel at the end of the conference. He was asked if he thought that physicists were getting anywhere with answering the "big questions." Feynman replied: "You ask, Are we getting anywhere. I'm reminded of a situation when I was asked the same question. I was trying to pick a safe. Somebody asked me, 'How are you doing? Are you getting anywhere?' You can't tell until you open it. But you have tried a lot of numbers that you know don't work!" The second story is the last Feynman story of all. Gweneth was by his bedside in the hospital and Feynman was in a coma. She noticed that his hand was moving as if he wanted to hold hands with Gweneth. She asked the doctor if this was possible but was told that the motion was automatic and did not mean anything. At which point, Feynman, who had been in a coma for a day and a half or so, picked up his hands, shook out his sleeves and folded his hands behind his head. It was Feynman's way of telling the doctor that even in a coma he could hear and think—and that you should always distrust what so-called experts tell you!

The final word deserves to be given to James Gleick, author of *Genius: The Life and Science of Richard Feynman* (Pantheon, 1992). Gleick memorably summed up Feynman's philosophy toward science with the following words: "He believed in the primacy of doubt, not as a blemish upon on our ability to know but as the essence of knowing."

[PHYSICS TODAY had a special issue on Feynman in February, 1989.] ■