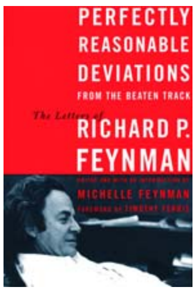
Correspondence Paints a Portrait of Feynman

Perfectly Reasonable Deviations from the Beaten Track: The Letters of Richard P. Feynman

Edited by Michelle Feynman
Basic Books, New York, 2005. $26.00 (486 pp.). ISBN 0-7382-0636-9

Reviewed by Silvan S. Schweber



In 2004 Richard Feynman's daughter Michelle went through her father's papers in the Caltech archives and in her basement and culled from them his nontechnical correspondence. She edited a good number of those letters and published them in *Perfectly Reasonable Deviations from the Beaten Track: The Letters of Richard P. Feynman*. The letters reveal a facet of Feynman's personality that is less well known than those revealed in his other works. In the letters he is, as his daughter describes him in her introduction, "articulate, insightful, considerate, humble, nurturing, funny, and charming."

The first letters, written between 1939 and 1942, are those Feynman wrote as a graduate student at Princeton University, to his parents and to his high-school sweetheart, Arline Greenbaum. The letters reveal a remarkably mature and sensitive 21-year-old man. Arline was sick with lymphatic tuberculosis, and Feynman's parents were fearful about his marrying her and getting infected; they were also concerned about what taking care of her would mean to his future. At his father's suggestion, Feynman asked Henry De Wolf Smyth, the chairman of Princeton's physics department, how his marriage to Arline might affect his career. Smyth said it made no difference to him but suggested that Feynman ask the university doctor whether he might infect students in the classes he would teach. The doctor assured him there was no danger of infection and predicted "how long she would have it," given her condition. He also warned Feynman that "it would be very bad for an active case of T.B. to become pregnant" (page 9).

To overcome his mother's objections and assuage her worries, Feynman wrote to her that he wanted to marry Arline not to be noble, nor to fulfill a promise he had made to her five years earlier, but because he loved her and wanted to take care of her. He stressed that he had "other desires and aims in the world," one of them being "to contribute as much to physics" as he could. That goal, in his mind, was "of even greater importance than [his] love for Arline." Moreover, he was quite sure that marrying her would "interfere very slightly, if at all with [his] main job in life" (page 13). And so they were married on 29 June 1942 upon his obtaining his doctorate.

The letters between Arline and Feynman, written after he had gone to Los Alamos Laboratory in New Mexico—and she to a sanatorium in nearby Albuquerque in the spring of 1943—are the most moving letters in the volume. His letters to his mother, with whom he was very close and to whom he turned for comfort and advice when Arline's condition deteriorated, chronicle the course of Arline's illness. His letters to his wife indicate how hard he was working but also, above all, how remarkably devoted, caring, comforting, and loving a husband he was. Arline died on 16 June 1945, and Feynman was clearly devastated. Two years after her death, Feynman, the supreme rationalist, wrote a heart-rending love letter to her: "I want to tell you I love you. I want to love you—I will always love you." He concluded his letter with a postscript: "Please excuse my not mailing this—but I don't know your new address" (page 69).

Work at Los Alamos kept Feynman going after Arline's death. In August 1945, after an atomic bomb had been dropped on Hiroshima, he wrote his mother a long letter detailing his involvement with the bomb project and describing his reaction to witnessing the Trinity test. Only in Feynman's 1959 letters to Gweneth Howarth, whom he married in 1960, does one get the sense that he had recovered from the trauma of the loss of Arline.

The subsequent letters are arranged in essentially chronological order. They give a glimpse of Feynman's reactions to some of the major post–World War II political developments: his refusal to work on the hydrogen-bomb project after the detonation of the first Soviet atomic bomb (page 83) and his distress concerning the US government's actions in the Vietnam War (page 243). The letters also reveal his deep involvement in the improvement of the K1–12 teaching of mathematics and the sciences in California (pages 193 and 439); his stance regarding his Jewish background (page 234); his views on the relation between science and religion (page 147); his desire to become an astronaut on the Apollo project (page 231); his desire to resign from the National Academy of Sciences (page 105); and much else.

Important appendixes to the book present the spoken text of a 1959 television interview with journalist Bill Stout; Feynman's lecture on the future of physics delivered at MIT's centennial in 1961; his 1965 report on the new mathematics textbooks for elementary and high-school students in California; and Lee Edson's elegant article on "Two Men in Search of the Quark" (the two being Feynman and Murray Gell-Mann), which appeared in the *New York Times Magazine* in October 1967. Michelle Feynman's introduction provides valuable insights into life in the Feynman household. I hope Feynman's more technical correspondence will receive a similar devoted treatment and will eventually be published.

Many of the letters Feynman received when he was awarded the 1965 Nobel Prize in Physics are included in the book. The outpouring of admiration and affection is amazing. Even more remarkable are Feynman's replies. He personally acknowledged each message he received and in his reply he always referred to some statement in the original communication. In his letter to Laurie Brown, his first doctoral student, who had thanked him for what he had given and for what he had "added to the excitement, fun and seriousness of

Sam Schweber *is a professor emeritus in the physics department at Brandeis University in Waltham, Massachusetts, and a senior research associate at MIT's Dibner Institute. He is the author of* In the Shadow of the Bomb: Bethe, Oppenheimer, and the Moral Responsibility of the Scientist *(Princeton U. Press, 2000).*

our profession," Feynman commented that while he was "excited by all the congratulations telegrams," Laurie's stood out and "meant an especially great deal" to him. "It was like another little prize" (page 158).

To Julius Ashkin, whose friendship with Feynman dated back to Los Alamos, and who had very carefully gone over and corrected Feynman's 1949 quantum electrodynamics papers and thus, according to Feynman, had made them "worthy" of consideration by the Nobel Prize committee, he wrote, "I owe to you not only the great pleasure of having had your personal acquaintance but also ultimately, no doubt, the fact that I received the Nobel prize" (page 160).

Of all the letters presented in the book, one group stands out by virtue of the care and consideration devoted to each of them. Those are the letters Feynman wrote to young students who asked him for advice on career decisions and prospects and those written to the youngsters' parents. The answer was always a variation on "Work hard to find something that fascinates you. . . . When you find it you will know your lifework" (page 229).

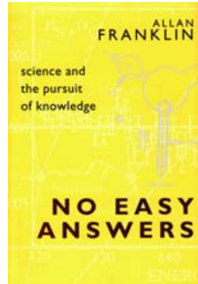
Among the most moving of these letters is the one Feynman wrote to Koichi Mano, a former student, who was despondent because he was tackling "humble" problems. Feynman issued a slight reproof: "The worthwhile problems are the ones you can really solve or help solve, the ones you can really contribute something to." And he listed some of the many problems, which Mano would call humble, that he had worked on, sometimes successfully, sometimes unsuccessfully: the coefficient of friction for highly polished surfaces, how to make electroplated metal stick to plastic objects, the design of a neutron counter, the theory of turbulence, and so forth (page 199).

It is difficult to convey the exhilaration and the increased admiration engendered by reading the letters of this remarkable human being. I urge that as wide an audience as possible read them for an uplifting experience.

No Easy Answers: Science and the Pursuit of Knowledge

Allan Franklin
U. of Pittsburgh Press, Pittsburgh, PA, 2005. $29.95 (258 pp.). ISBN 0-8229-4250-X

In the past 30 years the so-called constructivist sociology of science has produced a large number of studies on consensus building in science. Many of the analyses look at physics as a test case to show that controversies are more frequent than what philosophers and scientists often suggest. Building consensus in science involves negotiating what constitutes a fact in a given scientific community. Constructivist and relativist sociologists insist that the contingent aspects of the negotiations are important. By contrast, rationalist philosophers focus on the role of empirical tests and logical coherence as objective grounds for believing in the physical reality of phenomena and the explanatory value of theories.



The relativist conception of science, which minimizes the role of reason in science, has been criticized by many historians and philosophers of science. Allan Franklin is certainly among the most active critics of relativism. As a professor of physics at the University of Colorado in Boulder, he has published many important books on the role experiments and instrumentation have played in science. Titles such as *The Neglect of Experiment* (Cambridge U. Press, 1986), *Experiment, Right or Wrong* (Cambridge U. Press, 1990), and *Are There Really Neutrinos? An Evidential History* (Perseus Books, 2000) are technical contributions to the epistemology and history of modern physics and are written for professional scientists and historians and philosophers of science.

In *No Easy Answers: Science and the Pursuit of Knowledge*, Franklin wishes to present "an accurate picture of science," as he states in the preface, to both general readers and their colleagues who are in the humanities and social sciences and have no physics background. For their benefit, Franklin revisits, in a less technical manner, many of the case studies analyzed in his previous books. The picture he proposes is presented in contrast to the relativist's view largely diffused in such books as Harry Collins's *Changing Order: Replication and Induction in Scientific Practice* (Sage, 1985) and Andrew Pickering's *Constructing Quarks: A Sociological History of Particle Physics* (U. of Chicago Press, 1984). Franklin discusses Collins's book in chapter 13, which deals with the early search for gravitational waves; he takes up Pickering's book in chapter 14, which focuses on the history of the experiments on atomic parity violation.

Personally, I prefer those two chapters over the others because Franklin explicitly confronts previous interpretations of the events with his own. Four other chapters are devoted to the history of the neutrino; but curiously, the author does not contrast his narrative with the standard constructivist neutrino history provided by Trevor Pinch in *Confronting Nature: The Sociology of Solar-Neutrino Detection* (D. Reidel, 1986). Franklin's chosen cases are descriptive, with few extended analytical or philosophical discussions. Readers not familiar with the literature will learn through Franklin's book a great deal about the history of the electron, the neutrino, the magnetic monopole, and much else. Franklin also uses the well-known case of Robert Millikan's biased selection of data in his calculation of the electron's charge to raise the ethical question of the selection of data points in the analysis of experiments. He rightly distinguishes between "wrong" and "bad" physics: The former is part of normal science whereas the latter goes against the implicit, moral norms of the scientific community.

Franklin wisely selects his case studies to illuminate the influence that experiments have had in science. In addition to their obvious use in testing theories, experiments suggest new theories by uncovering new phenomena or by providing evidence for the existence of new entities like the electron or the neutrino. Also, experiments can have a life of their own independent of theory and can be devised simply to measure some constants of nature.

Franklin concludes that scientists had good reasons, "based on valid empirical evidence and reasoned and critical discussion," to assess, accept, or reject results as they did (page 227). For him, the cognitive aspects of science dominate any contingent social, or even psychological, factors. An important point rarely stressed in relativists' analyses is the fact that "there is very little instant rationality in science" (page 229): It took about eight years for scientists to clarify the validity of Enrico Fermi's theory of beta decay, and 30 years elapsed between the first reports of the solar neutrino anomaly and the solution of the discrepancy between theory and observation by confirming the existence of neutrino oscillations.

No Easy Answers is probably too technical for the general reader. Yet physicists will find in it a useful epitome of Franklin's past contributions