BOOK REVIEWS

A Reshaping in Physics

QED and the Men Who Made It. Dyson, Feynman, Schwinger, and Tomonaga. SILVAN S. SCHWEBER. Princeton University Press, Princeton, NJ, 1994. xxviii, 732 pp., illus., + plates. $99.50 or £65; paper, $39.50 or £29.95. Princeton Series in Physics.

Silvan Schweber's *QED and the Men Who Made It* is a remarkable and exciting book. The author, an accomplished practicing theoretical physicist, changed directions about 20 years ago and began to study historiography seriously with a view to becoming a similarly accomplished historian of science. He has succeeded. This history of quantum electrodynamics is both the proof and the result. Schweber brings to his efforts the tools of the professional historian (strict reliance on original sources, extended interviews with all the living principals and with just about everyone else who had any light to shed, detailed notes and references) and the tools of a professional physicist who has himself worked on QED, a field as highly technical and abstruse as it is important. That makes him, if not unique, then a pretty rare bird, a physicist-historian.

Richard Feynman "explaining a point at Shelter Island." *Left to right: Standing*, W. Lamb Jr., K. K. Darrow, V. Weisskopf, G. E. Uhlenbeck, R. E. Marshak, J. Schwinger, D. Bohm. *Seated*, J. R. Oppenheimer, A. Pais, Feynman, H. Feshbach. [From *QED and the Men Who Made It*; National Academy of Sciences Archives]

Schweber's approach is comprehensive and ambitious. He is not content to let the principals be defined by their work but has reached into their family and sociological background, schooling, personalities, psyches, and "philosophies." Neither is he content with a descriptive account of the physical content and ideas of QED, but chooses also to go into substantial and extended technical detail, beginning on page 2. Perhaps 25 percent of the text is mathematics, nontrivial and error-plagued; my advice to readers (including physicists) is to float lightly above the mathematics, paying just enough attention to pick up the themes. That is not as difficult as it might seem, for Schweber has done a fine job of organizing and presenting his material, beginning at the beginning.

The first third of *QED* is devoted to indispensable background, primarily theoretical, in which Paul Dirac is seen to be the central figure. But it is one of the virtues of the book that experiment is also stressed, and here it is Willis Lamb who is central. It was Dirac, in 1928, who united quantum mechanics (in the formulation of which he had earlier played a decisive role) and special relativity. The clarity and simplicity of his approach led almost at once to the famous equation that bears his name. The Dirac equation of the electron accounted without further ado for such known properties of the electron as its intrinsic (spin) angular momentum and its magnetic moment. It led inexorably, but with much further ado, to the totally unanticipated idea of antimatter. And it gave the correct spectrum of the hydrogen atom. However, it was observed early on that there were serious and unresolved difficulties with the Dirac equation when it was applied to more complex problems. Even so, these dazzling successes of a formulation so transparently and logically clear as to seem inevitable gave to the Dirac equation an aura of absolute truth.

Such an aura invites and demands challenge, of course, and challenges there were in the form of ever more refined measurements of the spectrum of hydrogen and of the electron's magnetic moment. Eventually, in 1946–47, Lamb, using microwave techniques developed during the war, carried out the exquisitely refined experiments on the hydrogen spectrum that showed that there was an indisputable discrepancy (the Lamb shift) of about one-tenth of one percent with the spectrum calculated from the Dirac equation. Somewhat later, Polykarp Kusch detected an anomaly of about the same size in the magnetic moment of the electron. Something was clearly a little bit wrong. But, given the almost sacrosanct nature of the Dirac equation, that little bit was everything.

Lamb and Kusch shared the Nobel Prize in 1955 because their work "led to a reevaluation and a reshaping of the theory of the interaction of electrons and electromagnetic radiation" (Ivor Waller to Lamb in the Nobel presentation speech). Schweber cites Freeman Dyson's letter to Lamb 20 or so years later: "You were the first to see that that tiny shift, so elusive and hard to measure, would clarify in a fundamental way our thinking about particles and fields."

Lamb's results were announced in early June 1947 at the famous Shelter Island Conference, the first of three such yearly postwar conferences. Schweber does an excellent job, with thorough documentation, of describing these lively meetings. He admirably conveys the remarkable excitement that was generated by the work of Lamb. It was almost immediately recognized by some of those in attendance, notably Hans Bethe, Viktor Weisskopf, and Julian Schwinger, that the most likely (or least improbable) explanation of the Lamb shift was that it was a consequence of the interaction of the Dirac electron with the electromagnetic field, of electrons and photons in other words or, in still other words, that it was to quantum electrodynamics that one must turn for understanding. There was a problem, however, for "the interaction [energy] of an electron with the radiation field . . . comes out infinite in all existing theories, and has therefore always been ignored." Thus wrote Bethe at the time. To some, including Niels Bohr, Werner Heisenberg, and Hideo Yukawa, this had long meant that QED was so deeply flawed that some radical new principle was required, as radical as that which had transformed classical mechanics into quantum mechanics two decades earlier. They were wrong.

The remaining two-thirds of *QED* is devoted to the extraordinary achievements of the "men who made it." No new principles were required, but rather the awesome power to conceive and work out a self-consistent and computable formulation of QED. That meant that the "infinity problem" had to be overcome.

It had been known for a long time that the infinities in QED arose from the properties of the theory at extremely short distances or equivalently at ultra-high energies. In 1948, in his first paper on the subject, Schwinger wrote, "Electrodynamics unquestionably requires revision at ultra relativistic energies, but," he added, "it is presumably accurate at moderate relativistic energies. It would be desirable therefore to isolate these aspects of the theory that involve high energies—from aspects that involve only moderate energies [such as the Lamb shift]. The interaction between matter and radiation," he asserted, then "produces a renormalization of the electron charge and mass, all divergences [infinities] being contained in the renormalization factors."

Mathematically, what was required was a manifestly covariant and gauge invariant formulation of QED, the first ensuring that the requirements of special relativity would be satisfied, the second that, despite interactions with electrons, the photon's mass would be identically zero.

What lay behind this physically was the following: The Lamb shift is a consequence of the fact that the self-energy of an electron in the Coulomb field of the hydrogen atom is different from its self-energy in free space and depends upon its specific quantum state. The difference in these state-dependent self-energies is just the Lamb shift. Now the quantum description of self-energy makes it a consequence of the ceaseless emission and absorption of (virtual) photons. The process is further complicated by the ceaseless (but less probable) creation and absorption of (virtual) electron-positron pairs (polarization of the vacuum, which thereby acquires structure and is far from quiescent in this description). The self-energy augments the mechanical mass of the electron; the sum of the two is the observed or renormalized mass. The polarizability of the vacuum gives it some of the properties of a dielectric and affects the electron's effective charge. The observed or renormalized charge includes this effect.

The trouble is that these corrections are infinite if one seeks to calculate them. Thus one is taking the difference of infinite quantities in calculating the Lamb shift or is incorporating such quantities into the electron's mass and charge in the renormalization process. It is the manifest gauge and Lorentz invariance that makes it possible to do so in an unambiguous and self-consistent way. Furthermore, it turns out, as Dyson later showed, that once these infinities have been isolated and absorbed through renormalization, all other quantities and processes are finite and calculable (at least in principle). To give meaning to this last statement, Schweber cites the best value of the electron's magnetic moment, measured by Hans Dehmelt, to be 1.001159652193(4) and the best theoretical result of Toichiro Kinoshita to be 1.001159652459(135), both in the same units. They are thus seen to agree to about two parts in ten trillion. The Dirac value in these same units is exactly unity.

Of course, this tiny difference between the Dirac value and experiment has no practical consequences. But it set off a revolution. "To be told," Schwinger said, "that the sacred Dirac theory was breaking down all over the place—that was incredible!"

Schweber describes in great (and overly technical) detail the distinctive contributions of Tomonaga, Schwinger, Feynman, and Dyson. It was Sin-itiro Tomonaga, working in isolation in Japan during the war, who first constructed a covariant formulation of QED. Lamb's experiments lay

Vignette: Interview with Dirac

I been hearing about a fellow they have up at the U. this spring—a mathematical physicist, or something, they call him—who is pushing Sir Isaac Newton, Einstein and all the others off the front page. So I thought I better go up and interview him for the benefit of State Journal readers, same as I do all other top notchers. . . .

So the other afternoon I knocks at the door of Dr. Dirac's office in Sterling Hall and a pleasant voice says "Come in." And I want to say here and now that this sentence "come in" was about the longest one emitted by the doctor during our interview. He sure is all for efficiency in conversation. It suits me. I hate a talkative guy.

. . . .

The thing that hit me in the eye about him was that he did not seem to be at all busy. Why if I went to interview an American scientist of his class—supposing I could find one—I would have to stick around an hour first. Then he would blow in carrying a big briefcase, and while he talked he would be pulling lecture notes, proof, reprints, books, manuscript, or what have you out of his bag. But Dirac is different. He seem to have all the time there is in the world and his heaviest work is looking out the window. If he was a typical Englishman it's me for England on my next vacation.

Then we sat down and the interview began.

"Professor," says I, "I notice you have quite a few letters in front of your last name. Do they stand for anything in particular?"

"No," says he.

"You mean I can write my own ticket?"

"Yes," says he.

"Will it be all right if I say P.A.M. stands for Poincaré Aloysius Mussolini?"

"Yes," says he.

"Fine, says I, "We are getting along great! Now doctor will you give me in a few words the low-down on all your investigations?"

"No," says he.

. . . .

"This is the most important thing yet, doctor," says I. . . . "Do you ever run across a fellow that even you cant understand?"

"Yes," says he.

"This will make a great reading for the boys down at the office," says I. "Do you mind releasing to me who he is?"

"Weyl," says he.

. . . .

If that fellow Professor Weyl ever lectures in this town again I sure am going to take a try at understanding him! A fellow ought to test his intelligence once in a while.

—"*Roundy*," in the *Wisconsin State Journal* 30[?] April 1931, as quoted in *QED and the Men Who Made It* in the future, of course, and Tomonaga's motivation was to provide a sounder basis for the then unsatisfactory quantum theory of fields but without any specific applications in mind.

Schwinger's entirely independent development of a covariant formulation was remarkably similar in spirit to Tomonaga's, and as remarkably different in spirit and in detail from Feynman's slightly later work. It was Schwinger who first saw the problem whole, who first grasped the relationship between the Lamb shift and the anomalous magnetic moment and who first calculated both. About this work Schweber says:

> The importance of Schwinger's calculation cannot [should not] be underestimated. In the course of theoretical developments there sometimes occur important calculations that alter the way the community thinks about particular approaches. Schwinger's calculation is one such instance. By indicating, as Feynman had noted, that "the discrepancy in the hyperfine structure of the hydrogen atom . . . could be explained on the same basis as that of electromagnetic self-energy, as can the line shift of Lamb," Schwinger had transformed the perception of quantum electrodynamics. He had made it into a effective, coherent, and consistent computational scheme to order e^2.

Feynman's approach was entirely different and absolutely extraordinary. So innovative and so breathtakingly original was his formulation and so incomplete and sketchy was its foundation, that the first reaction to it was skeptical and even negative. It was in some measure a set of rules for calculation, using the now famous and ubiquitous Feynman diagrams, with positrons depicted as electrons moving backward in time. But it soon became apparent that the rules worked and were incomparably easier to use in doing calculations than was Schwinger's formulation. And soon, too, the foundation was filled in. Schwinger remarked, not exactly admiringly, that Feynman had brought QED to the masses.

It was Dyson who completed the development of QED in two ways: first, by showing that Feynman's and Schwinger's formulations were equivalent and, second, by showing that renormalization worked to all orders, that there were no infinities remaining after mass and charge were renormalized. Both were major accomplishments. Schwinger's approach, as was Tomonaga's, was that of a field theorist; quantum fields were the primary constructs. Feynman's approach, at least initially, eliminated all references to fields and focused instead on particles (electrons and photons) and on their space-time trajectories. Neither understood the other, and their only point of contact was that each approach yielded the same answers. Dyson's demonstration of their equivalence was a great triumph. Even more so was his second accomplishment, which required the most penetrating kind of analysis. "His perception and power," Frank Yang wrote, "were dazzling."

Schweber argues that Dyson should have shared in the Nobel Prize awarded to Tomonaga, Schwinger, and Feynman in 1965. Many, this reviewer among them, do not agree. The difference between clarification and innovation is all the difference.

One of the pleasures of *QED* comes in reading what Schweber calls "loving biographies of the principals involved and an admiring account of the community of theoretical physicists." Loving and admiring, yes, but the airbrush has been sparingly used, the warts (most of them) show, and so do some sharp tongues. Much is presented in the principals' own words, and that is all to the good, for they are livelier and more graceful than Schweber's. Some wonderful anecdotes and vignettes are presented; the reader is referred to the interview with Dirac on pages 18–20 (excerpted on page 1889 of this issue of *Science*) for a delightful example.

Assessments of the character of the principals and of the relative importance of their contributions involve, of course, matters of judgment about which reasonable people can (and will) disagree. But all will agree that Schweber has presented the story fully and fairly enough to enable readers to draw their own conclusions.

Schweber is neither psychologist nor sociologist, and his efforts in such directions happily are limited in number and extent, although the fact that the cast of characters is almost exclusively white and male ought at least to have been noted. Schweber is no philosopher, either, but then neither are the principals. Schwinger was indifferent to philosophical discussions of science, Feynman disdainful. When Dirac was once asked to express his philosophy of physics, he wrote a single sentence: "Physical laws should have mathematical beauty."

The point to be made here is that the philosophy of physics is not physics. The same, of course, is true of the history of physics. As a result, working physicists tend not to give serious attention to either, even when they should. No matter. It is the scientifically literate reader for whom this book is intended, and not merely the working physicist. It is not an easy read but is well worth the effort and, once started, hard to put down.

QED is important, worth reading about, and worth writing about, because it accounts—with extraordinary precision—for the properties and interactions of those most common, accessible, understandable, and fundamental of objects, the electron and the photon. But what is more, QED sparked a revolution by providing a model for the application of quantum field theory in other domains. In particular, the renormalization concept has been remarkably fruitful in such fields as condensed matter physics. Further, the view that renormalizability is not a problem but a basic requirement led Steven Weinberg and Abdus Salam to their great unification of electromagnetic and weak interactions, electroweak theory. The concepts embodied in QED have also been extended to the strong interactions, where the analogous developments are denoted by QCD (for quantum chromodyamics).

Perhaps the ultimate irony is that the men who made QED took a rather limited view of its validity. Feynman had sought a divergence-free QED but concluded that he had merely swept the infinities under the carpet, as he put it in his Nobel acceptance speech. Dyson had hoped to prove that the renormalized QED perturbation expansion converged, but proved the contrary to his great disappointment. And Schwinger devoted his later years to the successful construction of a divergence-free alternative approach (source theory). Not surprisingly, however, the next generation found the ideas of renormalizable QED far easier to accept uncritically than did the founders.

A final word. Among his many other accomplishments, Schweber does a very good deed in giving to Dirac the credit he deserves. Schweber emphasizes that Dirac towers above everyone else in his influence and that, along with many others, the four who made QED were all "students" of Dirac. Perhaps, with justice, the book could have been titled "QED and the Men Who Made It: Dirac, Dyson, Feynman, Schwinger, and Tomonaga." (The order remains alphabetical, please note.)

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Experimentalist's Career

Antoine Lavoisier. Science, Administration, and Revolution. ARTHUR DONOVAN. Blackwell, Cambridge, MA, 1994. xvi, 351 pp., illus. $29.95 or £35. Blackwell Science Biographies.

Although widely known as a founder of the Chemical Revolution who was guillotined in 1794 during the Terror, Antoine Lavoisier devoted only a small part of his public career to science. As indicated by its title, this new biography attempts to broaden our view by portraying not only the chemist but also the "other" Lavoisier—the barrister, tax official, agricultural reformer, financier, director of the Gunpowder Ad-