

Julian Schwinger, But Not the Definitive Julian Schwinger

Climbing the Mountain: The Scientific Biography of Julian Schwinger

 Jagdish Mehra
and Kimball A. Milton
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Reviewed by Tian Yu Cao

Julian Schwinger (1918–1994) was a legendary figure in the history of fundamental physics. While still a teenager, he amazed leaders of theoretical physics in the US with his prodigious theoretical insights. As a young Harvard professor, he quickly became the supreme intellectual leader in quantum field theory (QFT) and particle physics. Soon after receiving the Nobel Prize in 1965, he left the mainstream of modern physics and challenged its foundation. But his new pursuit was contemptuously dismissed by mainstream physicists as mistaken or irrelevant. His research papers were rejected by *Physical Review Letters* and other journals in a deeply insulting manner. His response was to resign in protest as a member and fellow of the American Physical Society.

Schwinger's life and work, in particular his paradoxical legacy in the theoretical foundations of fundamental physics, constitute a fascinating chapter in 20th-century physics; they deserve careful study and assessment. Thus, the appearance of *Climbing the Mountain: The Scientific Biography of Julian Schwinger*, by Jagdish Mehra and Kimball A. Milton, following as it does Silvan S. Schweber's pioneering *QED and The Men Who Made It*, (Princeton U. Press, 1994) is extremely timely.

Schwinger's contributions were numerous and profound. He reformulated quantum electrodynamics (QED)

in the late 1940s, when QED and QFT in general were widely conceived to be intrinsically wrong in terms of covariant formalism. His reformulation, together with the idea of renormalization, was the first self-consistent framework in fundamental physics from which physical consequences could be extracted and checked with experiments.

Schwinger's contributions were initially highly appreciated. In mid-1966, however, Schwinger started reformulating the foundations of fundamental physics and establishing a new framework, source theory, to replace the old one—in fact his own creation and named by him operator field theory. His pursuit of source theory precipitated his downfall; hostility toward it among his colleagues pushed him out of the mainstream of modern physics and contributed, at least psychologically, to his involvement in such fringe projects as cold fusion.

Schwinger's move was motivated by deep concerns and convictions. As he wrote in "The Future of Fundamental Physics," in *Nature of Matter: Purpose of High Energy Physics*, Luke Yuan, ed. (Brookhaven National Laboratory, 1965), he believed that "the scientific level of any period is epitomized by the current attitude toward the fundamental properties of matter. The world view of the physicist sets the style of the technology and the culture of the society and gives direction to future progress." However, the then-current conception of matter offered by operator formalism was problematic and had to be reformulated. When the interactions are weak, such as in the case of QED, it is possible to move from field to particle through a zigzag way of renormalization (which separates the effects of inaccessible high-energy dynamics from those within the reach of experiments and incorporates them into phenomenological parameters). When the interactions are strong, in which case an unlimited number of particles with all kinds of complicated interactions are involved, no correspondence between observable particles and fundamental fields can be established, and thus there is no way to incorpo-

rate the strong interactions into the operator formalism.

The failure indicated the need for new ideas. But the then-fashionable alternatives were not acceptable: S-matrix theory because of its lack of dynamics; current algebra because of, among other things, its futile attempt at using kinematic groups to convey dynamic information.

Encouraged by the success of using phenomenological Lagrangians to reproduce the soft-pion results of current algebra, Schwinger turned to developing a phenomenological approach for promoting a modest scientific goal: to move from solid knowledge of phenomena at accessible energies to that at higher energies. The result was source theory.

According to Schwinger, source theory as a mathematical description of laboratory practice is conceptually sound and mathematically simple, without difficulties in operator formalism. It incorporates no infinities, and thus needs no renormalization; no new constants would appear, because all parameters are fixed when the class of phenomena under examination is enlarged.

Conceptual clarity and calculational power notwithstanding, source theory's reception was chilly. Why? To Mehra and Milton, the explanation is straightforward and threefold: First, times had changed; the attractive features of and experimental supports for the quark model of hadrons and quantum chromodynamics (QCD) had removed the phenomenological appeal of source theory. Second, rigidity within the scientific establishment made it intolerant of ideas departing radically from the reigning orthodoxy. And, third, Schwinger's attitude was too provocative. If not for these things, they argue, reception of source theory would have been warmer and its impact on the field greater; its techniques were not an abrupt break from the mainstream ones, and much of Schwinger's criticism of QCD, whose ground has been tenuous, was quite valid.

But it seems to me that Schwinger's conflict with the mainstream was too deep to be explained by those contingent factors. Despite his awesome

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mathematical talents, fundamental physics for Schwinger was more than mathematical exercises; rather it was a way to understand our intervention into the physical world. Thus, quantum mechanics for him was only the mathematical symbolism of measurement in atomic physics, while source theory was the mathematical symbolism of our manipulations in high-energy physics. The quark model and QCD, with particles having no asymptotic states, were too distasteful to him, and, later, string theories, with their unbelievable desert between 1 TeV and the Planck scale, were outrageous. Conversely, Schwinger's interpretation of scaling and J/ψ particles were too iconoclastic to be acceptable to other physicists. Reconciliation seems not to be possible.

In 1973, before his final withdrawal from fundamental physics, Schwinger expressed his sensitivity in a 1973 talk at UCLA: He felt that it was a great tragedy for a scientist that none of his marvelous insights and pioneering of new paths had the slightest influence on the actual evolution of science. His tragic feelings aside, his case was quite different. His views influenced Steven Weinberg's work on the phenomenological Lagrangian approach to chiral dynamics and, later, on the effective field theory (EFT) approach. Source theory and EFT (which is an operator field theory) share three features: the denial of being a fundamental theory, flexibility in incorporating new particles and new interactions into existing schemes, and the capacity to consider nonrenormalizable interactions. The rationalizing of EFT has produced a resurgence of interest in Schwinger's legacy, even if its long-term impact on fundamental physics is unpredictable at this stage.

Mehra and Milton provide a great deal of material from interviews and archival files and thus help give us a fuller picture of Schwinger. Perhaps their most important contribution is their account of the evolution of many of Schwinger's thoughts.

However, the weaknesses of the book are many: There are too many quotations and summaries and too few analyses and judgments, which turns hundreds of pages into a simple chronicle of events and publications in which small things, like simple-measurement algebra, can take 10 pages, while crucial points, like the "valid premise that scaling does not necessitate the existence of point-like constituents," are simply stated. Moreover, the authors are not always fully aware of the context—of "his time." For example, their

claim—that the move of the idea of color from being a means for addressing the statistics of the naive quark model to becoming a ground for QCD is an "obvious" step—has trivialized the intellectual history of QCD. Another claim, that constructive field theory is just another name for axiomatic field theory, has missed the subtle but important difference between the two pursuits. Further, to mention several later comers in the investigations of asymptotic freedom without mentioning its first investigator, Gerard 't Hooft, is also not fair. More serious than any of these is the fact that the authors are frequently sloppy about historical details; such carelessness may incline the reader to question their reliability on other matters. To give just two examples: John Wheeler's S-matrix of 1937 was concerned only with nuclear physics and, contrary to what the authors claim, had nothing to do with Werner Heisenberg's ambitious project. And to say that in 1964 "the approximation symmetry group was not yet established" directly contradicts the historical facts; since at least 1959, Sheldon Glashow and Murray Gell-Mann were publishing on the subject in terms of the soft-mass problem.

Despite these criticisms, the book is certainly of value to those physicists, historians, and philosophers who are concerned with foundational problems in fundamental physics. Even for those who are not, many of the anecdotes are at least entertaining. On the whole, the book does shed light on a many-faceted genius. A definitive and scholarly scientific biography, however, is yet to be written.

Hitler's Gift: The True Story of the Scientists Expelled by the Nazi Regime

► Jean Medawar and David Pyke
Arcade Publishing, New York,
2000. \$25.95 (268 pp.).
ISBN 1-55970-564-7

As its provocative title indicates, *Hitler's Gift* is an account of the achievements of a group selected from among the thousands who fled Nazi oppression in Europe to find refuge in England in the 1930s. The book is a solid addition to the growing body of exile literature. The authors, Jean Medawar and David Pyke, are highly qualified, having had easy access to many of the distinguished scientists, scholars, and artists who were interviewed for the book.

The rescue effort started with William Beveridge, director of the London School of Economics, who happened to read in March 1933 of the dismissal of Jewish scientists and other professionals in Germany. Outraged, he and some of his prominent colleagues founded what became known as the Society for the Protection of Science and Learning. The official British authorities kept themselves distant, but the society, with money raised from individuals, eventually registered 2541 refugees, chiefly from Central Europe. Most of the refugees had already achieved distinction in academe or the arts. Indeed, what started as individual acts of compassion and decency turned into what the book calls "a highly profitable venture" for the country. As the authors summarize it, "Of the 2600 . . . rescued, twenty became Nobel laureates, fifty-four were elected Fellows of the Royal Society, thirty-four became Fellows of the British Academy, and ten received knighthoods."

While the authors claim that the book is not "a scholarly study," it is nevertheless unique. It contains extensive accounts, mostly based on interviews with a sample of 40 scientists and physicians. Those in the sample arrived in Britain before World War II and were able to become highly successful despite the trauma of displacement, the unhelpfulness of Whitehall, and the bad economic conditions at the time. Among the physicists were Erwin Schrödinger, Francis Simon, Max Born, Fritz Haber, Otto Frisch, and Rudolf Peierls; the chemists and biologists included Hans Krebs, Max Perutz, Ernst Chain, and Edith Bülbirg.

A couple of useful and perceptive chapters recount the high points of politics and science in Germany before 1933. In a novel and thoroughly researched chapter on the rescuers, the authors discuss in detail the roles of prominent Britons, including Ernest Rutherford, A. V. Hill, and Beveridge. They and members of the Jewish community in England and, later, the Rockefeller Foundation mobilized the funds to take advantage of the enlightened decision by the British cabinet in April 1933 to "try and secure for this country prominent Jews who were being expelled from Germany and had achieved distinction . . ." A similar action in the US helped to bring 1700 academic refugees there.

There follows a chilling chapter—"Those Who Stayed," (Max Planck, Werner Heisenberg, Max von Laue, Carl Friedrich von Weizsäcker, Otto Warburg)—and then the first good account I have seen on the internment