Julian Schwinger: Nuclear Physics, the Radiation Laboratory, Renormalized QED, Source Theory, and Beyond

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Abstract

Julian Schwinger's influence on twentieth century science is profound and pervasive. Of course, he is most famous for his renormalization theory of quantum electrodynamics, for which he shared the Nobel Prize with Richard Feynman and Sin-itiro Tomonaga. But although this triumph was undoubtedly his most heroic work, his legacy lives on chiefly through subtle and elegant work in classical electrodynamics, quantum variational principles, proper-time methods, quantum anomalies, dynamical mass generation, partial symmetry, and more. Starting as just a boy, he rapidly became the pre-eminent nuclear physicist in the late 1930s, led the theoretical development of radar technology at MIT during World War II, and then, soon after the war, conquered quantum electrodynamics, and became the leading quantum field theorist for two decades, before taking a more iconoclastic route during his last quarter century.

Keywords: Julian Schwinger, nuclear physics, waveguides, quantum electrodynamics, renormalization, quantum action principle, source theory, axial-vector anomaly

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1 Introduction

Given Julian Schwinger's commanding stature in theoretical physics for half a century, it may seem puzzling why he is relatively unknown now to the educated public, even to many younger physicists, while Feynman is a cult figure with his photograph needing no more introduction than Einstein's.[†] This relative obscurity is even more remarkable, in view of the enormous number of eminent physicists, as well as other leaders in science and industry, who received their Ph.D.'s under Schwinger's direction, while Feynman had practically none. In part, the answer lies in Schwinger's retiring nature and reserved demeanor. Science, research and teaching, were his life, and he detested the limelight. Generally, he was not close to his students, so few knew him well. He was a gracious host and a good conversationalist, and had a broad knowledge of many subjects, but he was never one to initiate a relationship or flaunt his erudition.

His style of doing physics was also difficult to penetrate. Oppenheimer once said that others gave talks to show others how to do the calculation, while Schwinger gave talks to show that only *he* could do it. Although a commonly shared view, this witticism is unkind and untrue. He was, in fact, a superb teacher, and generations of physicists, students and faculty alike, learned physics at his feet. On the one hand he was a formalist, inventing formalisms so powerful that they could lead, at least in his hands, unerringly to the correct answer. He did not, therefore, display the intuitive visualizations, for example, that Feynman commanded, which eventually took over the popular and scientific culture.

But, more profoundly, he was a phenomenologist. Ironically, even some of his own students criticized him in his later years for his phenomenological orientation, not recognizing that he had, from his earliest experiences in nuclear physics, insisted in grounding theoretical physics in the phenomena and data of experiment. Isidor Rabi, who discovered Schwinger and brought him to Columbia University, generally had a poor opinion of theoretical physicists. But Rabi was always very impressed with Schwinger because in nearly every paper, he "got the numbers out" to compare with experiment. Even in

[†]An example is the series of posters produced by the American Physical Society in which the impression is given that Feynman was the chief innovator in quantum electrodynamics. In contradiction to this, Norman Ramsey has stated that "it is my impression that Schwinger overwhelmingly deserved the greatest credit for QED. I don't think Feynman had an explanation of the anomalous hyperfine structure before the [1948 APS] meeting."[1]

his most elaborate field-theoretic papers he was always concerned with making contact with the real world, be it quantum electrodynamics, or strongly interacting hadrons.

Although his first, unpublished, paper, written at the age of 16, was on the subject of the then poorly-understood quantum electrodynamics, Julian Schwinger was almost exclusively a nuclear physicist until he joined the Radiation Laboratory at MIT in 1943. There, faced with critical deadlines and the difficulty of communicating with electrical engineers, he perfected variational techniques for solving the classical electrodynamic problems of waveguides. As the War wound down, he started thinking about radiation produced by electrons in betatrons and synchrotrons, and in so doing recognized that the reactive and resistive portions of the electromagnetic mass of the electron are united in a invariant structure. Recruited by Harvard, he started teaching there in 1946, and at first continued research in nuclear physics and in classical diffraction. The Shelter Island conference of 1947 changed all that. He and Weisskopf suggested to Bethe that electrodynamic processes were responsible for the Lamb shift, which had been known for some time as the Pasternack effect. Immediately, however, Schwinger saw that the most direct consequence of quantum electrodynamics lay in the hyperfine anomaly reported for the first time at Shelter Island. He anticipated that the effect was due to an induced anomalous magnetic moment of the electron. The actual calculation had to wait three months, while Schwinger took an extended honeymoon, but by December 1947 Schwinger had his famous result for the gyromagnetic ratio. In the process he invented renormalization of mass and charge, only dimly prefigured by Kramers. This first formulation of QED was rather crude, being noncovariant; to obtain the correct Lamb shift, a relativistic formulation was required, which followed the next year. A comedy of errors ensued: Both Feynman and Schwinger made an incorrect patch between hard and soft photon processes, and so obtained identical, but incorrect, predictions for the Lamb shift, and the weight of their reputations delayed the publication of the correct, if pedestrian, calculation by French and Weisskopf.[‡] By 1950 Schwinger had started his third reformulation of quantum electrodynamics, in terms of the quantum action principle. At the same time he wrote his remarkable paper, "On Gauge Invariance and Vacuum Polarization," formulated in a completely gauge-covariant way, which

[‡]Schwinger later claimed that his first noncovariant approach had yielded the correct result, except that he had not trusted it.

anticipated many later developments, including the axial vector anomaly.

His strong phenomenological bent eventually led him away from the mainstream of physics. Although he had given the basis for what is now called the Standard Model of elementary particles in 1957, he never could accept the existence of quarks because they had no independent existence outside of hadrons. (A secondary consideration was that quarks were invented by Murray Gell-Mann, with whom a long-running feud had developed.) He came to appreciate the notion of supersymmetry, but he rejected notions of "Grand Unification" and of "Superstrings" not because of their structure but because he saw them as preposterous speculations, based on the notion that nothing new remains to be found in the enormous energy range from 1 TeV to 10^{19} GeV. He was sure that totally new, unexpected phenomena were waiting just around the corner. This seems a reasonable view, but it resulted in a self-imposed isolation, in contrast, again, to Feynman, who contributed mightly to the theory of partons and quantum chromodynamics up to the end.

A complete biography of Julian Schwinger was published six years ago.[2] The present paper draws upon that book, as well as on later interviews and research by the author. Quotations of Schwinger not otherwise attributed are based on an extended interview conducted for that book by my co-author Jagdish Mehra in 1988.

2 Early Years

Julian Schwinger was born in Manhattan, New York City, on February 12, 1918, to rather well-off middle-class parents. His father was a well-known designer of women's clothes. He had a brother Harold ten years older than himself, whom Julian idolized as child. Harold claimed that he taught Julian physics until he was 13. Although Julian was recognized as intelligent in school, everyone thought Harold was the bright one. (Harold in fact eventually became a respected lawyer, and his mother always considered him as the successful one, even after Julian received the Nobel Prize.) The Depression cost Julian's father his business, but he was sufficiently appreciated that he was offered employment by other designers; so the family survived, but not so comfortably as before.

The Depression did mean that Julian would have to rely on free education, which New York well-provided in those days: A year or two at Townsend Harris High School, a public preparatory school feeding into City College, where Julian matriculated in 1933. Julian had already discovered physics, first through Harold's *Encyclopedia Britannica* at home, and then through the remarkable institution of the New York Public Library.

Larry Cranberg was a student at Townsend Harris at the same time as Julian Schwinger.[3] They had some classes together, and both graduated in January 1933, with a diploma that stated that graduates were entitled to automatic entry to CCNY. He recalled that Julian was "very, very quiet. He never gave recitations. He sat in the last row, unsmiling and unspeaking, and was a real loner. But the scuttlebutt was that he was our star. He very early showed promise," but Cranberg saw nothing overt. "Rumors were that he was not very good outside math and physics, and that he was flunking German."

Among the teachers at Townsend Harris, Cranberg particularly remembers Alfred Bender,[§] who was apparently not on the regular faculty. Eileen Lebow, who recently wrote a history of Townsend Harris High School,[4] does not recall Bender's name. Cranberg said that Bender "fixed me on the course to be a physicist. He was diligent, passionate, and meticulous in his recitations. He was a great guy, one of the best teachers at Townsend Harris." It seems very likely that it was Bender to whom Schwinger referred to as an anonymous influence:

> I took my first physics course in High School. That instructor showed unlimited patience in answering my endless questions about atomic physics, after the class period was over. Although I try, I cannot live up to that lofty standard.[5]

At City College Julian was reading and digesting the latest papers from Europe, and starting to write papers with instructors who were, at the same time, graduate students at Columbia and NYU. Joseph Weinberg, who went on to become a well-known relativist, was his closest friend at City College. Weinberg recalled his first meeting with Julian.[6] Because of his outstanding laboratory reports, Weinberg had been granted the privilege of entering the closed library stacks at City College. One day he was seeking a mathematics book,[7] which had been mentioned at the Math Club the day before, and while he reached for it, another youngster was trying to get it. They had both heard the talk, on functions which are continuous but nowhere differentiable, and

[§]Bender was the father of physicist Carl Bender. Carl's uncle Abram Bader was the physics teacher of Richard Feynman at Far Rockaway High School.

so they shared the book between them, balancing the heavy volume on one knee each. The other fellow kept finishing the page before Weinberg, who was a very fast reader. Of course, his impatient co-reader was Julian Schwinger. Both were 15. Weinberg mentioned that he usually spent his time, not in the mathematics section of the library, but in the physics section, which turned out to be Julian's base as well. Weinberg complained that Dirac's book on quantum mechanics[8] was very interesting and exciting, but difficult to follow. Julian concurred, and said it was because it was polished too highly; he said that Dirac's original papers were much more accessible. Weinberg had never conceived of consulting the original literature, so this opened a door for him. This advice about over-refinement Schwinger himself forgot to follow in later life.

Julian no longer had the time to spend in the classroom attending lectures. In physics and mathematics he was able to skim the texts and work out the problems from first principles, frequently leaving the professors baffled with his original, unorthodox solutions, but it was not so simple in history, English, and German. City College had an enormous number of required courses then in all subjects. His marks were not good, and he would have flunked out if the College had not also had a rather forgiving policy toward grades.

Joe Weinberg recalled another vivid incident. Among the required courses were two years of gymnasium. One had to pass exams in hurdling, chinning, parallel bars, and swimming. Because Weinberg and Julian had nearby lockers, they often fell into physics conversations half dressed, and failed the class for lack of attendance. Weinberg remembered seeing Julian's hurdling exam. Julian ran up to the bar, but came to a standstill when he was supposed to jump over sideways. The instructor reprimanded him, at which point Julian said, *sotto voce*, "there's not enough time to solve the equations of motion."

Edward Gerjuoy was another of Julian's classmates at City College.[9] "My main claim to fame is that Julian and I took the same course in mechanics together, taught by a man named Shea, and I got an A and Julian a B," because Julian did not do the work. "It took about a week before the people in the class realized we were dealing with somebody of a different order of magnitude." At a time when knowledge of a bit of vector algebra was considered commendable, "Julian could make integrals vanish—he was very, very impressive. The only person in the classroom who didn't understand this about Julian was the instructor himself." "He was flunking out of City College in everything except math and physics. He was a phenomenon. He didn't lead the conventional life of a high school student before he came to City College"—unlike Gerjuoy and Sidney Borowitz he was not on the math team in high school so they had not known him earlier—"when he appeared he was just a phenomenon."

Morton Hamermesh recalled another disastrous course.[10]

> We were in a class called Modern Geometry. It was taught by an old dodderer named Fredrick B. Reynolds. He was head of the math department. He really knew absolutely nothing. It was amazing. But he taught this course on Modern Geometry. It was a course in projective geometry from a miserable book by a man named Graustein from Princeton, and Julian was in the class, but it was very strange because he obviously never could get to class, at least not very often, and he didn't own the book. That was clear. And every once in a while, he'd grab me before class and ask me to show him my copy of this book and he would skim through it fast and see what was going on. And this fellow Reynolds, although he was a dodderer, was a very mean character.[¶] He used to send people up to the board to do a problem and he was always sending Julian to the board to do problems because he knew he'd never seen the course and Julian would get up at the board, and—of course, projective geometry is a very strange subject. The problems are trivial if you think about them pictorially, but Julian never would do them this way. He would insist on doing them algebraically and so he'd get up at the board at the beginning of the hour and he'd work through the whole hour and he'd finish the thing and by that time the course was over and anyway, Reynolds didn't understand the proof, and that would end it for the day.

Sidney Borowitz, another classmate of Julian's, recalled that "we had the pleasure of seeing Julian attack a problem *de novo*, and this used to drive Reynolds crazy."[12]

[¶]In addition, he was also apparently a notorious antisemite. He used to discourage Jewish students from studying mathematics, which worked to the advantage of physics.[11]

3 Paper Number Zero

Not only was Julian already reading the literature at City College, he quickly started to do original research. Julian had studied a paper by Christian Møller[13] in which he had calculated the two-electron scattering cross section by using a retarded interaction potential. Of course, Schwinger read all of Dirac's papers on quantum field theory, and was particularly impressed by the one on "Relativistic Quantum Mechanics,"[14] "in which Dirac went through his attempt to recreate an electrodynamics in which the particles and light were treated differently." In a paper of Dirac, Fock, and Podolsky,[15]

> it was recognized that this was simply a unitary transformation of the Heisenberg-Pauli theory,[16] in which the unitary transformation was applied to the electromagnetic field. And I said to myself, 'Why don't we apply a similar unitary transformation to the second-quantized electron field?' I did that and worked out the lowest approximation to the scattering amplitudes in unrelativistic notation. It was a relativistic theory but it was not covariant. That was in 1934, and I would use it later; [the notion, called the *interaction representation,*] is always ascribed to Tomonaga, but I had done it much earlier.

In deriving his result, Schwinger had to omit a term which "represents the infinite self-energy of the charges and must be discarded." This he eventually came to see as a mistake: "The last injunction merely parrots the wisdom of my elders, to be later rejected, that the theory was fatally flawed, as witnessed by such infinite terms, which at best, had to be discarded, or subtracted. Thus, the 'subtraction physics' of the 1930s."[17]

This paper was never submitted to a journal, but was recently published in a selection of Schwinger's works.[18]

4 Columbia University

It was Lloyd Motz, one the instructors at City College, who had learned about Julian from Harold, and with whom Julian was writing two papers, who introduced him to Isidor I. Rabi. Then, in a conversation between Rabi and Motz over the famous Einstein, Podolsky, and Rosen paper,[19] which had just appeared, Julian's voice appeared with the resolution of a difficulty through the completeness principle, and Schwinger's career was assured. Rabi, not without some difficulty, had Schwinger transferred to Columbia, and by 1937 he had 7 papers published, mostly on nuclear physics, which constituted his Ph.D. thesis, even though his bachelor's degree had not yet been granted.

Schwinger still was derelict in attending classes, and ran into trouble in a chemistry course taught by Victor LaMer. It was a dull course with a dull exam. A question on the final exam was "Prove that $d\epsilon = d\xi + d\eta$," where none of the variables ϵ, ξ, or η were defined. Rabi recalled,[20]

> LaMer was, for a chemist, awfully good. A great part of his lifework was testing the Debye-Hückel theory[21] rather brilliantly. But he was this rigid, reactionary type. He had this mean way about him. He said, 'You have this Schwinger? He didn't pass my final exam.' I said, 'He didn't? I'll look into it.' So I spoke to a number of people who'd taken the same course. And they had been greatly assisted in that subject by Julian. So I said, I'll fix that guy. We'll see what character he has. 'Now Vicky, what sort of guy are you anyway, what are your principles? What're you going to do about this?' Well, he did flunk Julian, and I think it's quite a badge of distinction for him, and I for one am not sorry at this point, they have this black mark on Julian's rather elevated record. But he did get Phi Beta Kappa as an undergraduate, something I never managed to do.

The papers which Julian wrote at Columbia were on both theoretical and experimental physics, and Rabi prized Julian's ability to "get the numbers out" to compare with experiment. The formal awarding of the Ph.D. had to wait till 1939 to satisfy a University regulation. In the meantime, Schwinger was busy writing papers (one, for example, was fundamental for the theory of nuclear magnetic resonance,[22]) and spent a somewhat lonely, but productive winter (1937) in Wisconsin,[‖] where he laid the groundwork for his prediction that the deuteron had an electric quadrupole moment,[23] independently established by his experimental colleagues at Columbia a year later.[24] Wisconsin confirmed his predilection for working at night, so as not to be "overwhelmed" by his hosts, Eugene Wigner and Gregory Breit.

Rabi later amusingly summarized Schwinger's year in Wisconsin.[20]

[‖]It was a cold winter as well, for he failed to unpack the trunk in which his mother had placed a warm winter coat.

> I thought that he had about had everything in Columbia that we could offer—by we, as theoretical physics is concerned, [I mean] me. So I got him this fellowship to go to Wisconsin, with the general idea that there were Breit and Wigner and they could carry on. It was a disastrous idea in one respect, because, before then, Julian was a regular guy. Present in the daytime. So I'd ask Julian (I'd see him from time to time) 'How are you doing?' 'Oh, fine, fine.' 'Getting anything out of Breit and Wigner?' 'Oh yes, they're very good, very good.' I asked them. They said, 'We never see him.' And this is my own theory—I've never checked it with Julian—that—there's one thing about Julian you all know—I think he's an even more quiet man than Dirac. He is not a fighter in any way. And I imagine his ideas and Wigner's and Breit's or their personalities did not agree. I don't fault him for this, but he's such a gentle soul, he avoided the battle by working at night. He got this idea of working nights—it's pure theory, it has nothing to do with the truth.

But the theory seems validated.

5 Two Years in Berkeley

By 1939, Rabi felt Schwinger had outgrown Columbia, so with a NRC Fellowship, he was sent to J. Robert Oppenheimer in Berkeley. This exposed him to new fields: quantum electrodynamics (although as we recall, he had written an early, unpublished paper on the subject while just 16) and cosmic-ray physics, but he mostly continued to work on nuclear physics. He had a number of collaborations; the most remarkable was with William Rarita, who was on sabbatical from Brooklyn College; Rarita was Schwinger's "calculating arm"** on a series of papers extending the notion of nuclear tensor forces which he had conceived in Wisconsin over a year earlier. Rarita and Schwinger also wrote the prescient paper on spin-3/2 particles,[25] which was to be influential decades later with the birth of supergravity.

Ed Gerjuoy, who had been an undergraduate with Schwinger at City College in 1934, now was one of Oppenheimer's graduate students. He recalled[9]

**Left-leaning Joe Weinberg accused Julian of exploiting Rarita, but Julian responded that these papers established Rarita's reputation.

an amusing incident which happened one day while he, Schwinger, and Oppenheimer were talking in Oppenheimer's office in LeConte Hall. Two other students, Chaim Richman and Bernard Peters, came in seeking a suggestion for a research problem from Oppenheimer. Schwinger listened with interest while Oppenheimer proposed calculating the cross section for the electron disintegration of the deuteron. That midnight, when Gerjuoy came to pick up Schwinger for the latter's breakfast before their all-night work session, he noted that Schwinger, while waiting for him in the lobby of the International House, where Julian was living, had filled the backs of several telegram blanks with calculations on this problem. Schwinger stuffed the sheets in his pocket and they went to work. Six months later, again Gerjuoy and Schwinger were in Oppenheimer's office when Richman and Peters returned beaming. They had solved the problem, and they covered the whole board with the elaborate solution. Oppenheimer looked at it, said it looked reasonable, and then asked, "Julie, didn't you tell me you worked this cross section out?" Schwinger pulled the yellowed, crumpled blanks from his pocket, stared at them a moment, and then pronounced the students' solution okay apart from a factor of two. Oppenheimer told them to find their error, and they shuffled out, dispirited. Indeed, Schwinger was right, they found they had made a mistake, and they published the paper,[26] but they were sufficiently crushed that both switched to experimental physics.

At the time, Schwinger and Gerjuoy were collaborating on a paper[27] following from Schwinger's tensor theory of nuclear forces. The work

> involved calculating about 200 fairly complicated spin sums, which sums Julian and I performed independently and then compared. To have the privilege of working with Julian meant I had to accommodate myself to his working habits, as follows. Except on days when Julian had to come into the university during conventional hours to confer with Oppenheimer, I would meet him at 11:45 pm in the lobby of his residence, the Berkeley International House. He would then drive us to some Berkeley all-night bistro where he had breakfast, after which we drove to LeConte Hall, the Berkeley physics building, where we worked until about 4:00 am, Julian's lunchtime. After lunch it was back to LeConte Hall until about 8:30 am, when Julian was ready to think about dinner and poor TA me would meet my 9:00 am recitation class. Since I had just gotten married, and still was young enough to badly

> need my sleep, these months of working with Julian were trying, to put it mildly.
>
> What made it even more trying is the fact that when Julian and I carefully worked out together the 20 or so spin sums where our independent calculations disagreed, Julian proved to be right every time! I accepted the fact that Julian was a much better theorist than I, but I felt I was at least pretty good, and was infuriated by his apparent constitutional inability to make a single error in 200 complicated spin sum calculations. This inability stood Schwinger well when he embarked on the calculations that earned him the Nobel Prize. ... [Al]though Julian certainly realized how extraordinarily talented he was, he never gloated about his error free calculations or in any other way put me down.[28]

The year of the NRC Fellowship was followed by a second year at Berkeley as Oppenheimer's assistant. They wrote an important paper together which would prove crucial nearly a decade later: Although Oppenheimer was happy to imagine new interactions, Schwinger showed that an anomaly in fluorine decay could be explained by the existence of *vacuum polarization,* that is, by the virtual creation of electron-positron pairs.[29] This gave Schwinger a head start over Feynman, who for years suspected that vacuum polarization did not exist.

6 The War and the Radiation Laboratory

After two years at Berkeley, Oppenheimer and Rabi arranged a real job for Schwinger: He became first an instructor, then an Assistant Professor at Purdue University, which had acquired a number of bright young physicists under the leadership of Karl Lark-Horowitz. But the war was impinging on everyone's lives, and Schwinger was soon recruited into the work on radar. The move to the MIT Radiation Laboratory took place in 1943. There Schwinger rapidly became the theoretical leader, even though he was seldom seen, going home in the morning just as others were arriving. He developed powerful variational methods for dealing with complicated microwave circuits, expressing results in terms of quantities the engineers could understand, such as impedance and admittance.

At first it seems strange that Schwinger, by 1943 the leading nuclear theorist, should not have gone to Los Alamos, where nearly all his colleagues eventually settled for the duration. There seem to be at least three reasons why Schwinger stayed at the Radiation Laboratory throughout the war.

- The reason he most often cited later in life was one of moral repugnance. When he realized the destructive power of what was being constructed at Los Alamos, he wanted no part of it. In contrast, the radiation lab was developing a primarily defensive technology, radar, which had already saved Britain.

- He believed that the problems to solve at the Radiation Laboratory were more interesting. Both laboratories were involved largely in engineering, yet although Maxwell's equations were certainly well known, the process of applying them to waveguides required the development of special techniques that would prove invaluable to Schwinger's later career.

- Another factor probably was Schwinger's fear of being overwhelmed. In Cambridge he could live his own life, working at night when no one was around the lab. This privacy would have been much more difficult to maintain in the microworld of Los Alamos. Similarly, the working conditions at the Rad Lab were much freer than those at Los Alamos. Schwinger never was comfortable in a team setting, as witness his later aversion to the atmosphere at the Institute for Advanced Study.

The methods and the discoveries he made at the Rad Lab concerning the reality of the electromagnetic mass would be invaluable for his work on quantum electrodynamics a few years later. As the war wound down, physicists started thinking about new accelerators, since the pre-war cyclotrons had been defeated by relativity, and Schwinger became a leader in this development: He proposed a microtron,[††] an accelerator based on acceleration through microwave cavities, developed the theory of stability of synchrotron orbits, and most importantly, worked out in detail the theory of synchrotron radiation,[‡‡] at a time when many thought that such radiation would be negligible because of destructive interference. Schwinger never properly wrote up the work he conducted in his one and one-half years at the Rad Lab, an omission that has now be rectified in part by publication of a volume based

[††]The microtron is usually attributed to Veksler.

[‡‡]This was first circulated as a preprint in 1945. The paper[30] published in 1949 was substantially different.

on his lectures then and later, and including both published and unpublished papers.[31]

Although he never really published his considerations on self-reaction, he viewed that understanding as the most important part of his work on synchrotron radiation:

> It was a useful thing for me for what was to come later in electrodynamics, because the technique I used for calculating the electron's classical radiation was one of self-reaction, and I did it relativistically, and it was a situation in which I had to take seriously the part of the self-reaction which was radiation, so why not take seriously the part of the self-reaction that is mass change? In other words, the ideas of mass renormalization and relativistically handling them were already present at this classical level.

Just after the Trinity atomic bomb test, Schwinger traveled to Los Alamos to speak about his work on waveguides, electromagnetic radiation, and his ideas about future accelerators. There he met Richard Feynman for the first time. Feynman recalled that at the time Schwinger[33]

> had already a great reputation because he had done so much work ... and I was very anxious to see what this man was like. I'd always thought he was much older than I was [they were the same age] because he had done so much more. At the time I hadn't done anything.

7 QED

In 1945 Harvard offered Schwinger an Associate Professorship,* which he promptly accepted, partly because in the meantime he had met his future wife Clarice Carrol. Counteroffers quickly appeared, from Columbia, Berkeley, and elsewhere, and Harvard shortly made Schwinger the youngest full professor on the faculty to that date. There Schwinger quickly established a pattern that was to persist for many years—he taught brilliant courses on classical electrodynamics, nuclear physics, and quantum mechanics, surrounded himself with a devoted coterie of graduate students and post-doctoral assistants, and conducted incisive research that set the tone for theoretical physics throughout the world.

*He beat out Hans Bethe for the job.

Work on classical diffraction theory, begun at the Radiation Lab, continued for several years largely due to the presence of Harold Levine, whom Schwinger had brought along as an assistant. Variational methods, perfected in the electrodynamic waveguide context, were rapidly applied to problems in nuclear physics. But these were old problems, and it was *quantum electrodynamics* that was to define Schwinger's career.

But it took new experimental data to catalyze this development. That data was presented at the famous Shelter Island meeting held in June 1947, a week before Schwinger's wedding. There he, Feynman, Victor Weisskopf, Hans Bethe, and the other participants learned the details of the new experiments of Lamb and Retherford[32] that confirmed the pre-war Pasternack effect, showing a splitting between the $2S_{1/2}$ and $2P_{1/2}$ states of hydrogen, that should be degenerate according to Dirac's theory. In fact, on the way to the conference, Weisskopf and Schwinger speculated that quantum electrodynamics could explain this effect, and outlined the idea to Bethe there, who worked out the details, nonrelativistically, on his famous train ride to Schenectady after the meeting.[34]

But the other experiment announced there was unexpected: This was the experiment by Rabi's group and others[35] of the hyperfine anomaly that would prove to mark the existence of an anomalous magnetic moment of the electron, expressing the coupling of the spin of the electron to an applied magnetic field, deviating from the value again predicted by Dirac. Schwinger immediately saw this as the crucial calculation to carry out first, because it was purely relativistic, and much cleaner to understand theoretically, not involving the complication of bound states. However, he was delayed three months in beginning the calculation because of an extended honeymoon through the West. He did return to it in October, and by December 1947 had obtained a result[36] completely consistent with experiment. He also saw how to compute the relativistic Lamb shift (although he did not have the details quite right), and found the error in the pre-war Dancoff calculation of the radiative correction to electron scattering by a Coulomb field.[37] In effect, he had solved all the fundamental problems that had plagued quantum electrodynamics in the 1930s: The infinities were entirely isolated in quantities which renormalized the mass and charge of the electron. Further progress, by himself and others, was thus a matter of technique. Concerning Schwinger's technique at the time, Schweber writes[38]

> The notes of Schwinger's calculation [of the Lamb shift] are extant

> [and] give proof of his awesome computational powers. ... These traces over photon polarizations and integrations over photon energies ... were carried out fearlessly and seemingly effortlessly. ... Often, involved steps were carried out mentally and the answer was written down. And, most important, the lengthy calculations are error free!

8 Covariant Quantum Electrodynamics

During the next two years Schwinger developed two new approaches to quantum electrodynamics. His original approach, which made use of successive canonical transformations, while sufficient for calculating the anomalous magnetic moment of the electron, was noncovariant, and as such, led to inconsistent results. In particular, the magnetic moment appeared also as part of the Lamb shift calculation, through the coupling with the electric field implied by relativistic covariance; but the noncovariant scheme gave the wrong coefficient. (If the coefficient were modified by hand to the correct value, what turned out to be the correct relativistic value for the Lamb shift emerged, but what that was was unknown in January 1948, when he announced his results at the American Physical Society meeting.)

Norman Ramsey added an amusing footnote to the story about LaMer, the chemist who flunked Julian.[39] In 1948 Schwinger had to repeat his brilliant lecture on quantum electrodynamics three times at the American Physical Society meeting at Columbia, in successively larger rooms.[†]

> It was a superb lecture. We were impressed. And as we walked back together—Rabi and I were sitting together during the lecture—Rabi invited me to the Columbia Faculty Club for lunch. We got in the elevator [in the Faculty Club] when who should happen to walk in the elevator with us but LaMer. And as soon as Rabi saw that, a mischievous gleam came into his eye and he began by saying that was the most sensational thing that's ever happened in the American Physical Society. The first time there's been

[†]K. K. Darrow, secretary of the Physical Society, who apparently had little appreciation of theory, always scheduled the theoretical sessions in the smallest room. Schwinger's second lecture was given in the largest lecture hall in Pupin Lab, and the third in the largest theatre on campus.

> this three repeats—it's a marvelous revolution that's been done—LaMer got more and more interested and finally said, 'Who did this marvelous thing?' And Rabi said, 'Oh, you know him, you gave him an F, Julian Schwinger.'

So first at the Pocono Conference in April 1948, then in the Michigan Summer School that year, and finally in a series of three monumental papers, "Quantum Electrodynamics I, II, and III,"[40] Julian set forth his covariant approach to QED. At about the same time Feynman was formulating his covariant path-integral approach; and although his presentation at Pocono was not well-received, Feynman and Schwinger compared notes and realized that they had climbed the same mountain by different routes. Feynman's systematic papers[41] were published only after Dyson had proved the equivalence of Schwinger's and Feynman's schemes.[42]

It is worth remarking that Schwinger's approach was conservative. He took field theory at face value, and followed the conventional path of Pauli, Heisenberg, and Dirac. His genius was to recognize that the well-known divergences of the theory, which had stymied all pre-war progress, could be consistently isolated in renormalization of charge and mass. This bore a superficial resemblance to the ideas of Kramers advocated as early as 1938,[43] but Kramers proceeded classically. He had insisted that first the classical theory had to be rendered finite and then quantized. That idea was a blind alley. Renormalization of quantum field theory is unquestionably the discovery of Schwinger.

Feynman was more interested in finding an alternative to field theory, eliminating entirely the photon field in favor of action at a distance. He was, by 1950, quite disappointed to realize that his approach was entirely equivalent to the conventional electrodynamics, in which electron and photon fields are treated on the same footing.

As early as January 1948, when Schwinger was expounding his noncovariant QED to overflow crowds at the American Physical Society meeting at Columbia University, he learned from Oppenheimer of the existence of the work of Tomonaga carried out in Tokyo during the terrible conditions of wartime. Tomonaga had independently invented the "Interaction Representation" which Schwinger had used in his unpublished 1934 paper, and had come up with a covariant version of the Schrödinger equation as had Schwinger, which upon its Western rediscovery was dubbed the Tomonaga-Schwinger equation.[44] Both Schwinger and Tomonaga independently wrote the same equation, a generalization of the Schrödinger equation to an arbitrary spacelike surface, using nearly the same notation. The formalism found by Tomonaga and his school was essentially identical to that developed by Schwinger five years later; yet they at the time calculated nothing, nor did they discover renormalization. That was certainly no reflection on the ability of the Japanese; Schwinger could not have carried the formalism to its logical conclusion without the impetus of the postwar experiments, which overcame prewar paralysis by showing that the quantum corrections "were neither infinite nor zero, but finite and small, and demanded understanding."[17]

However, at first Schwinger's covariant calculation of the Lamb shift contained another error, the same as Feynman's.[45]

> By this time I had forgotten the number I had gotten by just artificially changing the wrong spin-orbit coupling. Because I was now thoroughly involved with the covariant calculation and it was the covariant calculation that betrayed me, because something went wrong there as well. That was a human error of stupidity.

French and Weisskopf[46] had gotten the right answer,

> because they put in the correct value of the magnetic moment and used it all the way through. I, at an earlier stage, had done that, in effect, and also got the same answer.

But now he and Feynman "fell into the same trap. We were connecting a relativistic calculation of high energy effects with a nonrelativistic calculation of low energy effects, a la Bethe." Based on the result Schwinger had presented at the APS meeting in January 1948, Schwinger claimed priority for the Lamb shift calculation:

> I had the answer in December of 1947. If you look at those [other] papers you will find that on the critical issue of the spin-orbit coupling, they appeal to the magnetic moment. The deficiency in the calculation I did [in 1947] was [that it was] a non-covariant calculation. French and Weisskopf were certainly doing a non-covariant calculation. Willis Lamb[47] was doing a non-covariant calculation. They could not possibly have avoided these same problems.

The error Feynman and Schwinger made had to do with the infrared problem that occurred in the relativistic calculation, which was handled by giving the photon a fictitious mass.

> Nobody thought that if you give the photon a finite mass it will also affect the low energy problem. There are no longer the two transverse degrees of freedom of a massless photon, there's also a longitudinal degree of freedom. I suddenly realized this absolutely stupid error, that a photon of finite mass is a spin one particle, not a helicity one particle.

Feynman was more forthright and apologetic in acknowledging[45] his error which substantially delayed the publication of the French and Weisskopf paper.

9 Quantum Action Principle

Schwinger learned from his competitors, particularly Feynman and Dyson. Just as Feynman had borrowed the idea that henceforward would go by the name of Feynman parameters from Schwinger, Schwinger recognized that the systematic approach of Dyson and Feynman was superior in higher orders. So by 1949 he replaced the Tomonaga-Schwinger approach by a much more powerful engine, the quantum action principle. This was a logical outgrowth of the formulation of Dirac,[48] as was Feynman's path integrals; the latter was an integral approach, Schwinger's a differential. The formal solution of Schwinger's differential equations was Feynman's functional integral; yet while the latter was ill-defined, the former could be given a precise meaning, and for example, required the introduction of fermionic variables, which initially gave Feynman some difficulty.

It may be fair to say that while the path integral formulation to quantum field theory receives all the press, the most precise exegesis of field theory is provided by the functional differential equations of Schwinger resulting from his action principle. He began in the "Theory of Quantized Fields I"[49] by introducing a complete set of eigenvectors "specified by a spacelike surface . . . and the eigenvalues . . . of a complete set of commuting operators constructed from field quantities attached to that surface." The question is how to compute the transformation function from one spacelike surface to another. After remarking that this development, time-evolution, must be described by a unitary transformation, he *assumed* that any infinitesimal change in the transformation function must be given in terms of the infinitesimal change in a quantum action operator, or of a quantum Lagrange function. This is the quantum dynamical principle, a generalization of the principle of least action, or of Hamilton's principle in classical mechanics. If the parameters of the system are not altered, the only changes arise from those of the initial and final states, from which Schwinger deduced the *Principle of Stationary Action*, from which the field equations may be derived. A series of six papers followed with the same title, and the most important "Green's Functions of Quantized Fields" published in the Proceedings of the National Academy.[50]

Paul Martin presented an entertaining account of the prehistory of their work together.[51]

> During the late 1940s and early 1950s Harvard was the home of a school of physics with a special outlook and a distinctive set of rituals. Somewhat before noon three times each week, the master would arrive in his blue chariot and, in forceful and beautiful lectures, reveal profound truths to his Cantabridgian followers, Harvard and M.I.T. students and faculty.[‡] Cast in a language more powerful and general than any of his listeners had ever encountered, these ceremonial gatherings had some sacrificial overtones—interruptions were discouraged and since the sermons usually lasted past the lunch hour, fasting was often required. Following a mid-afternoon break, private audiences with the master were permitted and, in uncertain anticipation, students would gather in long lines to seek counsel.
>
> During this period the religion had its own golden rule—the action principle—and its own cryptic testament—'On the Green's Functions of Quantized Fields.'[50] Mastery of this paper conferred on followers a high priest status.[§] The testament was couched in terms that could not be questioned, in a language whose elements

[‡]In a later recollection,[52] Martin elaborated: "Speaking eloquently, without notes, and writing with both hands, he expressed what was already known in new, unified ways, incorporating original examples and results almost every day. Interrupting the flow with questions was like interrupting a theatrical performance. The lectures continued through Harvard's reading period and then the examination period. In one course we attended, he presented the last lecture—a novel calculation of the Lamb Shift—during Commencement Week. The audience continued coming and he continued lecturing."

[§]Schwinger evidently was aware of the mystique. In a later letter recommending Martin for a permanent appointment at Harvard he stated that Martin was "superior in intrinsic ability and performance. Quantum field theory is the new religion of physics, and Paul C. Martin is one of its high priests."[5] However, as the last paragraph of the present essay demonstrates, Schwinger throughout his life maintained a tension between an elitist and a democratic view of science.

> were the values of real physical observables and their correlations. The language was enlightening, but the lectures were exciting because they were more than metaphysical. Along with structural insights, succinct and implicit self-consistent methods for generating true statements were revealed.

Recently, a perceptive analysis of Schwinger's Green's functions papers has been given by Schweber[53]. There he concludes that

> Schwinger's formulation of relativistic QFTs [quantum field theories] in terms of Green's functions was a major advance in theoretical physics. It was a representation in terms of elements (the Green's functions) that were intimately related to real physical observables and their correlation. It gave deep structural insights into QFTs; in particular, it allowed the investigation of the structure of the Green's functions when their variables are analytically continued to complex values, thus establishing deep connections to statistical mechanics.

10 "Gauge Invariance and Vacuum Polarization"

The paper "On Gauge Invariance and Vacuum Polarization"[54], submitted by Schwinger to the *Physical Review* near the end of December 1950, is nearly universally acclaimed as his greatest publication. As his lectures have rightfully been compared to the works of Mozart, so this might be compared to a mighty construction of Beethoven, the 3rd Symphony, the *Eroica*, perhaps. It is most remarkable because it stands in splendid isolation. It was written over a year after the last of his series of papers on his second, covariant, formulation of quantum electrodynamics was completed: "Quantum Electrodynamics III. The Electromagnetic Properties of the Electron—Radiative Corrections to Scattering"[40] was submitted in May 1949. And barely two months later, in March 1951, Schwinger would submit the first of the series on his third reformulation of quantum field theory, that based on the quantum action principle, namely, "The Theory of Quantized Fields I."[49] But "Gauge Invariance and Vacuum Polarization" stands on its own, and has endued the rapid changes in tastes and developments in quantum field theory, while the papers in the other series are mostly of historical interest now. As Lowell Brown[55] pointed out, "Gauge Invariance and Vacuum Polarization" still has over one hundred citations per year, and is far and away Schwinger's most cited paper.[¶] Yet even such a masterpiece was not without its critics. Abraham Klein, who was finishing his thesis at the time under Schwinger's direction, and would go on to be one of Schwinger's second set of "assistants" (with Robert Karplus), as, first, an instructor, and then a Junior Fellow, recalled that Schwinger (and, independently, he and Karplus) ran afoul of a temporary editor at the *Physical Review.* That editor thought Schwinger's original paper repeated too many complicated expressions and that symbols should be introduced to represent expressions that appeared more than once. Schwinger complied, but had his assistants do the dirty work. Harold Levine, who was still sharing Schwinger's office, working on the never-to-be-completed waveguide book, typed the revised manuscript, while Klein wrote in the many equations. Klein recalled that he took much more care in writing those equations than he did in his own papers.[57]

Schwinger recalled later that he viewed this paper, in part, as a reaction to the "invariant regularization" of Pauli and Villars.[58]

> It was this paper, with its mathematical manipulation, without physical insight particularly about questions such as photon mass and so forth, which was the direct inspiration for 'Gauge Invariance and Vacuum Polarization.' The whole point is that if you have a propagation function, it has a certain singularity when the two points coincide. Suppose you pretend that there are several particles of the same type with different masses and with coupling constants which can suddenly become negative instead of positive. Then, of course, you can cancel them. It's cancellation again, subtraction physics, done in a more sophisticated way, but still, things must be made to add up to zero. Who needs it?

In this paper, Schwinger obtained a closed form for the electron propagator in an external magnetic field, by solving proper-time equations of motion, opening a field which would be fashionable nearly three decades later with the discovery of pulsars; gave the definitive derivation of the Euler-Heisenberg

[¶]In the 2005 *Science Citation Index*, it had 105 citations, out of a total of 458 citations to all of Schwinger's work.[56] These numbers have remained remarkably constant over the years.

Lagrangian describing the scattering of light by light, a phenomenon still not observed directly; and gave the precise connection between axial-vector and pseudoscalar meson theories, what became known as the axial-vector anomaly when it was rediscovered nearly two decades later by Adler, Bell, and Jackiw.[59] (We will discuss this the anomaly later in Sec. 17.) The paper is not only a thing of great beauty, but a powerful storehouse of practical technique for solving gauge-theory problems in a gauge-invariant way.

11 Harvard and Schwinger

So it was no surprise that in the late 1940s and early 1950s Harvard was the center of the world, as far as theoretical physics was concerned. Everyone, students and professors alike, flocked to Schwinger's lectures. Everything was revealed, long before publication; and not infrequently others received the credit because of Schwinger's reluctance to publish before the subject was ripe. A case in point is the so-called Bethe-Salpeter equation,[60] which as Gell-Mann and Low noted,[61] "first appeared in Schwinger's lectures at Harvard." At any one time, Schwinger had ten or twelve Ph.D. students, who typically saw him but rarely. In part, this was because he was available to see his large flock but one afternoon a week, but most saw him only when absolutely necessary, because they recognized that his time was too valuable to be wasted on trivial matters. A student may have seen him only a handful of times in his graduate career, but that was all the student required. When admitted to his sanctum, students were never rushed, were listened to with respect, treated with kindness, and given inspiration and practical advice. One must remember that the student's problems were typically quite unrelated to what Schwinger himself was working on at the time; yet in a few moments, he could come up with amazing insights that would keep the student going for weeks, if not months. A few students got to know Schwinger fairly well, and were invited to the Schwingers' house occasionally; but most saw Schwinger primarily as a virtuoso in the lecture hall, and now and then in his office. A few faculty members were a bit more intimate, but essentially Schwinger was a very private person.

12 Custodian of Field Theory

Feynman left the field of quantum electrodynamics in 1950, regarding it as essentially complete. Schwinger never did. During the next fifteen years, he continued to explore quantum field theory, trying to make it reveal the secrets of the weak and strong interactions. And he accomplished much. In studying the relativistic structure of the theory, he recognized that all the physically significant representations of the Lorentz group were those that could be derived from the "attached" four-dimensional Euclidean group, which is obtained by letting the time coordinate become imaginary.[62] This idea was originally ridiculed by Pauli, but it was to prove a most fruitful suggestion. Related to this was the CPT theorem, first given a proof for interacting systems by Schwinger in his "Quantized Field" papers of the early 1950s, and elaborated later in the decade.[63]

By the end of the 1950s, Schwinger, with his former student Paul Martin, was applying field theory methods of many-body systems, which led to a revolution in that field.[64] Methods suitable for describing systems out of equilibrium, usually associated with the name of Keldysh,[65] were obtained some four years earlier by Schwinger.[66] Along the way, in what he considered rather modest papers, he discovered Schwinger terms,[67] anomalies in the commutation relations between field operators, and the Schwinger model,[68] still the only known example of dynamical mass generation. The beginnings of a quantum field theory for non-Abelian fields was made;[69] the original example of a non-Abelian field being that of the gravitational field, he laid the groundwork for later canonical formulations of gravity.[70]

13 Measurement Algebra

In 1950 or so, as we mentioned, Schwinger developed his action principle, which applies to any quantum system, including nonrelativistic quantum mechanics. Two years later, he reformulated quantum kinematics, introducing symbols that abstracted the essential elements of realistic measurements. This was measurement algebra, which yielded conventional Dirac quantum mechanics. But although the result was as expected, Schwinger saw the approach as of great value pedagogically, and as providing a interpretation of quantum mechanics that was self-consistent. He taught quantum mechanics this way for many years, starting in 1952 at the Les Houches summer school;

but only in 1959 did he start writing a series of papers expounding the method to the world. He always intended to write a definitive textbook on the subject, but only an incomplete version based on the Les Houches lectures ever appeared during his lifetime.[71] Englert has now put his later undergraduate UCLA lectures together in a lovely book published by Springer.[72]

One cannot conclude a retrospective of Schwinger's work without mentioning two other magnificent achievements in the quantum mechanical domain. He presented a definitive development of angular momentum theory derived in terms of oscillator variables in "On Angular Momentum," which was never properly published;[73‖] and he developed a "time-cycle" method of calculating matrix elements without having to find all the wavefunctions in his beautiful "Brownian Motion of a Quantum Oscillator,"[66] which as we mentioned above anticipated the work of Keldysh.[65] We should also mention the famous Lippman-Schwinger paper,[75] which is chiefly remembered for what Schwinger considered a standard exposition of quantum scattering theory, not for the variational methods expounded there.

14 Electroweak Synthesis

In spite of his awesome ability to make formalism work for him, Schwinger was at heart a phenomenologist. He was active in the search for higher symmetry; while he came up with W_3, Gell-Mann found the correct approximate symmetry of hadronic states, $SU(3)$. Schwinger's greatest success in this period was contained in his masterpiece, his 1957 paper "A Theory of the Fundamental Interactions".[76] Along with many other insights, such as the existence of two neutrinos and the $V-A$ structure of weak interactions, Schwinger there laid the groundwork for the electroweak unification. He introduced two charged intermediate vector bosons as partners to the photon, which couple to charged weak currents.

A few years later, his former student, Sheldon Glashow, as an outgrowth of his thesis, would introduce a neutral heavy boson to close the system to the modern $SU(2)\times U(1)$ symmetry group;[77] Steven Weinberg[78] would complete the picture by generating the masses for the heavy bosons by spontaneous symmetry breaking. Schwinger did not have the details right in 1957, in particular because experiment then seemed to disfavor the $V-A$ theory his

‖This and other of Schwinger's most important papers were reprinted in two selections of his work.[18,74]

approach implied, but there is no doubt that Schwinger must be counted as the grandfather of the Standard Model on the basis on this paper.

15 The Nobel Prize and Reaction

Recognition of Schwinger's enormous contributions had come early. He received the Charles L. Meyer Nature of Light Award in 1949 on the basis of the partly completed manuscripts of his "Quantum Electrodynamics" papers. The first Einstein prize was awarded to him, along with Kurt Gödel, in 1951. The National Medal of Science was presented to him by President Johnson in 1964, and, of course, the Nobel Prize was received by him, Tomonaga, and Feynman from the King of Sweden in 1965.

But by that point his extraordinary command of the machinery of quantum field theory had convinced him that it was too elaborate to describe the real world, at least directly. In his Nobel Lecture, he appealed for a phenomenological field theory that would make immediate contact with the particles experiencing the strong interaction. Within a year, he developed such a theory, Source Theory.

16 Source Theory and UCLA

It surely was the difficulty of incorporating strong interactions into field theory that led to "Particles and Sources," received by the *Physical Review* barely six months after his Nobel lecture, in July 1966,[79] based on lectures Schwinger gave in Tokyo that summer. One must appreciate the milieu in which Schwinger worked in 1966. For more than a decade he and his students had been nearly the only exponents of field theory, as the community sought to understand weak and strong interactions, and the proliferation of "elementary particles," through dispersion relations, Regge poles, current algebra, and the like, most ambitiously through the S-matrix bootstrap hypothesis of Geoffrey Chew and Stanley Mandelstam.[80–83] What work in field theory did exist then was largely axiomatic, an attempt to turn the structure of the theory into a branch of mathematics, starting with Arthur Wightman,[84] and carried on by many others, including Arthur Jaffe at Harvard.[85] (The name changed from axiomatic field theory to constructive field theory along the way.) Schwinger looked on all of this with considerable distaste; not that he did not appreciate many of the contributions these techniques offered in specific contexts, but he could not see how they could form the *basis* of a theory.

The new source theory was supposed to supersede field theory, much as Schwinger's successive covariant formulations of quantum electrodynamics had replaced his earlier schemes. In fact, the revolution was to be more profound, because there were no divergences, and no renormalization.

> The concept of renormalization is simply foreign to this phenomenological theory. In source theory, we begin by hypothesis with the description of the actual particles, while renormalization is a field theory concept in which you begin with the more fundamental operators, which are then modified by dynamics. I emphasize that there never can be divergences in a phenomenological theory. What one means by that is that one is recognizing that all further phenomena are consequences of one phenomenological constant, namely the basic charge unit, which describes the probability of emitting a photon relative to the emission of an electron. When one says that there are no divergences one means that it is not necessary to introduce any new phenomenological constant. All further processes as computed in terms of this primitive interaction automatically emerge to be finite, and in agreement with those which historically had evolved much earlier.[86]

16.1 Engineering Approach to Particle Theory

In 1969 Schwinger gave the Stanley H. Klosk lecture to the New York University School of Engineering Science. Because that lecture captures his philosophy underpinning source theory so well, at an early stage in the development of that approach, I quote the transcription of it in full.[87]

> It is a familiar situation in physics that when an experimental domain is to be codified, even though a fundamental theory may be available, rarely is it brought directly to bear upon the experimental material. The fundamental theory is too complicated, generally too remote from the phenomena that you want to describe. Instead, there is always an intermediate theory, a phenomenological theory, which is designed to deal directly with

the phenomena, and therefore makes use of the language of observation. On the other hand, it is a genuine theory, and employs abstract concepts that can make contact with the fundamental theory.

The true role of fundamental theory is not to confront the raw data, but to explain the relatively few parameters of the phenomenological theory in terms of which the great mass of raw data has been organized.

I learned this lesson 25 years ago during World War II, when I became interested in the problems of microwave systems, wave guides in particular. Being very naive, I started out solving Maxwell's equations. I soon learned better. Most of the information in Maxwell's equations is really superfluous. As far as any particular problem is concerned, one is only interested in the propagation of just a few modes of the wave guide. A limited number of quantities that can be measured or calculated tell you how these few modes behave and exactly what the system is doing.

You are led directly to a phenomenological theory of the kind engineers invariably use—a picture, say, in terms of equivalent transmission lines. The only role of Maxwell's equations is to calculate the few parameters, the effective lumped constants that characterize the equivalent circuits.

The engineer's intermediate phenomenological theory looks in both directions. It can be connected to the fundamental theory at one end, and at the other it is applied directly to the experimental data. This is an example of the engineering attitude. It is a pragmatic approach that is designed specifically for use. It is a nonspeculative procedure. Hypotheses that go beyond what is relevant to the available data are avoided.

Now, when we come to realm of high-energy physics, we are in a new situation. We do not know the underlying dynamics, the underlying fundamental theory. That raises the question of finding the best strategy. That is, what is the most effective way of confronting the data, of organizing it, of learning lessons from results within a limited domain of experimental material?

I want to argue that we should adopt a pragmatic engineering approach. What we should *not* do is try to begin with some fundamental theory and calculate. As we saw, this is not the best thing to do even when you have a fundamental theory, and if you don't have one, it's certainly the wrong thing to do.

Historically, relativistic quantum mechanics had proved very successful in explaining atomic and nuclear physics until we got accelerators sufficiently high in energy to create the strongly interacting particles, which include particles that are highly unstable and decay through very strong forces. The ordinary methods that had evolved up to this point were simply powerless in the face of this new situation. At the higher energies, particles can be—and are—created and destroyed with high probability.

In other words, the immutability of the particle—a foundation of ordinary physics—had disappeared.

If the immutable particle has ceased to exist as the fundamental concept in terms of which a situation can be described, what do we replace it with? There have been two different points of view about how to construct a fundamental theory for the strong interactions.

The first—the point of view of conventional operator field theory—proposes to replace the particle with three-dimensional space itself. In other words, you think of energy, momentum, electric charge, and other properties as distributed throughout space, and of small volumes of three-dimensional space as the things that replace particles. These volumes are the carriers of energy, momentum, and so on.

People, including myself, have been actively developing the field idea for many years. I believe that this kind of theory may be the ultimate answer, but please recognize that it is a speculation. It assumes that one is indeed able to describe physical phenomena down to arbitrarily small distance, and, of course, that goes far beyond anything we know at the moment. All we are able to do experimentally as we go to higher and higher energies is to plumb to smaller and smaller distances, but never to zero distance.

The question is, should you, in discussing the phenomena that are presently known, make use of a speculative hypothesis like operator field theory? Can we not discuss particle phenomenology and handle the correlations and organization of data without becoming involved in a speculative theory? In operator field theory you cannot separate particle phenomena from speculations about the structure of particles. The operators of quantum-mechanical field theory conceptually mix these together.

To be able to discuss anything from the operator-field-theory point of view, you must accept its fundamental hypothesis. You have to accept a speculation about how particles are constructed before you can begin to discuss how particles interact with each other.

Historically, this has proved to be a very difficult program to apply, and people have, of course, been anxious to deal directly with the experimental data, and so there has been a reaction. The extreme reaction to operator field theory is to insist that there is nothing more fundamental than particles and that, when you have a number of particles colliding with each other and the number of particles ceases to be constant, all you can do is correlate what comes into a collision with what goes out, and cease to describe in detail what is happening during the course of the collision.

This point of view is called S-matrix theory. The quantitative description is in terms of a scattering matrix that connects the outgoing state with the incoming state. In this theory the particles are basic and cannot be analyzed. Then, of course, the question comes up: what distinguishes the particular set of particles that do exist from any other conceivable set?

The only answer that has been suggested is that the observed particles exist as a consequence of self-consistency. Given a certain set of particles, other particles can be formed as aggregates or composites of these. On the other hand, if particles are unanalyzable, then this should not be a new set of particles, but the very particles themselves.

That is the second idea, but I beg you to appreciate that it is also a speculation. We do not know for a fact that our present inability to describe things in terms of something more fundamental than particles reflects an intrinsic impossibility.

So these are the two polarized extremes in the search for a fundamental theory—the operator-field-theory point of view and the S-matrix point of view. Now my reaction to all of this is to ask again why we must speculate, since the probability of falling on the right speculation is very small.

Can we not separate the theoretical problem of describing the properties of these particles from speculations about their inner structure? Can we not set aside the speculation of whether particles are made from operator fields or are made from nothing but themselves, and find an intermediate theory, a phenomenological theory that directly confronts the data, but that is still a creative theory?

This theory should be sufficiently flexible so that it can make contact with a future, more fundamental theory of the structure of particles, if indeed any more fundamental theory ever appears. This is the line of reasoning that led me to consider the theoretical problem for high-energy physics from an engineering point of view. Clearly I have some ideas in mind about how to carry out such a program, and I would like to give you an enormously simplified account of them.

We want to eliminate speculation and take a pragmatic approach. We are not going to say that particles are made out of fields, or that particles sustain each other. We are simply going to say that particles are what the experimentalists say they are. But we will construct a theory and not an experimenter's manual in that we will look at realistic experimental procedures and pick out their essence through idealizations.

There is one characteristic that the high-energy particles have in common—they must be created. Through the act of creation, we can define what we mean by a particle. How, in fact, do you create a particle? By a collision. The experimentalist arranges for a beam of particles to fall on a target. In the center-of-mass system, the target is just another beam, so two beams of particles are colliding. Out of the collision, the particle that we are interested in may be produced.

We say that it is a particle rather than a random bump on an excitation curve because its properties are reproducible. We still recognize the same particle event though we vary a number of experimental parameters, such as energy, angles, and the kind of reaction. The properties of the particle in question remain the same—it has the same mass, the same spin, the same charge.

These criteria can be applied to an object that may last for only 10^{-24} sec—which decays even before it gets out of the nucleus in which it was created. Nevertheless, it is still useful to call this kind of object a particle because it possesses essentially all of the characteristics that we associate with the particle concept.

What is significant is that within a somewhat controllable region of space and time, the properties characteristic of the particle have become transformed into the particle itself. The other particles in the collision are there only to supply the net balance of properties. They are idealized as the *source* of the particle.

This is our new theoretical construct. We introduce a quantitative description of the particle source in terms of a source function, $S(x)$, where x refers to the space and time coordinates. This function indicates that, to some extent, we can control the region the particle comes from.

But we do not have to claim that we can make the source arbitrarily small as in operator field theory. We leave this question open, to be tested by future experiment.

A particular source may be more effective in making particles that go in one direction rather than another, so there must be another degree of control expressed by a source function of momentum, $S(p)$. But from quantum mechanics we know that the dimension of the system and the degree of directionality are closely related. The smaller the system, the less directional it can be. And relativistic mechanics is incorporated from the very beginning in that the energy and the momentum are related to its mass in the usual relativistic way.

Now the experimenter's job only begins with the production of a beam. At the other end, he must detect the particles. What is detection? Unstable particles eventually decay, and the decay process is a detection device. More generally, any detection device can be regarded as a kind of collision that annihilates the particle and hands its properties on in a more usable form. Thus the source concept can again be introduced as an abstraction of an annihilation collision, with the source acting negatively, as a sink.

We now have a complete theoretical picture of any physical situation, in which sources are used to create the initial particle of interest from the vacuum state, and sources are used to detect the final particles resulting from some interaction, thus returning to the vacuum state.

[Schwinger then wrote down an expression that describes the probability amplitude that the vacuum state before sources act remains the vacuum state after sources act, the vacuum persistence amplitude.] The basic things that appear in this expression are the source functions and space-time functions that represent the state into which the particle is emitted and from which it is absorbed, thus describing the intermediate propagation of the particle.

This simple expression can be generalized to apply to particles that have charge, spin, etc., and to situations where more than one particle is present at a time. Interactions between particles are described in terms containing more than two sources.

Our starting point accepts particles as fundamental—we use sources to identify the particles and to incorporate a simplified view of dynamics. From that we evolve a more complete dynamical theory in which we combine simple source arrangements like building blocks to produce descriptions of situations that can in principle be as complex as we want.

A first test of this approach would be to see if we can reproduce the results of some well-established theory such as quantum-electrodynamics. What is the starting point in this attack on electrodynamics? It is the photon, a particle that we know has certain striking properties such as zero rest mass and helicity 1. So we must include all these aspects of the photon in the picture, and describe how photons are emitted and absorbed. In consequence, the source must be a vector, and it must be divergenceless.

This approach leads us to something resembling a vector potential, and when we ask what differential equations it satisfies we find they are Maxwell's equations. We start with the concept of the source as primary and are led to Maxwell's differential equations as derived concepts.

The description of interactions follows the tentative procedures of life in the real world. The theory is not stated once and for all. It begins with simple phenomena—for example, accelerated charges radiate. It then extrapolates that information outside its domain, predicts more complicated phenomena, and awaits the test of experiment. We do not begin with a final description of, say, electron scattering. We extrapolate to it from more elementary situations, and this is still not the final description.

As the theory develops and becomes more encompassing, we go back to refine the description of the scattering process and obtain a more quantitative account of it. This is the concept of an interaction skeleton. The process is there but it is not finally described to start with, its existence is merely recognized. This simplified reconstruction of electrodynamics is completely successful.

To indicate the wide sweep of the new approach, I mention that classical gravitation theory (Einstein) can be reconstructed and simplified in a similar way by beginning with the quantum relativistic properties of the basic particle, the graviton, although here indirect evidence for its properties must be adduced.

But the real proving ground for source theory comes from the domain for which it was invented, strong interactions. The starting point is experimental information at low energies. The tentative extrapolations are toward higher energies. The method is quite elementary compared to other current techniques. The successful correlations that have been obtained emphasize the completely phenomenological nature of our present knowledge about particles and refute attempts to lend fundamental credence to this or that particle model.

A more fundamental theory may come into being one day, but it will be the outcome of continued experimental probing to higher energies, and will doubtless involve theoretical concepts that are now only dimly seen. But that day will be greatly speeded if the flood of experimental results is organized and analyzed with the aid of a theory that does not have built into it a preconception about the very question that is being attacked. This theory is source theory.

16.2 The Impact of Source Theory

Robert Finkelstein has offered a perceptive discussion of Schwinger's source theory program:

> In comparing operator field theory with source theory Julian revealed his political orientation when he described operator field theory as a trickle down theory (after a failed economic theory)—since it descends from implicit assumptions about unknown phenomena at inaccessible and very high energies to make predictions at lower energies. Source theory on the other hand he described as anabatic (as in Xenophon's Anabasis) by which he meant that it began with solid knowledge about known phenomena at accessible energies to make predictions about physical phenomena at higher energies. Although source theory was new, it did not represent a complete break with the past but rather was a natural evolution of Julian's work with operator Green's functions. His trilogy on source theory is not only a stunning display of Julian's power as an analyst but it is also totally in the spirit of the modest scientific goals he had set in his QED work and which had guided him earlier as a nuclear phenomenologist.[88]

But the new approach was not well received. In part this was because the times were changing; within a few years, 't Hooft[89] would establish the renormalizability of the Glashow-Weinberg-Salam $SU(2)\times U(1)$ electroweak model, and field theory was seen by all to be viable again. With the discovery of asymptotic freedom in 1974,[90] a non-Abelian gauge theory of strong interactions, quantum chromodynamics, which was proposed somewhat earlier,[91] was promptly accepted by nearly everyone. An alternative to conventional field theory did not seem to be required after all. Schwinger's insistence on a clean break with the past, and his rejection of "rules" as opposed to learning through serving as an "apprentice," did not encourage conversions.

Already before the source theory revolution, Schwinger felt a growing sense of unease with his colleagues at Harvard. But the chief reason Schwinger left Harvard for UCLA was health related. Formerly overweight and inactive, he had become health conscious upon the premature death of Wolfgang Pauli in 1958. (Ironically, both died of pancreatic cancer.) He had been fond of tennis from his youth, had discovered skiing in 1960, and now his doctor was recommending a daily swim for his health. So he listened favorably to the entreaties of David Saxon, his closest colleague at the Radiation Lab during the war, who for years had been trying to induce him to come to UCLA. Very much against his wife's wishes, he made the move in 1971. He brought along his three senior students at the time, Lester DeRaad, Jr., Wu-yang

Tsai, and the present author, who became long-term "assistants" at UCLA. He and Saxon expected, as in the early days at Harvard, that students would flock to UCLA to work with him; but they did not. Schwinger was no longer the center of theoretical physics.

This is not to say that his little group at UCLA did not make an heroic attempt to establish a source-theory presence. Schwinger remained a gifted innovator and an awesome calculator. He wrote 2-1/2 volumes of an exhaustive treatise on source theory, *Particles, Sources, and Fields*,[92] devoted primarily to the reconstruction of quantum electrodynamics in the new language; unfortunately, he abandoned the project when it came time to deal with strong interactions, in part because he became too busy writing papers on an "anti-parton" interpretation of the results of deep-inelastic scattering experiments.[93] He made some significant contributions to the theory of magnetic charge; particularly noteworthy was his introduction of dyons.[94] He reinvigorated proper-time methods of calculating processes in strong-field electrodynamics;[95] and he made some major contributions to the theory of the Casimir effect, which are still having repercussions.[96] But it was clear he was reacting, not leading, as witnessed by his quite pretty paper on the "Multispinor Basis of Fermi-Bose Transformation,"[97] in which he kicked himself for not discovering supersymmetry, following a command private performance by Stanley Deser on supergravity.

17 The Axial-Vector Anomaly and Schwinger's Departure from Particle Physics

In 1980 Schwinger gave a seminar at MIT that marked his last scientific visit to the East Coast,[**] and caused him to abandon his attempt to influence the development of high-energy theory with his source theory revolution. The talk was on a subject that he largely started in his famous "Gauge Invariance and Vacuum Polarization" paper,[54] the triangle or axial-vector anomaly. In its simplest and basic manifestation, this "anomaly" describes how the neutral pion decays into two photons. The pion coupling could be regarded as occurring either through a pseudoscalar or an axial vector

[**]This does not count a talk he gave at MIT in 1991 in honor of birthdays of two of his students, where he gave a "progress report" on his work on cold fusion and sonoluminescence, excerpts of which is given in Ref. [2].

coupling, which formally appeared be equivalent, but calculations in the 1940s gave discrepant answers. Schwinger resolved this issue in 1950 by showing that the two theories were indeed equivalent provided that proper care (gauge-invariance) was used, and that the formal result was modified by an additional term. Problem solved, and it was then forgotten for the next 18 years. In the late 1960s Adler, Bell, and Jackiw rediscovered this solution,[59] but the language was a bit different. The extra term Schwinger had found was now called an anomaly, but the form of the equations, and the prediction for the decay of the pion, were identical. In fact, at first it is apparent that Adler, Bell, and Jackiw were unaware of Schwinger's much earlier result, and it was the addition of Ken Johnson (one of Schwinger's many brilliant students) into the collaboration that corrected the historical record.[98]

Shortly thereafter, Adler and Bardeen proved the "nonrenormalization" theorem,[99] that the anomaly is exact, and is not corrected by higher-order quantum effects. This is in contrast to most physical phenomena, such as the anomalous magnetic moment of the electron, which is subject to corrections in all orders of perturbation theory in the strength of the electromagnetic coupling, the fine structure constant. This seemed surprising to Schwinger, so he suggested to his postdocs at UCLA that they work this out independently, and they did, publishing two papers in 1972,[100] in which they showed, using two independent methods, that there was indeed such a correction in higher order. However, Adler, who was the reviewer of these papers forced them to tone down their conclusion, and to point out that the result depends on the physical point at which the renormalization is carried out. Nonrenormalization indeed can be achieved by renormalization at an unphysical point, which may be acceptable for the use of the theorem in establishing renormalizability of gauge theories, its chief application, but it is nevertheless true that physical processes such as the original process of pion decay receives higher-order corrections.

As was typical, Schwinger apparently took no notice of this dispute at the time. But toward the end of the 1970s, while he was writing the third volume of *Particles, Sources, and Fields*, he looked at the questions of radiative corrections to neutral pion decay and found the same result as DeRaad, Milton, and Tsai. He wrote an explicitly confrontational paper on the subject, which was the basis for the above-mentioned talk at MIT. The paper was apparently definitively rejected, and the talk was harshly criticized, and on the basis of these closed-minded attacks, Schwinger left the field. Fortunately for the record, Schwinger's paper exists as a chapter in the finally published third volume of *Particles, Sources, and Fields.*

However, the controversy lives on. In 2004 Steve Adler wrote a historical perspective on his work on the axial-vector anomaly.[101] He devotes five pages of his retrospective to attack the work of Schwinger and his group. He even denies that Schwinger was the first to calculate the anomaly, in blatant disregard of the historical record. Of course, physical understanding had increased in the nearly two decades between Schwinger's and Adler's papers, but to deny that Schwinger was the first person to offer the basis for the connection between the axial-vector and pseudoscalar currents, and the origin of the photonic decay of the neutral pion, is preposterous.

18 Thomas-Fermi Atom, Cold Fusion, and Sonoluminescence

When the last of his Harvard postdocs left UCLA in 1979, and the flap over the axial-vector anomaly ensued, Schwinger abandoned high-energy physics altogether. In 1980, after teaching a quantum mechanics course (a not-unusual sequence of events), Schwinger began a series of papers on the Thomas-Fermi model of atoms.[102] He soon hired Berthold-Georg Englert, replacing Milton as a postdoc, to help with the elaborate calculations. This endeavor lasted until 1985. It is interesting that this work not only is regarded as important in its own right by atomic physicists, but has led to some significant results in mathematics. A long series of substantial papers by C. Fefferman and L. Seco[103] has been devoted to proving his conjecture about the atomic number dependence of the ground state energy of large atoms. As Seth Putterman has remarked, it is likely that, of all the work that Schwinger accomplished at UCLA, his work on the statistical atom will prove the most important.[104]

Following the Thomas-Fermi work, Schwinger continued to collaborate with Englert, and with Marlan Scully, on the question of spin coherence. If an atomic beam is separated into sub-beams by a Stern-Gerlach apparatus, is it possible to reunite the beams? Scully had argued that it might be possible, but Julian was skeptical; the result was three joint papers, entitled "Is Spin Coherence Like Humpty Dumpty?", which bore out Julian's intuition of the impossibility of beating the effects of quantum entanglement.[105]

In March 1989 began one of the most curious episodes in physical science in the last century, one that initially attracted great interest among the scientific as well as the lay community, but which rapidly appeared to be a characteristic example of "pathological science."[††] The effect to which we refer was the announcement by B. S. Pons and M. Fleischmann[107] of the discovery of cold fusion. That is, they claimed that nuclear energy, in the form of heat, was released in a table-top experiment, involving a palladium cathode electrolyzing heavy water.

So it was a shock to most physicists[‡‡] when Schwinger began speaking and writing about cold fusion, suggesting that the experiments of Pons and Fleischmann were valid, and that the palladium lattice played a crucial role. In one of his later lectures on the subject in Salt Lake City, Schwinger recalled, "Apart from a brief period of apostasy, when I echoed the conventional wisdom that atomic and nuclear energy scales are much too disparate, I have retained my belief in the importance of the lattice."[5] His first publication on the subject was submitted to *Physical Review Letters*, but was roundly rejected, in a manner that Schwinger considered deeply insulting. In protest, he resigned as a member (he was, of course, a fellow) of the American Physical Society, of which *Physical Review Letters* is the most prestigious journal. (At first he intended merely to withdraw the paper from *PRL*, and his fellowship, but then he felt compelled to respond to the referees' comments: One comment was something to the effect that no nuclear physicist could believe such an effect, to which Julian angrily retorted, "I am a nuclear physicist!"[5]) In this letter to the editor (G. Wells) in which he withdrew the paper and resigned from the American Physical Society, he also called for the removal of the source theory index category the APS journals used: "Incidentally,

[††]This term was coined in 1953 by Irving Langmuir, who gave a celebrated lecture at General Electric's Knolls Atomic Power Laboratory (transcribed from a disc recording by Robert Hall) on the phenomenon wherein reputable scientists are led to believe that an effect, just at the edge of visibility, is real, even though, as precision increases, the effect remains marginal. The scientist becomes self-deluded, going to great lengths to convince one and all that the remarkable effect is there just on the margins of what can be measured. Great accuracy is claimed nevertheless, and fantastic, *ad hoc*, theories are invented to explain the effect. Examples include N-rays, the Allison effect, flying saucers, and ESP. It was not a coincidence that *Physics Today* published the article, without comment, in the fall of 1989.[106]

[‡‡]However, a few other eminent physicists spoke favorably of the possibility of cold fusion, notably Edward Teller and Willis Lamb, who published three articles in the *Proceedings of the U.S. National Academy of Sciences* on the subject.

the PACS entry (1987) 11.10.mn can be deleted. There will be no further occasion to use it.';[5,108] A rather striking act of hubris: If he couldn't publish source theory, neither could anybody else. But the *Physical Review* obliged. (Unfortunately, Schwinger failed to realize that the PACS index system has become the predominant system for physics journals worldwide, a reflection of the premier status of the APS journals. So he largely spited his own contributions.) Not wishing to use any other APS venue, he turned to his friend and colleague, Berthold Englert, who arranged that "Cold Fusion: A Hypothesis" be published in the *Zeitschrift für Naturforschung*, where it appeared in October of that year.[109] Schwinger then went on to write three substantial papers, entitled "Nuclear Energy in an Atomic Lattice I, II, III," to flesh out these ideas.[5,110] The first was published in the *Zeitschrift für Physik D*,[111] where it was accepted in spite of negative reviews,[5] but directly preceded by an editorial note, disclaiming any responsibility for the the paper on the part of the journal. They subsequently refused to publish the remaining papers.

Schwinger's last physics endeavor marked a return to the Casimir effect, of which he had been enamored nearly two decades earlier. It was sparked by the remarkable discovery of single-bubble sonoluminescence, in which a small bubble of air in water, driven by a strong acoustic standing wave, undergoes a stable cycle of collapse and re-expansion; at minimum radius an intense flash of light, consisting of a million optical photons, is emitted. It was not coincidental that the leading laboratory investigating this phenomenon was, and is, at UCLA, led by erstwhile theorist Seth Putterman, long a friend and confidant. Putterman and Schwinger shared many interests in common, including appreciation of fine wines, and they shared a similar iconoclastic view of the decline of physics. So, of course, Schwinger heard about this remarkable phenomenon from the horse's mouth, and was greatly intrigued.[*]

Schwinger immediately had the idea that a dynamical version of the Casimir effect might play a key role. He saw parallels between cold fusion and sonoluminescence in that both deal with seemingly incommensurate energy scales, and both depend significantly on nonlinear effects. Since by the early 1990s, cold fusion was largely discredited, he put his efforts to understanding sonoluminescence, which undoubtedly does exist. Unfortunately neither Schwinger, nor anyone subsequently, was able to get very far with dynam-

[*]For a review of the phenomena, and a detailed evaluation of various theoretical explanations, see Ref. [112].

ical zero-point phenomena; he largely contented himself with an adiabatic approximation based on static Casimir energies; and was able to obtain sufficient energy only because he retained the "bulk energy," which most now believe is unobservable, being subsumed in a renormalization of bulk material properties. His work on the subject appeared as a series of short papers in the PNAS, the last appearing[113] shortly after his death in June 1994.

19 Conclusion

It is impossible to do justice in a few words to the impact of Julian Schwinger on physical thought in the 20th Century. He revolutionized fields from nuclear physics to many body theory, first successfully formulated renormalized quantum electrodynamics, developed the most powerful functional formulation of quantum field theory, and proposed new ways of looking at quantum mechanics, angular momentum theory, and quantum fluctuations. His legacy includes "theoretical tools" such as the proper-time method, the quantum action principle, and effective action techniques. Not only is he responsible for formulations bearing his name: the Rarita-Schwinger equation, the Lippmann-Schwinger equation, the Tomonaga-Schwinger equation, the Dyson-Schwinger equation, the Schwinger mechanism, and so forth, but some attributed to others, or known anonymously: Feynman parameters, the Bethe-Salpeter equation, coherent states, Euclidean field theory; the list goes on and on. His legacy of nearly 80 Ph.D. students, including four Nobel laureates, lives on. It is impossible to imagine what physics would be like in the 21st century without the contributions of Julian Schwinger, a very private yet wonderful human being. It is most gratifying that a dozen years after his death, recognition of his manifold influences is growing, and research projects he initiated are still underway.

It is fitting to close this retrospective with Schwinger's own words, delivered some six months before his final illness, when he received an honorary degree from the University of Nottingham.[114†]

> The Degree Ceremony is a modern version of a medieval rite that seemed to confer a kind of priesthood upon its recipients, thereby excluding all others from its inner circle. But that will

†This brief acceptance speech was followed by a brilliant lecture on the influence of George Green on Schwinger's work.[114]

not do for today. Science, with its offshoot of Technology, has an overwhelming impact upon all of us. The recent events at Wimbledon invite me to a somewhat outrageous analogy. Very few of us, indeed, are qualified to step onto centre court. Yet thousands of spectators gain great pleasure from watching these talented specialists perform. Something similar should be, but generally is not, true for the relationship between the practitioners of Science and the general public. This is much more serious than not knowing the difference between 30 all and deuce. Science, on a big scale, is inevitably intertwined with politics. And politicians have little practice in distinguishing between, say common law and Newton's law. It is a suitably educated public that must step into the breach. This has been underlined lately by Minister Waldegrave's cry for someone to educate him about the properties of the Higgs boson, to be rewarded with a bottle of champagne. Any member of the educated public could have told him that the cited particle is an artifact of a particular theoretical speculation, and the real challenge is to enter uncharted waters to see what is there. The failure to do this will inevitably put an end to Science. A society that turn in on itself has sown the seeds of its own demise. Early in the 16th century, powerful China had sea-going vessels exploring to the west. Then a signal came from new masters to return and destroy the ships. It was in those years that Portuguese sailors entered the Indian Ocean. The outcome was 400 years of dominance of the East by the West.

There are other threats to Science. A recent bestseller in England, *Understanding the present*, has the subtitle *Science and the soul of Modern Man*. I shall only touch on the writer's views toward quantum mechanics, surely the greatest intellectual discovery of the 20th century. First, he complains that the new physics of quantum mechanics tosses classical physics in the trash bin. This I would dismiss as mere technical ignorance; the manner in which classical and quantum mechanics blend into each other has long been established. Second, the author is upset that its theories can't be understood by anyone not mathematically sophisticated and so must be accepted by most people on faith. He is, in short, saying that there is a priesthood. Against this I pose my own experience in presenting the basic concepts of quantum

> mechanics to a class of American high school students. They understood it; they loved it. And I used no more than a bit of algebra, a bit of geometry. So: catch them young; educate them properly; and there are no mysteries, no priests. It all comes down to a properly educated public.

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