Julian Schwinger
(1918-1994)

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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 accomplishment, 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.

Given his commanding stature in theoretical physics for decades 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 but few. 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 most people 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 I. 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 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.

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. 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 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 Brief Life of Schwinger

A full biography of Julian Schwinger has been published [1], as well as a selection of his most important papers [2]. Here we will sketch a brief outline of Schwinger's life and work, referring the interested reader to the biography for more details. An excellent 100-page account of Schwinger's career through 1950 may also be found in Schweber's history of quantum electrodynamics [3].

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 seven 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 well-known lawyer, and his mother always considered him as the successful son, 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. It 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. 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. He 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 grades were not good, and he would have flunked out if the College had not also had a rather forgiving policy toward grades.

Not only was Julian already reading the literature at City College, but he quickly started to do original research. Thus before he left the City College, Schwinger wrote a paper entitled 'On the Interaction of Several Electrons,' in which he introduced a procedure that he would later call the interaction representation to describe the scattering of spin-1/2 Dirac particles, electron-electron scattering or Møller scattering. This paper he wrote entirely on his own, but showed it to no one, nor did he submit it to a journal. It was 'a little practice in writing,' but it was a sign of great things to come.

It was Lloyd Motz, one the instructors at City College, who had heard about Julian from Harold, and with whom Julian was writing papers, who introduced him to Rabi. Then, in a conversation between Rabi and Motz over the Einstein, Rosen, Podolsky paper [4], 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 with a scholarship, and by 1937 he had 7 papers published, which constituted his Ph.D. thesis, even though his bachelor's degree had not yet been granted. The papers which Julian wrote at Columbia were on both theoretical and experimental physics, and Rabi prized Julian's ability to obtain the numbers 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, laid the foundation for the theory of nuclear magnetic resonance), and spent a somewhat lonely, but productive winter of 1937 in Wisconsin, where he provided the groundwork for his prediction that the deuteron had an electric quadrupole moment, independently confirmed by his experimental colleagues at Columbia a year later [5], both announced at the Chicago meeting of the American Physical Society in November 1938. Wisconsin confirmed his predilection for working at night, so as not to be 'overwhelmed' by his hosts, Eugene Wigner and Gregory Breit.

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, which was to be influential decades later with the birth of supergravity.

The year of the NRC Fellowship was followed by a second year at Berkeley as Oppenheimer's assistant. They had already written an important paper together which would prove crucial several years 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. This gave Schwinger a head start over Feynman, who for years suspected that vacuum polarization did not exist.

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. These methods, and the discoveries he made there 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, a 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.[1]

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.' [7]

At first it may seem strange that Schwinger, by 1943 the leading nuclear

[1] Material based on his Radiation Lab work has now been published [6].

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.

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. 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.

Quantum Electrodynamics

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 [8] 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, non-relativistically, on his famous train ride to Schenectady after the meeting [9]. But the other experiment announced there was unexpected: This was the experiment by Rabi's group and others [10] of the hyperfine anomaly that would prove to mark the existence of an anomalous magnetic moment of the electron,

$$\boldsymbol{\mu} = g\frac{e}{2m}\mathbf{S}, \tag{1}$$

differing from the value $g = 2$ 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 the result $g/2 = 1 + \alpha/2\pi$, 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 [11]. In effect, he had solved all the fundamental problems that had plagued quantum electrodynamics in the 1930s: The infinities were entirely isolated in quantities that renormalized the mass and charge of the electron. Further progress, by himself and others, was thus a matter of technique.

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 to isolate the infinities, 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 right number, what turned out to be the correct relativistic value for the Lamb shift emerged, but what that was was unknown in January 1948, when Schwinger announced his results at the APS meeting in New York.) 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,' Schwinger set forth his covariant approach to QED. At about the same time Feynman was formulating his covariant path-integral approach; and although Feynman's 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 [12] were published only after Dyson [13] had proved the equivalence of Schwinger's and Feynman's schemes.

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 [14]. 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 [15], 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 [16]. 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 by Oppenheimer the Tomonaga-Schwinger equation. Both Schwinger and Tomonaga independently wrote the same equation, a generalization of the Schrödinger equation to an arbitrary spacelike surface σ, using nearly the same notation:

$$i\hbar c\frac{\delta\Psi[\sigma]}{\delta\sigma(x)} = \mathcal{H}(x)\Psi[\sigma], \tag{2}$$

where $\mathcal{H}$ is the interaction Hamiltonian,

$$\mathcal{H}(x) = -\frac{1}{c}j_\mu(x)A_\mu(x), \tag{3}$$

j_μ being the electric current density of the electrons, and A_μ the electromagnetic vector potential. 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 [18]. '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.' [7] French and Weisskopf [19] 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.' [7] 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 [20] 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 [12] was more forthright and apologetic in acknowledging his error which substantially delayed the publication of the French and Weisskopf paper, in part because he, unlike Schwinger, had published his incorrect result [18].

Quantum Action Principle

Schwinger learned from his competitors, particularly Feynman and Dyson. Just as Feynman had borrowed the idea from Schwinger that henceforward would go by the name of Feynman parameters, Schwinger recognized that the systematic approach of Dyson-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 [21], 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, at the beginning of the new millennium, that while the path integral formulation of 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' by introducing a complete set of eigenvectors 'specified by a spacelike surface σ and the eigenvalues ζ' 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, that is, $(\zeta_1', \sigma_1|\zeta_2'', \sigma_2)$. 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, W_{12}, or of a quantum Lagrange function $\mathcal{L}$. This is the quantum dynamical principle:

$$\begin{aligned}\delta(\zeta_1', \sigma_1|\zeta_2'', \sigma_2) &= \frac{i}{\hbar}(\zeta_1', \sigma_1|\delta W_{12}|\zeta_2'', \sigma_2)\\ &= \frac{i}{\hbar}(\zeta_1', \sigma_1|\delta \int_{\sigma_2}^{\sigma_1}(dx)\mathcal{L}(x)|\zeta_2'', \sigma_2).\end{aligned} \tag{4}$$

Here, $\mathcal{L}$ is a relativistically invariant Hermitian function of the fields and their derivatives,

$$\mathcal{L}(x) = \mathcal{L}(\phi^a(x), \partial_\mu \phi^a(x)), \tag{5}$$

where a labels the different field operators of the system. If the parameters of the system are not altered, the only changes arise from those of the initial and final states, which changes are effected by infinitesimal generating operators $F(\sigma_1)$, $F(\sigma_2)$, expressed in terms of operators associated with the surfaces σ_1 and σ_2. In this way, Schwinger deduced the *Principle of Stationary Action*,

$$\delta W_{12} = F(\sigma_1) - F(\sigma_2), \tag{6}$$

from which the field equations may be deduced. 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 of Sciences.

The paper 'On Gauge Invariance and Vacuum Polarization,' 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' 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.' 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. Among many other remarkable developments, Schwinger discovered here the axial-vector anomaly, nearly twenty years before its rediscovery and naming by Adler, Bell, and Jackiw [22]. As Lowell Brown [23] 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.[2]

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 [24], which as Gell-Mann and Low noted [25], 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.

[2] In 2005 the *Science Citation Index* lists 104 citations, out of a total of 458 citations to all of Schwinger's work. These numbers have remained remarkably constant over ten years.

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. 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. By the end of the 1950s, Schwinger, with his former student Paul Martin, was applying field theory methods to many-body systems, which led to a revolution in that field, and independently developed techniques which opened up non-equilibrium statistical mechanics. Along the way, in what he considered rather modest papers, he discovered Schwinger terms, anomalies in the commutation relations between field operators, and the Schwinger model, still the only known example of dynamical mass generation. The beginning of a quantum field theory for non-Abelian fields was made; the original example of a non-Abelian field being that of the gravitational field, he laid the groundwork for later canonical formulations of gravity. (See also [26].) Fundamental here were his consistency conditions for a relativistic quantum field theory.

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. (In the last few years, Englert brought his UCLA quantum mechanics lectures to a wider audience [27].)

One cannot conclude a retrospective of Schwinger's work without mentioning two other magnificent achievements in the quantum mechanical domain. He presented in 1952 a definitive development of angular momentum theory derived in terms of oscillator variables in 'On Angular Momentum,' which was never properly published; 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' (1961). We should also mention the famous Lippmann-Schwinger paper (1950), which is chiefly remembered for what Schwinger considered a standard exposition of quantum scattering theory, not for the variational methods expounded there.

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.' 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. That coupling is exactly that found in the standard model. 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 [28]; Steven Weinberg [29] 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 seemed to disfavor the $V-A$ theory his 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.

The Nobel Prize and Reaction

Recognition of Schwinger's enormous contributions had come early. He received the Charles L. Mayer 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. The following year, Schwinger, Tomonaga, and Feynman received the Nobel Prize in Physics from the King of Sweden.

But by this 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 describe directly the particles experiencing the strong interaction. Within a year, he developed such a theory, Source Theory.

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, 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 [30, 31, 32, 33]. 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 [34], and carried on by many others, including Arthur Jaffe at Harvard [35]. (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.' [36]

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.' [37]

But the new approach was not well received. In part this was because the times were changing; within a few years, 't Hooft [38] 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 [39], a non-Abelian gauge theory of strong interactions, quantum chromodynamics, which was proposed somewhat earlier [40], 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 while 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. 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*, 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. He made some significant contributions to the theory of magnetic charge; particularly noteworthy was his introduction of dyons. He reinvigorated proper-time methods of calculating processes in strong-field electrodynamics; and he made some major contributions to the theory of the Casimir effect, which are still having repercussions. But it was clear he was reacting, not leading, as witnessed by his quite pretty paper on the 'Multispinor Basis of Fermi-Bose Transformation' (1979), in which he kicked himself for not discovering supersymmetry.

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 Schwinger-Dyson equations, 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. 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.

Julian Schwinger lectured twice at the Erice International School on Subnuclear Physics, in the years 1986 and 1988.

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