I recall my interactions with Julian Schwinger, first as a graduate student at Harvard, and then as a postdoc at UCLA, in the period 1968–81, and subsequently. Some aspects of his legacy to physics are discussed.

Keywords: Quantum field theory, source theory, quantum mechanics, magnetic charge, Casimir effect

1. The Birth of Source Theory

When I came to Harvard in 1967, Julian Schwinger had already heeded the advice he gave in his Nobel Lecture to find an alternative to quantum field theory. He was increasingly concerned that conventional field theory, which he had so very largely developed, was becoming physically remote. Renormalization was supposed to connect the fundamental fields with the physical particles observed in the laboratory. His attempts to include gravity and dual electrodynamics in the framework which so gloriously accommodated quantum electrodynamics sparked his frustration, as did the general feeling then that quantum field theory could not describe strong interactions. Surely, Schwinger had mused, there had to be a more direct way to confront the phenomena of nature.

So within a year, he did come up with a more phenomenological approach, which he dubbed source theory. Source theory papers started appearing in 1966, first applied to electrodynamics and then to chiral symmetry. The emphasis was on effective Lagrangians, and the avoidance of infinities. In some sense, source theory blended dispersion relations and field theory. That is, typical calculations of processes involving virtual particles involved constructing a situation where real particles were exchanged between effective sources, and the resulting amplitude was “space-time extrapolated” to the general situation, with the amplitude being written as a spectral form. At least in simple cases, the process was straightforward and quite effective.

I learned the theory first from the detailed notes of Wu-yang Tsai, taken the first year I was at Harvard, when I foolishly did not sit in on Julian’s lectures. By the end of that year, I had approached the great man and asked if I could work with him. To my surprise, he was quite encouraging, so I worked hard the next three years to justify his faith in me.
2. What Was It Like to Work with Julian?

Typically, at Harvard, he had twelve students, at various stages of development. He was available only on Wednesdays, after lunch, first come, first served (advance booking required). I recall staying up most of the night on Tuesdays working feverishly, then getting up at the ungodly hour of 8:30 to post my name near the top of the list kept by his secretary, who would arrive at 9:00. If you were near enough to the top, and Julian returned from lunch early enough, you would receive admittance. Once you entered Julian’s office in the afternoon, there was no time pressure, although you didn’t want to appear to be too stupid. If the phone rang, it was invariably ignored. Time with the master was unmetered! All his students had distinct problems, none of which coincided with what Julian was working on himself, but after a few minutes of explanation, he would come up with a valuable suggestion. It might not work, but it would take a week or two to follow the ideas through, and thirty minutes or so of consultation every week or two was more than sufficient to keep progress on track.

Other than these weekly or biweekly meetings, we saw Julian in his classes. All his students, and some faculty, sat in on his courses on quantum mechanics and field theory. (Questions were not encouraged!) The lectures were like musical performances, and he held the audience in rapt attention. Every time he taught a course, the content was completely new, so it was advantageous to attend every reincarnation. Much original research appeared in his lectures, and sometime never anywhere else! (An example was the first appearance of the Bethe-Salpeter equation.)

My oral exam, in 1969, on a derivation of the Lamb shift via “sidewise dispersion relations,” devolved into a dispute between Paul Martin and Schwinger on the fundamentals of source theory, but I emerged unscathed, being able to speak for no more than five minutes. I was rewarded with a copy of his recent Brandeis lectures. Even easier was my defense of my thesis, two years later, largely on trace anomalies it turned out. It consisted of an excellent lunch at a French restaurant in Westwood. (I successfully answered Julian’s single question, on my birthplace.)

3. The Schwinger’s Move to California

As I was working on stress tensors (which I still am), Clarice and Julian took a sabbatical to Japan, a wonderful experience for them. There he completed his first volume of *Particles, Sources, and Fields*. When they returned in Fall 1970, Julian announced his plan to move to UCLA, which was met with great consternation by his gang of students. However, he invited his three senior students, me, Lester DeRaad, Jr., and Wu-yang Tsai, to accompany him as his assistants (postdocs). The move was accomplished in February 1971. (They were greeted by the San Fernando earthquake!)
3.1. Why did Julian leave Harvard?

Of course, he was unhappy with the reception of source theory at Harvard, by his own former students, Paul Martin and Shelly Glashow, and others—but this was not the chief reason for relocating. Julian had become obsessed with exercise, after the death in 1958 of Wolfgang Pauli to pancreatic cancer, so he took up skiing and tennis, and California offered more opportunities. He could, and did, even have a pool. For years, his assistant at the MIT Radiation Laboratory during the war, David Saxon, who had become chair of the physics department at UCLA (eventually he would become president of the whole University of California system), had been urging Julian to move to UCLA. He finally accepted, much to the regret of Boston-bred Clarice. Saxon and Schwinger both assumed many students would flock to him, as they had when he joined Harvard in 1946. But this was not to be.

3.2. The Sourcerer’s Apprentices

I arrived at UCLA soon after the Schwingers did, while Lester and Wu-yang arrived in the summer of 1971. We formed a close research group at UCLA for several years. Wu-yang stayed till 1976, when he left for Coral Gables, while Lester left in 1978, although he stayed in Southern California; I left the following year for Ohio State University in Columbus, when my wife was offered a job there in the Dance Department.

As grad students, we never had any social contact with the great man. That changed with our change in status: We had lunch with him (usually including Bob Finkelstein and visitors) once a week, and occasionally were invited to the Schwinger’s home in Bel Air, with its magnificent view of LA. Julian was always a gracious host. He was not status conscious, and had a remarkable ability to listen to others. An example was the occasion when Julian brought my wife’s nephew, a sullen teenager, into the conversation by an insightful remark about Thelonious Monk.

4. Interactions with Feynman

Although Schwinger and Richard Feynman were both living in the Los Angeles area for more than 20 years, they rarely socialized. A story I learned from Berge Englert is that once the Feynmans were invited to the Schwinger’s home, and were having a good time, until another couple arrived, which spoiled the evening for the Feynmans. When they did meet at conferences they were always very cordial, and had great mutual respect, which dated back to the encounters at Shelter Island and the Poconos, during their parallel development of quantum electrodynamics.

5. Particles, Sources, and Fields

“If you can’t join ’em, beat ’em” was the motto to his three-volume exposition of source theory which contains a wealth of information about fields with arbitrary
spin, and included many detailed calculations in QED. The second volume even includes a very complete calculation of the fourth order electron anomalous magnetic moment, which was first correctly calculated by his student Charlie Sommerfield. As mentioned, the first volume was written during his sabbatical in Japan, while the second and third (unfinished) volumes were completed at UCLA. He abandoned this book project just as he was about to embark on strong interactions, being diverted by his attempt to understand deep inelastic scattering. Although we three apprentices diligently proofread the later volumes, Julian forgot to acknowledge our help in the published books!

6. Collaborations with his “Assistants”

With the dramatic discovery in 1974 of what is now called the \( J/\psi \) particle, Julian was quick to come up with an explanation, involving a “hidden sector,” maybe “dyons.” The latter was based on his Science article, “Magnetic Model of Matter,” written in 1969. We joined in with papers on the decay of \( \psi(3.7) \) to \( \psi(3.1) \). His work on “renormalization group without renormalization or a group” led to parallel papers with me, eventually finding applications in QCD. Julian’s last papers before the “source theory revolution” were on electric and magnetic charge renormalization. He revisited the subject periodically thereafter, with variations on the \( eg = n\hbar c/2 \) quantization condition. (He bemoaned the lack of experimental evidence for magnetic monopoles, “If only the Price had been right.”) This led to the joint “Dyon-dyon scattering” paper, with rainbows and glories. The entirely separate but joint interest of Luis Alvarez and Julian Schwinger in the quest for magnetic charge would eventually lead to the OU search for magnetic monopoles, which set the best limit on their masses for a decade, until LHC data became available.

6.1. Magnetic charge

Julian’s last major effort in “particle physics” was his effort to describe the dramatic scaling phenomena discovered in deep inelastic scattering without reference to Feynman’s partons or Gell-Mann’s quarks of which he had disparaged the naming. His approach involved double spectral forms, related closely to the Deser-Gilbert-Sudarshan representation. We followed on with some explicit related calculations, but eventually showed that although the double spectral representation was generally valid, it required anomalous spectral regions, which invalidated some of the expected behaviors.
6.3. QED in background fields

Julian also revisited the electrodynamics of particles in strong magnetic fields, harking back to the famous “Gauge Invariance and Vacuum Polarization” paper, his most cited paper, and what I regard as the first source theory paper, even though it was written in 1951. This work recalled his days at the Rad Lab during World War II, where he worked out the theory of synchrotron radiation, independently of the Russians. The sequel to Schwinger’s 1949 paper appeared in 1973, and collaborative papers with Tsai and Erber appeared subsequently. Independent papers based on Schwinger’s powerful formalism continued to emerge.

6.4. The triangle anomaly

Sometimes the interactions were bottom-up. What is called the axial-vector anomaly was discovered by Julian in his famous 1951 paper. We postdocs showed in 1972 that there were radiative corrections to neutral pion decay into two photons, in apparent (but not actual?) disagreement with the Adler-Bardeen theorem. Julian later confirmed our result, a correction by a factor of $1 + \alpha/\pi$, but instead of seeking accommodation with received wisdom, wrote a joint confrontational paper. Before the paper was submitted, Julian gave a talk on the subject at MIT, which was badly received. The remains of that unpublished paper exist in the 3rd volume of *Particles, Sources, and Fields*.

6.5. Supersymmetry

With the emergence of supersymmetry, and especially its local version, supergravity, Julian regretted he had not thought of it first, since he had expounded the multispinor basis of particles with integer and half-integer spin in *Particles, Sources, and Fields*. A command performance by Stan Deser in 1977 led to his own reconstruction of the theory, but with negligible impact (according to the current Inspire HEP database, this paper received only 12 citations). The same fate befell the follow-up paper by Bob Finkelstein, Luis Urrutia, and me in which we reconstructed supergravity following Schwinger’s lead.

6.6. Casimir effect

Invariably, Julian put his current research into his lectures, which of course we always attended. He became intrigued with how the Casimir effect could be understood without the zero point energy which seemed not to be present in the source theory approach, inspired, I believe, by conversations with Seth Puttman. This lecture got quickly written up and then, with Lester and myself we rederived the Lifshitz theory which included the infamous “Schwinger prescription,” concerning how to treat the thermal corrections, an amazingly ongoing controversy. We went on to rederive the surprising result, first found by Tim Boyer, that the
Casimir self-stress on a perfectly conducting spherical shell is repulsive.\textsuperscript{55} I’ve been strongly bound to the Casimir effect ever since.\textsuperscript{56}

7. Books
We have already mentioned his monumental three-volume \textit{Particles, Sources, and Fields}. But his continual lectures led to several other volumes.

7.1. \textit{Classical Electrodynamics}
For the first time since the War, Julian taught graduate electrodynamics at UCLA, and we assistants sat in. We started turning the lecture notes into a book, and ended up with a contract with W.H. Freeman. Julian then began to pay attention, decided it didn’t sound enough like himself, and began a nonconvergent series of revisions. He worked on it for the better part of a decade, abandoning the rewriting when he got to radiation theory. After his death, we turned it into the book which exists today,\textsuperscript{57} with the publisher cycling from Addison-Wesley to Perseus and now Taylor&Francis.

7.2. \textit{Understanding Space and Time}
Julian and astronomer George Abell developed a BBC course with the Open University (1976–79). The bulk of the course dealt with cosmology; Julian’s part was to explain relativity to a general audience. A “Robie the Robot” graced the Schwinger’s living room thereafter. It aired at a “good time” on BBC2, but rather obscurely on KCET in Los Angeles, because its release coincided with that of Carl Sagan’s \textit{Cosmos}. Unlike Sagan, Julian was not a natural performer. “Not God’s gift to presenting” was how he was described by his producer Ian Rosenbloom. However, a Scientific American Library volume \textit{Einstein’s Legacy}\textsuperscript{58} did memorialize this effort, which is full of interesting historical anecdotes and insights.

7.3. \textit{Quantum Mechanics}
For the first time Schwinger began to teach quantum mechanics to undergraduates at UCLA. (Of course, at Harvard, half his audience for his graduate quantum mechanics courses consisted of bright Harvard undergrads.) He taught many subjects in very original ways: the framework was the “measurement algebra” that he had developed in the early 1950s based on an analysis of Stern-Gerlach experiments on polarized atoms. Some notes based on these ideas were published rather obscurely (for physicists) at the end of that decade in the Proceedings of the National Academy of Sciences;\textsuperscript{59–63} most famous were the Les Houches lectures of 1955, which appeared in print only in 1970 (together with reprints of some of the PNAS papers).\textsuperscript{64} But the definitive record of his lectures only was published under the editorship of Berge Englert.\textsuperscript{65}
More impact, of course, followed from Schwinger’s Quantum Action Principle, which originated at the same time. This was immediately applied to his reformulation (his third) of quantum electrodynamics. We cannot trace that profound redevelopment here, but note that a rather accessible volume describing his action principle based largely on his lectures over the years has now appeared.

7.4. Electromagnetic Radiation

It will be recalled that much of the impetus for Schwinger’s rapid scaling of the peak of quantum electrodynamics derived from his wartime work on radar theory at the Radiation Laboratory. (Ironically, it was thought then that “radiation” was a word that would not suggest classified work was in progress). Of course, toward the end of the war, he gave brilliant lectures on his researches to the other lab workers, and many of these were transcribed by David Saxon, and mimeographed. There was supposed to be a published volume of these lectures, but that never materialized.

Many years later, after the successful publication of Classical Electrodynamics, Alex Chao of SLAC and Chris Caron of Springer persuaded me to turn the archival materials into a book. I did so to the best of my ability, and supplemented it with some bits of the Schwinger archive at UCLA which had not found their way into the previous textbook. The resulting volume contains also reprints of a number of papers of Schwinger on waveguides, synchrotrons and synchrotron radiation, and diffraction.

8. Family and Diversions

8.1. My marriage to Margarita Baños

Alfredo Baños, Jr., was a colleague of Julian’s at the Rad Lab during the War. Alfredo moved to UCLA thereafter, eventually divorcing his first wife and marrying Alice (a great scandal at the time), and they had a child Margarita. She grew up and became a dancer. Naturally, Clarice thought to introduce Margarita, who in 1975 had just returned from 6 years with the Royal Ballet School in London, to one of Julian’s apprentices. Although I was second choice (Lester DeRaad was first, but, unbeknownst to Clarice, he was already taken), it did work out, and this year we celebrated our 40th anniversary with a most memorable trip to Bali after the Singapore Centennial.

8.2. V. Sattui Winery

The Schwingers became significant investors in this winery when it was relaunched in 1976. In those days, the winery and the BBC efforts were a frequent topic of our lunch conversations, which ranged widely. On Julian’s death, in 1994, the winery introduced a special Cabernet Sauvignon to commemorate their famous partner.
8.3. 60th Birthday Celebration

I helped organize, along with Bob Finkelstein and Margy Kivelson, his Fest in 1978. Julian was rather unhappy about the affair, for he thought of it as a retirement celebration. But he later apologized publicly to me and Margy when he received the Monie Fert award at Georgia Tech in 1986. Dick Feynman gave a wonderful talk at the banquet for Schwinger’s Fest. He recounted his encounters with Julian at Los Alamos and Pocono. “Although we’d come from the ends of the earth with different ideas, we’d climbed the same mountain from different sides.” His remarks were not included in the 60th birthday volume but were in the one assembled for Julian’s 70th, the Fest then being a somewhat more low-keyed affair.

9. The Later Years

9.1. Thomas-Fermi and Humpty-Dumpty

I left UCLA, as noted above, in 1979, when I went to Ohio State as a Visiting Associate Professor. But I didn’t resign my semi-permanent position at UCLA until 1981, when I accepted a regular faculty position at another OSU, this time in Oklahoma. Berge Englert became my replacement as Julian’s assistant in November of that year. He immediately joined Schwinger in renewed explorations of the Thomas-Fermi model of atoms, which Julian had started to analyze in his undergraduate quantum mechanics course. Lester helped at first, but then Berge became the chief calculator, and an impressive series of papers followed. Englert left UCLA for a position in Munich in 1985, but their collaboration continued. Marlan Scully, who was visiting the University of Munich in 1987 involved them in a series of papers questioning whether one can reunite beams of atoms which have been separated by a Stern-Gerlach measurement, with an unsurprising negative answer; Humpty-Dumpty cannot be put back together again.

9.2. Cold fusion and sonoluminescence

In 1989 began one of the most remarkable examples of “pathological science” with the announcement by Pons and Fleischman, noted chemists, of the discovery of cold fusion. Of course, fusion occurs at some very low rate at ambient temperature due to quantum tunneling, but they claimed to see significant energy released. The history of this sad affair is given in the book by Huizenga. It bears on our story because Schwinger, nearly alone among physicists, took the report seriously and believed he could explain it. The rejection of his paper by Physical Review Letters led him to resign his fellowship in the American Physical Society and to demand that the “source theory” index category be deleted, as he would no longer use it! (The journal complied, even though the PACS scheme was beginning to be widely used by other journals, and the source theory category was of course used by others.) Englert then helped him get the paper published in Zeitschrift für Naturforschung, in spite of negative reviews, the second paper was published in
Zeitschrift für Physik D but accompanied by an editorial note disavowing any responsibility for the validity of the conclusions by the journal! Third and fourth papers were rejected and never published, although Schwinger wrote three short notes to the PNAS\textsuperscript{89-92} Englert reports that eventually Schwinger began to doubt whether his theory was entirely correct, and doubts about the experimental evidence rapidly accumulated. But until his death, he thought there was something right about the phenomena and his explanation of it.

So Julian turned his attention to another seemingly impossible phenomenon, that of sonoluminescence. Again he learned about this from his good friend Seth Putterman. In “single-bubble sonoluminescence,” a tiny bubble of air injected into water and submitted to suitable ultrasonic acoustic vibrations undergoes rapid collapse and expansion, which can persist for months. Near the point of minimum radius (a factor of 10 smaller than the maximum) a flash of visible light is emitted, carrying a total energy of about 10 MeV. For a review, see Ref. \textsuperscript{93}. The dynamics of the bubble seem rather well understood, but the mechanism for the light emission remains poorly understood to this day.

Julian immediately thought: “dynamical Casimir effect”: the rapidly changing boundary conditions might convert virtual photons into the real ones seen in the observations. So he proceeded to write a series of papers, the first two being follow-ups on his first Casimir effect paper in 1975\textsuperscript{94,95}. The balance of the work appears in a series of short notes in PNAS, his favorite journal where he could avoid scrutiny by cynical referees.\textsuperscript{96-102} Unfortunately, he had forgotten, or perhaps never realized, that before I had left UCLA I had considered the Casimir effect for a dielectric sphere;\textsuperscript{103} such was the basis of his estimates, which he never made very precise. In fact, at our last encounter, at the annual Christmas party at the Baños’ home in Westwood, to which Clarice and Julian invariably came (Julian’s job being to hide the three wise men in the Christmas tree), Julian suggested we work together to put the theory of sonoluminescence on a firm footing. But that was not to be. Two months later, he was diagnosed, like Pauli, with pancreatic cancer, and he died in July 1994. I did go back and work out the theory; unfortunately, the conclusion was that the energy balance was too small by a factor of a million to explain the copious production of photons seen in sonoluminescence.\textsuperscript{104-106}

10. Schwinger’s Legacy

Julian Schwinger spent nearly as many years at UCLA as at Harvard; the former from 1946-70, the latter 1971-94. The contrast seems dramatic: he published 150 papers in his Harvard years, many of which were groundbreaking and founded new fields. (Besides the obvious field theory developments, for example, think of the Keldysh-Schwinger formalism\textsuperscript{107}) In contrast, the 80 published during the UCLA period seemed more reactive, for instance, suggesting alternatives to the Weinberg-Salam-Glashow theory\textsuperscript{108} or an alternative approach to the renormalization group\textsuperscript{15,16} or to supersymmetry\textsuperscript{52} or even wrong-headed as in the cold-fusion
papers. One could blame much of this on his heroic attempt to reformulate field theory free from infinities, his source theory. Had he not been so confrontational in his demand that students totally divorce themselves from the conventional approach, but recognized that he was developing an effective action approach which offers numerous computational advantages, reception to his ideas would have been much more favorable and he would have attracted more students. (He did invent the concept of effective action, after all.)

One could argue that the fact he had only some 5 students at UCLA as compared to (depending on how one counts) nearly 80 at Harvard led to his increased isolation. But maybe it was not Schwinger who changed, but the world around him. In 1965 conventional field theory, which Julian had largely developed, seemed to have reached a dead end, and current algebra and dispersion relations seemed the way forward to understand hadronic physics. Julian tried to find a third way, but just as he was getting launched, the electroweak theory (for which he had laid most of the groundwork) was shown to be consistent and experimentally verified. Source theory might be efficient, but it was not necessary, and so was ignored, since only by contact with the master could you become initiated. It is unfortunate that Julian gave up his reconstruction of field theory just at the point where he was turning to strong interactions, which was what had impelled him to start this development. He could have contributed, like Feynman, to the elucidation of the non-Abelian theory of quantum chromodynamics, which is still more of a framework than a precisely calculable theory like QED. Instead he turned to his “dispersive” approach to deep inelastic scattering, which led to limited insight.

His most cited papers from the UCLA years, with over 200 citations each according to INSPIRE, are those on the Casimir effect. His work on this subject lives on, even though we practitioners embrace the notion that the effect reflects the change of zero-point energy or field fluctuations which Julian rejected. This work has technological applications, and undoubtedly has something to say about the accelerating universe we live in.

For much more information about the life and work of Julian Schwinger, please see the biography updated in Ref. This presentation grew out of Ref.

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