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Julian Schwinger: Source Theory and the UCLA Years— From Magnetic Charge to the Casimir Effect*

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ABSTRACT: Julian Schwinger began the construction of Source Theory in 1966 in response to the then apparent failure of quantum field theory to describe strong interactions, the physical remoteness of renormalization, and the utility of effective actions in describing chiral dynamics. This development did not meet with wide acceptance, and in part for this reason Julian left Harvard for UCLA in 1971. This nonacceptance was quite understandable, given the revolution in gauge theories that was then unfolding, a revolution, of course, for which he had laid much of the groundwork. Acceptance of his ideas was further impeded by his rejection of the quark model of hadrons and of QCD. I will argue, however, that the source theory development was not really so abrupt a break with the past as Julian may have implied, for the ideas and techniques in large measure were present in his work at least as early as 1951. Those techniques and ideas are still of fundamental importance to theoretical physics, so much so that the designation "source theory" has become superfluous. Julian did a great deal of innovative physics during the last 30 years of his life, and I will touch on some of the major themes, including magnetic charge, chiral dynamics, radiation theory, Thomas-Fermi models, theory of measurement, and the Casimir effect, as well as various forays into phenomenology. The impact of much of this work is not yet apparent.

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Julian Schwinger, who died rather suddenly last July, was arguably the most significant theoretical physicist since Dirac. It is with great sadness that I will attempt to summarize the second half of his career, from 1966 until his death in 1994, a time during which he published nearly 100 papers on a great range of topics. His passing was especially difficult for me because at the time I, ignorant of his illness, was on my way to work with him on sonoluminescence; instead, when I arrived at UCLA I spoke at the private memorial his wife Clarice held at their home, and updated his complete publication list, a task I had commenced seventeen years previously in connection with his 60th birthday festivities.[1]

Birth of Source Theory

I begin the tale with "magnetic charge" because his last "operator" field theory papers [129,130,133,134], published in 1966, and the first "new" papers after source theory was established [147,150], published in 1968–69, were devoted to that subject. But probably the appropriate starting point is, in fact, his Nobel lecture, delivered on December 11, 1965 [132]. He ends the lecture with a discussion of phenomenological relativistic quantum field theory, and states that "One has still to appreciate the precise rules of phenomenological relativistic field theory, . . . , given that the strong fundamental interactions have operated to compose the various physical particles." Is this not a prefigurement of his attempt to create a source-theory revolution six months later?

It surely was the difficulty of incorporating strong interactions into field theory that led to "Particles and Sources," received by the Physical Review in July 1966 [135], a recording of lectures Julian gave in Tokyo that summer. Particle phenomenology is primary, and I personally note with relish that he cites my Oklahoma colleague George Kalbfleisch in the second sentence of the introduction for the discovery of the η' meson. This paper already included particles of all spins through the use of multispinors. The following year there

[1] M. Flato, C. Fronsdal, and K. A. Milton, *Selected Papers (1937–1976) of Julian Schwinger* (Reidel, Dordrecht, 1979). The numbers in this article enclosed in square brackets refer to the list of Julian's papers I had compiled in that reference, and updated in August of 1994. The updated list is attached.

was an explosion of partial (chiral) symmetry papers [137–41,143–45]. I believe that it was, in fact, his attempt to put current algebra in effective Lagrangian language, together with Weinberg, which was the immediate impetus to the source-theory development. These papers were quite important at the time.

What is Source Theory?

Although Julian had invented the notion of a source at least as early as 1951, it was only in 1966 that he realized that he could base the whole machinery of particle physics on the abstraction of particle-creation and annihilation acts. One can define a free action, say for a photon, in terms of propagation of virtual photons between photon sources, conserved in order to remove the scalar degree of freedom. But a virtual photon can in turn act as a pair of electron-positron sources, through a "primitive interaction" between electrons and photons, essentially embodied in the conserved Dirac current. So this multiparticle exchange gives rise to quantum corrections to the photon propagator, to vacuum polarization, and so on. All this without any reference to renormalization or "high-energy speculations."

In its "purest" or at least original form, such source theory ideas were used to generate perturbative amplitudes in "causal" form; that is, in which real particles were exchanged between virtual sources separated in time. From this one could deduce immediately ("space-time extrapolation") the full amplitude in spectral form, that is, in what most people would refer to as a "dispersion relation." Such a direct generation of amplitudes was extremely powerful, and often allowed a completely finite calculation to be carried out. An impressive example is our calculation of the 4th-order Compton-scattering helicity amplitudes directly in double-spectral form.[2] Noncausal methods, more reminiscent of usual Feynman diagram techniques, but significantly different in spirit, were also developed, and there we showed the power of the technique by some very simple pen-and-ink calculations of 6th-order processes contributing to the electron's magnetic moment.[3]

So what is the legacy of the source-theory experience? I think it is more evolutionary than revolutionary. New techniques were introduced by Julian, principally in the causal

[2] K. A. Milton, L. L. DeRaad, Jr., and W.-y. Tsai, Phys. Rev. D **6**, 1411 (1972)
[3] K. A. Milton, L. L. DeRaad, Jr., and W.-y. Tsai, Phys. Rev. D **9**, 1809, 1814 (1974)

formulation, that supplement those introduced in earlier decades, such as the proper-time technique (which everyone uses nowadays), the quantum action principle (particularly beloved by atomic physicists now), as so on, as detailed by Lowell Brown. One commonality is the emphasis of the power of differential, rather than integral techniques ("It continues to surprise me that so many people seem to accept this formal statement [the solution of the quantum action principle as a path integral] as a satisfactory *starting* point of a theory" [160]). Certainly in my own work that has been a continuing theme, even if the word source theory now occurs but rarely. I interpret the decision of the PACS indexers to remove the "source theory" category not as a sign that source theory has become irrelevant or redundant (in the British sense); but rather that these useful techniques are part of the common language and ammunition that theorists use to attack the most difficult problems in physics.

Let us return to the history.

Source Theory at Harvard

In 1967 "Source and Electrodynamics" [142] was published, which put QED into the new framework. The following year, Julian treated gravitons, and he gave *his* demonstration that full general relativity is essentially a consequence of assuming that the mediator of the gravitational force is a massless helicity-2 particle [146,162,163,177]. It was roughly at this point that I entered the picture, when, as a second-year student, all fear and trembling, I asked Julian if I could work for him. (But I was well prepared, bringing a good knowledge of Green's functions from the University of Washington.) I told him I was also taking Sydney Coleman's field theory lectures and Arthur Jaffe's constructive field theory course, but that was all right with Julian, in spite of his plea for the mind not "warped ... past the elastic limit." (The quotation is from the preface of [153].) The first book treatment of source theory, based on the Brandeis lectures, appeared in 1969 [149]; Julian presented me with a copy for successfully passing my oral exam (which I recall as primarily an argument between Julian and Paul Martin). I also recall the excitement of his source theory treatment of magnetic charge [147], particularly his speculative dyon model of matter which he published in Science in 1969 [150]. (His philosophy here was summed up in his quotation from Faraday: "Nothing is too wonderful to be true, if it be consistent with the laws of nature, and in such things as these, experiment is the best test of such consistency," which I would later find emblazoned on the walls of the old physics building at UCLA, Kinsey Hall.)

Three other books came out in as many years: *Discontinuity in Waveguides* (1968) [148], based on Dave Saxon's notes recording a small portion of his wartime radar work; *Quantum Kinematics and Dynamics* (1970) [152], an unfinished textbook on quantum mechanics, and *Particles, Sources, and Fields, Vol. 1* (1970) [153]. The latter was intended to be a comprehensive treatment of source theory, based on the motto "if you can't join 'em, beat 'em." Harold, the "hypothetical alert reader of limitless dedication," makes his appearance, and unlike a real student, is allowed to interrupt, particularly when he has "an historical gleam in his eye." Julian started writing the second volume of this book during a six-month sabbatical in Tokyo in 1970; on his return, he announced to his twelve or so graduate students that he was leaving Harvard in February 1971 for UCLA. Although I had only begun my fourth year at Harvard, I didn't have long to worry, for half an hour later he informed me, Lester DeRaad, Jr., and Wu-yang Tsai that he had arranged with UCLA to bring us along as postdocs. Little did I guess that my affiliation with UCLA would last a decade!

Source Theory at UCLA

Why did he leave Harvard in 1971? Certainly, he perceived a chilly reception for source theory at Harvard, and thought (more or less erroneously) that UCLA would be more hospitable. But, probably at least as important was the fact he had been at Harvard for 25 years, and felt the need of a change. The sunny climes of Southern California, where he could and did swim and play tennis every day were an enormous attraction. Although it was billed as a temporary move, it was always clear to me that it was to be a permanent change. Appropriately, LA greeted his arrival with a major earthquake. He soon bought a beautiful home in Bel Air, with magnificent views of the city and the ocean.[4] One thing

[4] He also took the opportunity to correct the error in his license plates discussed by Lowell Brown. Since California required at least one letter in vanity plates (unfortunately

Julian did not anticipate: the caliber of graduate students at UCLA was far inferior to what he was used to at Harvard. Consequently, after more than 70 Ph.D.'s at Harvard, I believe only three ever finished at UCLA (only a few more started). (I can only recall Luis Urrutia, Walter Wilcox, and Greg Wilensky.)

Of course, also in 1971 gauge theories took off again, which doomed general reception to source theory. Julian was very much aware of what was going on, and proposed his own $U(2)$ version of the "standard model" in 1972 [155], phenomenologically acceptable in those days. (Shelly Glashow has already reminded us of his fundamental work in making the electroweak synthesis possible.) [We self-styled "sourcerer's apprentices" contributed several papers to the development of the electroweak theory.] For the next two years he worked very hard on the second volume of *Particles, Sources, and Fields* (proofed scrupulously by us three), devoted to electrodynamics, which came out in 1973 [158]. Also in 1973 was the rebirth of strong-field electrodynamics, with the publication of "Classical Radiation of Accelerated Electrons. II. A Quantum Viewpoint" [156], the first paper in which series having been published in 1949 [56]. (This illustrates the continuity of Julian's work, a subject to which I will return.) This led to a series of papers with Tsai [159,176,186], the last of which harkens back to a 1954 paper on the quantum corrections to synchrotron radiation [78]. What Julian viewed as a prediction of J/ψ, in the form of a proposal of an alternative mechanism for avoiding strangeness-changing neutral currents, appeared in the same year [157], which, after the November revolution, was followed a series of related phenomenological forays on the ψ particles [166,169–71,173]. In 1974 he wrote two papers on "renormalization group without renormalization group" [164–65].[5]

With some very impressive work on electrodynamics (including methods harking back to his 1951 "Gauge Invariance and Vacuum Polarization" paper [64] and other classic papers, an independent calculation of the 4th-order contribution to the electron's magnetic moment, and a revisiting of the axial-vector anomaly which he had discovered in [64])

not Greek), he chose brevity and universality: *A137Z.*

[5] Sometime around this point Clarice introduced me to the lovely and talented Margarita Baños, daughter of fellow physicist and Radiation Lab colleague Alfredo Baños. We were married three years later.

constituting the first half of the third volume of PSF, he abandoned work on the book at the point where he had to face up to strong interactions. (The uncompleted third volume eventually came out in 1989 when Addison-Wesley repackaged the whole set [211].) However, he was not about to abandon high-energy physics, for in 1974 Julian continued his iconoclastic interpretation of phenomenology with an alternative viewpoint of deep-inelastic scattering based on double spectral forms (the precursor was the Deser-Gilbert-Sudarshan representation[6]), work which continued until 1977 [167,178,179,179a,181–83], starting from the valid premise that scaling does not necessitate point-like constituents.

Julian revisited magnetic charge in 1975 [172], just in time to hope that "the Price might be right" (paraphrased from *Selected Papers*).[7] A joint analysis of "dyon-dyon scattering" followed in 1976 [180]. He also became interested in the Casimir effect in 1975 [174], I think through conversations with Seth Putterman. We wrote some joint papers on the Casimir effect in 1978, among other things reconfirming Tim Boyer's surprising result on the sign of the spherical effect [187–88]. In 1977 or '78 Julian invited Stan Deser to UCLA to give us some private lessons on supersymmetry; although he submitted a paper on the multispinor basis of supersymmetry in 1978 [190], he kicked himself for not thinking of the idea first: In his words, "All right, wise guy! Then why didn't you do it first?"

During all these years he taught brilliant graduate and undergraduate courses in field theory (source theory) and quantum mechanics, lecturing for two hours a day, twice a week, followed by lunch with Bob Finkelstein and us. At first we ate at various Chinese restaurants, but then, as he became more diet conscious, at the Chatam in Westwood, where he always ordered rare roast beef. Tennis with Lester DeRaad was a regular part of his weekly regime.

I think it was in 1977 that Julian taught graduate electrodynamics, in a typically novel and very insightful way (including variational principles, of course, but especially noteworthy for the preëminence of physics over mathematics), and I suggested we turn the notes into a textbook. We completed a first draft (more properly, version 1.5) of a manuscript, all neatly typed by Gilda Reyes of UCLA, and signed a contract with

[6] S. Deser, W. Gilbert, and E. C. G. Sudarshan, Phys. Rev. **115**, 731 (1959)

[7] Julian often had the television on while doing physics.

W. H. Freeman. Unfortunately, about the time I left UCLA in 1979 Julian decided the manuscript did not sound enough like himself, and started rewriting, resulting in turgidity. The project was abandoned in 1981. However, I taught electrodynamics last fall (scheduled before Julian's death), and will do so again next year, so I have hope of reviving the book.

The Last 15 Years

In 1980, after teaching a quantum mechanics course (a not-unusual sequence of events), Julian began a series of papers on the Thomas-Fermi model of atoms [192–96,201–6]. He soon hired Berthold-Georg Englert replacing me as a postdoc to help with the elaborate calculations. This endeavor lasted until 1985.[8] In 1985 his popular book on relativity, *Einstein's Legacy*, appeared, based on a series of television programmes he presented for the Open University in the UK some years earlier. (Another legacy of those programmes was the robot who graced his living room thereafter.) He wrote three "Humpty Dumpty" measurement theory papers (dealing with spin coherence in a Stern-Gerlach interferometer) in 1988 [208–10], in collaboration with Marlan Scully and Englert. Those who have taken his quantum mechanics courses know how central the Stern-Gerlach experiment was to his formulation of quantum mechanics. He seemed to be spending a great deal of time on several book projects, but to my knowledge, nothing was completed. He also wrote three very interesting homages in the '80's: "Two Shakers of Physics" [200], the pun in the title referring to himself and Tomonaga, "Hermann Weyl and Quantum Kinematics" [208a] in which he acknowledges his debt to one of his "gods," whose ways "are mysterious, inscrutable, and beyond the comprehension of ordinary mortals," and "A Path to Electrodynamics" [212], dedicated to Richard Feynman. In 1989 began a series of papers on cold fusion [213–4,216–20], about which the less said, the better. His last physics endeavor, as I

[8] I understand from conversations after my talk in Washington 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 has been devoted to proving his conjecture about the Z dependence of the ground state energy of large atoms [193], starting with Bull. Am. Math. Soc. **23**, 525 (1990) and continuing through Adv. Math. **111**, 88 (1995).

implied above, was the suggestion that the puzzling phenomenon of sonoluminescence may be due to the "dynamic Casimir effect" [221–8]. (The last paper was submitted on April 30, 1993.) Typically, he was unaware of some of my own papers relevant to the subject, but, atypically, he was very explicitly seeking my collaboration in the last year of his life (I talked to him at some length in December 1993, at the annual Christmas party given by the Baños', which he and Clarice always attended, and at a subsequent lunch). He felt that "carrying out that program is—as one television advertiser puts it—job one" [229]. Jack Ng and I are indeed in the process of doing just that.

Conclusions

How do we place this portion of Julian's career in context? It seems to me that a number of general conclusions may be drawn.

1. I would argue that source theory was not so abrupt a break with the past as Julian presented it. It becomes increasingly clear as one reads PSF, or his general *oeuvre*, that he returns to techniques he invented in the '40's and '50's. Examples are "non-causal methods" which can be found in his famous 1951 "Gauge Invariance and Vacuum Polarization" paper [64], strong field methods, which go back to his early work on synchrotron radiation [56,78] (and also GIVP), and even the theory of sources, which he introduced also in 1951 [66]. He, of course, was aware of this continuity; but he felt the need to emphasize a rather complete break. He saw a great improvement in conceptual clarity, for when he did operator field theory he carried around a great deal of baggage (which *really* is essential) which most people had dispensed with or ignored. Source theory enabled *Julian* to dispense with the "physical remoteness" [153] of renormalization and confront the physics directly. Undoubtedly, with hindsight, we can say that his later work would have had much greater impact if he had not drawn such an exclusive distinction.

2. Of course, probably a bigger impediment to the reception of his ideas was a change in the times. Dispersion relations had died before he mounted his attack, and field theory was reborn with the discovery by 't Hooft that gauge theories of weak and strong interactions made sense. He could accept the electroweak synthesis (to which

he had contributed so much), but not quarks and QCD. The notion of "particles" which were not asymptotic states was too distasteful. (Yet his idea of dyons was not so different—maybe it was just the "unmellisonant" name [150].)

3. In many of his later projects, the first paper in the series was far and away the strongest. He had a very useful idea in the first deep inelastic scattering paper [167], but thereafter the work largely reduced to fitting data with many parameters. Although I am less familiar with that work, a similar characteristic is true of the Thomas-Fermi papers (although here it is the first two papers that stand out). And in the "dynamic Casimir effect" work there is enough in these many short papers for about one substantial article; the essential calculations have yet to be carried out (some of Julian's approximations are, I believe, erroneous); and the relevance to sonoluminescence remains to be established.

4. The last 30 years of his life were not Julian's strongest scientifically. Certainly not for lack of ability: He remained an awesome calculator and a brilliant expositor of unconventional and clever ideas. But the times had changed, and Julian was no longer the molder of ideas for theoretical physics. He is sometimes criticized for venturing into phenomenology—but in fact his first, and quite substantial, papers were phenomenological. [The unfortunate distinction between theory and phenomenology (not one that Julian ever made) is a product of the last decade or so.] Much of his criticism of QCD is quite valid—the theory remains on very tenuous ground, and is more of a parametrization than the first-principles theory it pretends to be. GUTs and strings he found outrageous not because of their theoretical failings but because he, quite rightly, found the notion of a desert between 1 TeV and the Planck scale completely unbelievable—this was, after all, his reason for inventing source theory, to separate high-energy speculations from models of low-energy phenomena.

5. As footnote 8 illustrates, we should not underestimate the power of his work to have a continuing impact. We can confidently expect future surprises. This may be true as well of the many papers in the attached list to which I have not referred, because they do not fit into a well-defined pigeonhole. I can only urge the reader to read his papers, for riches are contained therein.

Eight months before his death, Julian made his first appearance on the internet (and his final publication in any form) with his July 1993 Nottingham lecture, "The Greening of Quantum Field Theory: George and I" (hep-ph/9310283) [229]. This lecture provides a remarkable overview of Julian's work from his own perspective. I commend his final words to you: Like George Green, "he is, in a manner of speaking, alive, well, and living among us."

Acknowledgements

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Publications of Julian Schwinger, 1976–1994

[This updates the publication list found in

Selected Papers (1937–1976) of Julian Schwinger

edited by M. Flato, C. Fronsdal, and K. A. Milton (Reidel, Dordrecht, 1979)]

175. Magnetic Charge, in *Gauge Theories and Modern Field Theory*, eds. R. Arnowitt and P. Nath (MIT Press, Cambridge, Mass., 1976), p. 337.
176. Classical and Quantum Theory of Synergic Synchrotron-Cerenkov Radiation (with W.-y. Tsai and T. Erber), Ann. Phys. (N.Y.) **96**, 303 (1976).
177. Gravitons and Photons: The Methodological Unification of Source Theory, Gen. Rel. and Grav. **7**, 251 (1976).
178. Deep Inelastic Scattering of Leptons, Proc. Natl. Acad. Sci. USA **73**, 3351 (1976).
179. Deep Inelastic Scattering of Charged Leptons, Proc. Natl. Acad. Sci. USA **73**, 3816 (1976).

179a. Deep Inelastic Scattering of Polarized Electrons—A Dissident View, Talk presented at Symposium on High Energy Physics with Polarized Beams and Targets, Argonne Nat. Lab., August 22–27, 1976. (New York, 1976) pp. 288–305.

180. Nonrelativistic Dyon-Dyon Scattering (with K. A. Milton, W.-y. Tsai, L. L. DeRaad, Jr., and D. C. Clark), Ann. Phys. (N.Y.) **101**, 451 (1976).
181. Adler's Sum Rule in Source Theory, Phys. Rev. D **15**, 910 (1977).
182. Deep Inelastic Neutrino Scattering and Pion-Nucleon Cross Sections, Phys. Lett. **67B**, 89 (1977).
183. Deep Inelastic Sum Rules in Source Theory, Nucl. Phys. **B123**, 223 (1977).
184. The Majorana Formula, Trans. N. Y. Acad. Sci. **38**, 170 (1977). (Rabi Festschrift).
185. Introduction and Selected Topics in Source Theory, in *Proceedings of Recent Developments in Particle and Field Theory*, Tubingen 1977 (Braunschweig, 1979), pp. 227–333.
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187. Casimir Effect in Dielectrics (with L. L. DeRaad, Jr. and K. A. Milton) Ann. Phys.

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191. Relativistic Comets, Kinam, **1**, 87 (1979).
192. Thomas-Fermi Model: The Leading Correction, Phys. Rev. A **22**, 1827 (1980).
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194. New Thomas-Fermi Theory: A Test (with L. DeRaad, Jr.), Phys. Rev. A **25**, 2399 (1982).
195. Thomas-Fermi Revisited: The Outer Regions of the Atom (with B.-G. Englert), Phys. Rev. A **26**, 2322 (1982).
196. The Statistical Atom: A Study. University of Miami, P.A.M. Dirac Birthday Volume, 1982.
197. Quantum Electrodynamics, J. Physique **43**, 409 (1982).
198. Electromagnetic Mass Revisited, Found. Physics **13**, 373 (1983).
199. Renormalization Theory of Quantum Electrodynamics: An Individual View, in *The Birth of Particle Physics*, Cambridge University Press, p. 329 (1983).
200. Two Shakers of Physics, in *The Birth of Particle Physics*, Cambridge University Press, p. 354 (1983).
201. Statistical Atom: Handling the Strongly Bound Electrons (with B.-G. Englert), Phys. Rev. A **29**, 2331 (1984).
202. Statistical Atom: Some Quantum Improvements (with B.-G. Englert), Phys. Rev. A **29**, 2339 (1984).
203. New Statistical Atom: A Numerical Study (with B.-G. Englert), Phys. Rev. A **29**, 2353 (1984).
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206. Atomic-Binding-Energy Oscillations (with B.-G. Englert), Phys. Rev. A **32**, 47 (1985).

207. *Einstein's Legacy: The Unity of Space and Time*, Scientific American Library, Vol. 16 (1985).

208. Is Spin Coherence Like Humpty Dumpty? I. Simplified Treatment (with B.-G. Englert and M. O. Scully), Found. Phys. **18**, 1045 (1988).

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210. Spin Coherence and Humpty Dumpty. III. The Effects of Observation (with M. O. Scully and B.-G. Englert), Phys. Rev. A **40**, 1775 (1989).

211. *Particles, Sources, and Fields* (3 volumes), Addison-Wesley, Redwood City, CA (1989).

212. A Path to Quantum Electrodynamics, Physics Today, February 1989. (Reprinted in *Most of the Good Stuff: Memories of Richard Feynman*, ed. L. M. Brown and J. S. Rigden, AIP, New York, 1993, p. 59.)

213. Cold Fusion: A Hypothesis, Z. Nat. Forsch. A **45A**, 756 (1990).

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219. Nuclear Energy in an Atomic Lattice, Prog. Theor. Phys. **85**, 711 (1991).

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225. Casimir Light: A Glimpse, Proc. Natl. Acad. Sci. USA **90**, 958 (1993).

226. Casimir Light: The Source, Proc. Natl. Acad. Sci. USA **90**, 2105 (1993).

227. Casimir Light: Photon Pairs, Proc. Natl. Acad. Sci. USA **90**, 4505 (1993).

228. Casimir Light: Pieces of the Action, Proc. Natl. Acad. Sci. USA **90** 7285 (1993).

229. The Greening of Quantum Field Theory: George and I, Lecture at Nottingham, July 14, 1993 (hep-ph/9310283).

Compiled by K. A. Milton, August 9, 1994.