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"If you can't join 'em, beat 'em": Julian Schwinger's Conflicts in Physics

By Sameer Shah

In the mid 1970s, the widespread acceptance of quantum field theory by the particle physics community pushed out the other major contender in the field, *S*-matrix theory. In the standard historical narrative, these two competing theories each had their own adherents, battling for primacy, but problems riddling both led to widespread uncertainty within the community. "It is not yet clear whether field theory will continue to play a role in particle physics," theorist Steven Weinberg wrote, "or whether it will ultimately be supplanted by a pure *S*-matrix theory."[1] However, during this period of contention, a prominent theorist put forth a third alternative: source theory.

In 1966, the recent Nobel Laureate Julian Schwinger published a short paper in the *Physical Review* titled "Particles and Sources." This paper introduced a program that represented his personal philosophical outlook on particle physics, and, more generally, science. Schwinger staunchly adhered to the philosophy of source theory until his death in 1994. Unlike much of his other work, his 1966 article recieves little attention in the literature of twentieth century particle physics, as is also true of his entire source theory program. Perhaps more significantly, source theory is in many ways the culmination of Schwinger's training and his earlier work. The *historical* literature on the rise of particle physics also generally ignores Schwinger after 1966.[2] Schwinger's philosophy of science was crystallized in his exploration source theory. Understanding the phenomonolgy undergirding source theory and Schwinger's activities within the situation of particle physics during the 1960s, sheds revealing light onto both Schwinger's later life and his experience in the social practice of science.

Schwinger's involvement with cold fusion in the late 1980s and early 1990s echoes his engagement with source theory in the 1960s and 1970s. Cold fusion was at the forefront of Schwinger's thoughts for many years. He wrote journal articles, collected hundreds of newspaper and article clippings, and even terminated his membership to the American Physical Society, all

[1] Kragh, *Quantum Generations: A History of Physics in the Twentieth Century* (Princeton: Princeton University Press, 1999), 337.

[2] Schwinger's single biography, Jagdish Mehra and Kimball A. Milton, *Climbing the Mountain: The Scientific Biography of Julian Schwinger* (Oxford: Oxford University Press, 2000), discusses source theory in detail.

under the banner of cold fusion. Schwinger himself never made a definitive statement on the "reality" of cold fusion; however, he was intrigued by the possibility. Just as source theory reveals the deep philosophical commitments of Schwinger's scientific worldview, so too does cold fusion. Schwinger's biographers—Jagdish Mehra and Kimball Milton—have noted that his philosophy was consistent with his belief in the possibility of cold fusion, and hinted at the connections between his work on source theory and his work on cold fusion.[3] Making these connections more explicit will bring new light to an ambiguous aspect of Schwinger's life work. Reading Schwinger's source theory and cold fusion in concert with each other provides a lense through which to investigate the life and work of an increasingly isolated hero of quantum electrodynamics.

Source Theory

On 11 December 1965, Julian Schwinger gave his Nobel prize address on the history of relativistic quantum field theory to a filled auditorium in Stockholm. Schwinger concluded his lecture with an optimistic statement for the future of quantum field theory, expressing his belief that *phenomenological* relativistic quantum field theory would be the path leading to a bright future.[4] Schwinger rapidly developed this program in the coming years. However, instead of being the path of the future, the theory he dubbed "source theory" became the path not taken.

Source theory can be read as Schwinger's rejection to his own previous work in physics – work that led him to that very stage in Stockholm. Even though source theory never took off as Schwinger had hoped, it is still worth examining because it provides insight into Schwinger's philosophy of physics, and into Schwinger himself. Although source theory never became an important theory in the larger particle physics community, it was a crucial theory for Schwinger. So much so that, Schwinger never relinquished the theory, as he had done with his more prestigious work. From the perspective of the history of science, studying the the marginal theories in a science offers a remedy to a purely teleological and heroic history of science. Further, knowing what ideas did *not* succeed can sometimes illuminate how other did.[5] Finally, Schwinger

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[3] Mehra and Milton, *Climbing the Mountain*, 553.

[4] Julian Schwinger, "Relativistic Quantum Field Theory," in M. Flato, C. Fronsdal, and K. Milton, eds., *Selected Papers (1937-1976) of Julian Schwinger* (Dordrect: D. Reidel Publishing Company, 1979): 279-291.

[5] See, for example, Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: The University of Chicago Press, 1984) and James T. Cushing, *Theory Construction and Selection in Modern Physics: The* S *Matrix* (Cambridge: Cambridge University Press, 1990).

himself reminds us of another reason for studying forgotten theories; he clung to the belief that a theory unpopular in a particular scientific climate can eventually truimph, that discarded theories can be "rediscovered."

The mid-century particle physics community was plagued with the problem of "infinities", which arose when calculating the electromagnetic mass of an electron. In essence, these infinities were the result of the electron within an electromagnetic field interacting back on itself – self-interaction. Experimental evidence made the problem palpable. In 1947, Willis Lamb, using surplus World War II equipment, measured a shift in the energy levels of hydrogen atoms in two particular states. According to classical theories of physics, the energy of the hydrogen in the two states should have been equal; however, Lamb measured a slight but significant difference. Schwinger developed a theory explaining this difference by using a perturbation technique. Freeman Dyson described the initial enthusiasm that characterized those who witnessed Schwinger's famed talk at the New York American Physical Society meeting in January 1948:

> The great event came on Saturday morning, and was an hour's talk by Schwinger, in which he gave a masterly survey of the new theory which he has the greatest share in constructing and at the end made a dramatic announcement of a still newer and more powerful theory, which is still in embryo. This talk was so brilliant that he was asked to repeat it in the afternoon session, various unfortunate lesser lights being displaced in his favour. There were tremendous cheers when he announced that the crucial experiment had supported his theory; the magnetic splitting of two of the spectral lines of gallium (an obscure element hitherto remarkable only for being a liquid metal like mercury) were found to be in the ratio 2.00114 to 1; the old theory gave for this ratio exactly 2 to 1, while the Schwinger theory gave 2.0016 to 1.[6]

The successful renormalization of quantum electrodynamics caused jubilation among the participants of the physics community. Upon hearing of the close matching of experimental results to Schwinger's theoretical predictions, Schwinger's former mentor I.I. Rabi wrote Hans Bethe, "God is great." Bethe responded, "It is as exciting as in the early days of quantum mechanics."[7] In the next few months, alongside Richard Feynman and Shin-itiro Tomonaga, who were also independently working on the same problem, Schwinger developed a theory which removed the difficulties of the infinities. It was soon shown that the three men's methods of removing the infinities, a

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[6] Silvan S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton: Princeton University Press, 1994),

[7] Mehra and Milton, *Climbing the Mountain*, 225.

process known as "renormalization," were equivalent.

Not everyone shared Bethe's enthusiasm, nor would it last. During the mid-1950s there was, in the words of the physicist Steven Weinberg, a "collapse in confidence."[8] Some prominent physicists thought that renormalization was a mere mathematical trick, an ugly procedure, and philosophically suspect. At the same time, efforts to extend quantum field theory (QFT) to two additional fundamental forces, the strong and weak, were met with great difficulty, which was an important problem to contemporary researchers.[9] By 1960, faith in QFT was at its nadir.[10] *S*-matrix theory, an updated version of the original scattering matrix theory developed by Werner Heisenberg in 1925, was the popular alternative formulation to QFT.[11]

Schwinger was alert to of this division among his colleagues. In the early 1960s, he asked:

> Is the purpose of theoretical physics to be no more than a cataloguing of all the things that can happen when particles interact with each other and separate? Or is it to be an understanding at a deeper level in which there are things that are not directly observable as the underlying fields are, but in terms of which we shall have a more fundamental understanding.[12]

The passage illustrates that at this time, Schwinger found both *S*-matrix theory (the former) *and* QFT (the latter) problematic. Unable to renounce both at this time—he had nothing to replace them with—he came down on the side of quantum field theory. Still, if we can trust Schwinger's memory, it was early as 1962 that he began to have doubts about the accepted formulation of QFT.

> I think it was these [two papers on the "Quantized gravitational field"] that pushed me over the edge, the complexity that followed from the operator nature of all these fields simply said to me that this was not the real physics, that this was unnecessarily complicated...The difficulties seemed out of proportion to the nature of the physical questions being asked. It seemed as though the operator formalism was creating

[8] Kragh, *Quantum Generations*, 336.

[9] One of the most outspoken critics of renormalization was Paul Dirac. See Tian Yu Cao, *Conceptual Developments of 20th Century Field Theories* (Cambridge: Cambridge University Press, 1997), 203-204.

[10] For a description of the state of crisis that was felt in physics during this time period, see Kragh, *Quantum Generations*, 336-339.

[11] See chapter 7 in Cushing, *Theory Construction and Selection*, especially pages 169-173.

[12] Schweber, *QED*, 365.

problems of its own rather than being the best way of representing the field situation.[13]

Schwinger questioned the usefulness of retaining operators in quantum field theory on aesthetic grounds. The mathematical formulation using operator fields was "out of proportion," too distant, to the phenomena they described.

It was in this state of crisis in the high energy physics community that Schwinger put forth his alternative to both operator field theory (Schwinger's term for the popular form of QFT) and *S*-matrix theory.[14] If the physics community at large had subscribed to a single theory—if there was no "crisis"—it would have proved more difficult for Schwinger to put forth his alternative. The "collapse in confidence" in the two existing explanatory models was pivotal for Schwinger's presentation of source theory, which relied more on philosophical differences rather than on explanations of previously inexplicable phenomena. In his textbook, the first in a series of three, Schwinger positioned source theory in between the two prominent theories.[15] Adherents of source theory program gained the advantages of a simpler theory, without the drawbacks of the two philosophically unsound alternatives.

Operator field theory (QFT), for Schwinger, was exceedingly problematic. In operator field theory, the particle was relegated to a "stable or quasi-stable excitation of the fields."[16] In other words, the particle took second seat to the operator fields. Experimental physicists, however, connected to the particles themselves, not the fundamental fields that "generated" them. In addition, the theory *presupposed* its correctness at distances smaller than had been experimentally tested.[17] Furthermore, operator field theorists made an additional assumption regarding the interaction of two or three fundamental fields to make a baryon or meson. "In other words," Schwinger declared, "I

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[13] Mehra and Milton, *Climbing the Mountain*, 455.

[14] A brief treatment of alternative theories to quantum field theory, including some on source theory, can be found in Henrik Zinkernagel, "High-Energy Physics and Reality – Some Philosophical Aspects of a Science" (PhD Dissertation, Niels Bohr Institute, 1998), especially chapter 5. Schwinger himself used term "crisis" to describe this situation, in Julian Schwinger, *Particles, Sources, and Fields, Volume 1* (Reading, MA: Addison-Wesley Publishing Company, 1970), *iii*.

[15] Schwinger, *Particles, Sources, and Fields, Volume 1*.

[16] On 338 in Julian Schwinger, "Theory of sources" in M. Flato, C. Fronsdal ,and K. Milton, eds., *Selected Papers (1937-1976) of Julian Schwinger* (Dordrect: D. Reidel Publishing Company, 1979): 337-361.

[17] Julian Schwinger, "Julian Schwinger's Approach to Particle Theory," *Scientific research* 4, no. 17 (1969): 19.

make here the rather serious objection that in order to be able to talk about the physically interesting phenomena at all one must begin with a speculation about how these particles are formed."[18]

Schwinger's objections to *S*-matrix theory were different, but just as significant. *S*-matrix theory placed the *S*-matrix itself at its center. Schwinger spurned this theory because one could only "correlate what comes into a collision with what goes out, and cease to describe in detail what is happening during the course of the collision."[19] *S*-matrix theory relinquished QFT's space-time description, replacing it with a momentum space description. Moreover, because the *S*-matrix has no time description associated with it, when working with the theory, one could only be concerned with stable particles. Even more problematic for Schwinger, since particles were taken to be fundamental in the popular "bootstrap model" of the *S*-matrix (where all particles generate all other particles), it *precluded* the possibility of a deeper level of understanding of particles. This unwarranted assumption – that particles do not have an internal structure – could "intervene" negatively in the sense that it could wrongly guide future experimental and theoretical work. Though he acknowledged both QFT and *S*-matrix theory could prove correct (though probably not), he did not want to have these possibly flawed assumptions directing future research.[20]

Schwinger bypassed the problems underlying the dominant theories by turning to phenomenology. In the greater physics community, phenomenology provided a means for theorists and experimentalists to assist each other. The early 1950s provided an embarrassment of riches for physicists, coming off World War II. With new resources and an improved status in the public's eye, experimental particle physics grew. It was a time characterized by the use of powerful, expensive instrumentation by experimentalists. Bubble chambers helped established a veritable zoo of new particles. The period from 1954 to 1968 saw rapid development in bubble chamber technology, where new devices were built to "read" the numerous photographs being taken.[21] How to unify this giant mass of data with theory became a fundamental concern for physicists.[22]

The preface to a conference proceeding on phenomenology at Caltech declared that the goal of phenomenological work was to close

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[18] Schwinger, "Theory of Sources," 338.

[19] Schwinger, "Julian Schwinger's Approach to Particle Theory," 22.

[20] Julian Schwinger, "Theory of Sources," 339.

[21] Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: The University of Chicago Press, 1997), 370.

[22] Ibid.,, 641-668.

the "widening wedge between theories and experiment."[23] Doing this, the proceedings promised, could point to deficiencies in theory as well as highlight important experiments to perform. Also, by comparing experiment and theories, a researcher could find experimental anomalies that the theories could not explain, leading to new areas of fruitful and relevant research.[24]

Schwinger took a similar stance in his own phenomenology:

> The word phenomenological as I use it here [with source theory], I think, does not have the same associations for me that it did for Professor Heisenberg. I regard this as a phenomenological theory in the sense that we are dealing with the actual phenomena, but it is a creative theory in the sense that different phenomena are connected by fundamental principles. The procedures used, however, are flexible and reflect the complexity of the physical problem and the amount of information available.[25]

His phenomenological philosophy saw *two* types of theories: a *fundamental* theory and a *phenomenological* theory. The phenomenological theory is an *idealization* of physical observations (the data). Fundamental theories, on the other hand, are designed to "explain the relatively few parameters of the phenomenological theory in terms of which the great mass of raw data has been organized."[26] Schwinger's phenomenological method began with a simple phenomenon and created a phenomenological theory. Hypothesis were extrapolations from the phenomenological theory that lay outside the immediate domain, and awaited experimental confirmation. Experimentation was integral to this program – all hypotheses (extrapolations) needed testing. Instead of a "trickle down" theory, assuming the correctness of a grand superstructure, Schwinger's program advocated building up "from below."

Schwinger believed that phenomenological work was crucial because it separated *speculation* from *theory*. His work on source theory highlighted the unacknowledged assumptions behind both operator field theory and *S*-matrix theory. Specifically, operator field theory assumed the fields as the generator of the particles, while the *S*-matrix theory assumed the particles were fundamental entities (all generating the other). On the

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[23] C.B. Chiu, G.C. Fox, and A.J.G. Hey, eds., *Phenomenology in Particle Physics 1971: Proceedings of the Conference Held at the California Institute of Technology March 25 and 26, 1971* (Pasadena, CA: California Institute of Technology, 1971), *v*.

[24] Ibid., *v*.

[25] Schwinger, "Theory of sources," 341.

[26] Schwinger, "Julian Schwinger's Approach to Particle Theory," 19.

other hand, source theory was a purely:

> phenomenological theory, designed to describe the observed particles, be they stable or unstable. No speculations about the inner structure of particles are introduced, but the road to a conceivable more fundamental theory is left open. No abstract definition of particle is devised; rather, the theory uses symbolic idealizations of the realistic procedures that give physical meaning to the particle concept. The theory is thereby firmly grounded in space-time, the arena within which the experimenter manipulates his tools, but the question of an ultimate limitation to microscopic space-time description is left open, with the decisions reserved to experiment. [27]

With the assistance of his phenomenological program, his theory would never preclude a future theory because of assumptions. Although Schwinger acknowledged the use of speculation in physics, he made it a point to keep speculations as speculations, and nothing more. [28]

What are sources and how do they encapsulate Schwinger's philosophy? Sources are operationally defined via the situations they are studied—experimental collisions. [29] He explains, "a theory is...an abstraction and idealization in which one focuses on what is important about the particular acts that are involved...the first thing to do in developing a theory is to abstract from the details of the realistic collision." [30] A source S is used to represent a collision where particles are created. If S is large, the

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[27] Schwinger, *Particles, Sources, and Fields, Volume I*, 37

[28] I am certainly not the first to notice Schwinger's conservative approach to physics. Pauli noted that "He must have strong psychological reasons for the very conservative appearance of his theory." in Schweber, *QED*, 252.

[29] The term "operational" is Schwinger's: "How does one go about reconstructing a theory of particles in this phenomenological sense? By paying strict attention to the operational definition of a particle that is provided by the experimenter's manipulations, rather than through some a priori definition of a particle." Quoted in Julian Schwinger, *Introduction and Selected Topics in Source Theory* (Braunschweig: Friedr. Vieweg & Sohn, 1977), 229. His students also took this aspect away from working with him: "What did I carry away with me from my years with Schwinger? The self-admonition to try and measure up to his high standards; to dig for the essential; to pay attention to the experimental facts; to try to say something precise and operationally meaningful even if – as is usual – one cannot calculate everything; not to be satisfied until one has embedded his ideas into a coherent, logical, and aesthetically satisfying structure." Quoted in Paul C. Martin, "Julian Schwinger—Personal Recollections" in Y. Jack Ng, ed., *Julian Schwinger: The Physicist, the Teacher, and the Man* (Singapore: World Scientific, 1996), 88.

[30] Schwinger, *Introduction and Selected Topics in Source Theory*, 230-231.

probability that a particle that is created is large, and vice versa. The source, when quantified, becomes an idealization of the experimental procedure used to produce a particle.

Particles, Sources, and Fields

Schwinger intended his textbook *Particles, Sources, and Fields* for the student uninitiated with operator field theory and *S*-matrix theory: "I think it of the utmost importance that such acquaintance with the liberating ideas of source theory occur before exposure to one of the current orthodoxies has warped him past the elastic limit."[31] It is significant that Schwinger decided to write a textbook. Not only did it provide a "guidebook" for this new formulation of particle physics, a mathematically distinct formulation from the two commonly used and understood formulations, but it also provided a way to enroll younger physicists into a particular methodological and philosophical program. In the preface to his textbook, Schwinger revealed his strong personal convictions for the future of source theory, and the inadequacy of the existing models. He refused to give a historical account, providing instead priority to new ideas and techniques, because "it would have been too distracting if constant reference to *techniques for which obsolescence is intended* had accompanied the development of the new approach."[32]

In fact, spreading the gospel of source theory was so important that in his first letter querying Addison-Wesley's interest in becoming his publisher, Schwinger made four demands: "all possible speed in publication," "freedom from arbitrary editorial interference," "widespread advertising," and "low price." This book was his *piece de resistance*, his "highly personal reaction" to particle physics. Even more importantly, in this letter Schwinger emphasized his desire to "keep cost from standing in the way of its widespread distribution," so that instructors and graduate students *must* learn of the book's existence.[33]

During the 1950s and 1960s, textbook publishers responded to the demand generated by the rise in graduate-level enrollments in physics by publishing an increasing number of textbooks and lectures. The physicist Geoffrey Chew and his student Maurice Jacob published their summer school lecture notes cost effectively: "Photo-offset printing is used throughout, and the books are

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[31] Schwinger, *Particles, Sources, and Fields, Volume I, iii.*

[32] Ibid., *iv.* Emphasis added.

[33] Julian Schwinger to Melbourne W. Cummings, 26 November 1969, in the Julian Seymore Schwinger Papers in UCLA Special Collections (Collection 371, Box 17, Folder 1). From here on, I will write this as *JSS*, Box 17, Folder 1. This was not an uncommon practice at this time. The phrase "highly personal reaction" is in *Particles, Sources, and Fields, Volume I, iii.*

paperbound, in order to speed publication and reduce costs. It is hoped that the books will thereby be within the financial reach of graduate students in this country and abroad."[34] David Kaiser has noted that most of the new texts were not all advocating a single "theory"—as we have seen there was no single theory accepted by a large proportion of the community—but rather focused on presenting "techniques."[35] Many of those publishing were staking their claim in the future of physics. The situation of the post-war physics community created an opening for the publication of new textbooks. Not only did publishers approach physicists to write textbooks, but the growth of new theories provided those with different views a market in which to express them.

Schwinger's demand for freedom from arbitrary editorial influence indicates that, *Particles, Sources, and Fields* was not a standard textbook. Schwinger's message was conveyed from the start, with his epigraph: "If you can't join 'em, beat 'em." His book is an offensive challenge to the scientific establishment. These few words reveal the position that Schwinger abided by for the much of the rest of his career. He was not making nice or backing down. Instead, setting himself up for conflict, Schwinger did not write this text seeking the approval of the larger scientific establishment; indeed, he deemed both operator field theorists and *S*-matrix theorists part of the problem. Schwinger thought that the advocates of these theories were "warping" uninitiated student minds beyond repair; *Particles, Sources, and Fields* was designed to save them.

Though Schwinger was discontent with the complexity of operator field theory, source theory was not the necessary result. We must wonder, then, what experiences and resources Schwinger drew on that led to his phenomenological stance. At least some of the origins of this outlook can be traced to Schwinger's war work.[36] In 1942, while a professor at Purdue University, none other than physics luminary Hans Bethe recruited Schwinger to the MIT Radiation Laboratory that summer. The group Schwinger headed was charged with developing a usable account of microwave networks. Because the high frequency of microwaves rendered existing methods were rendered useless Schwinger returned to the basic and fundamental Maxwell equations, which he was dismayed to realize contained unnecessary information. He later wrote,

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[34] Kaiser, "Nuclear Democracy," 257.

[35] See chapter 7 in David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005).

[36] Schwinger, "Julian Schwinger's Approach to Particle Theory," 19.

> As far as any particular problem is concerned, one is only interested in the propagation of just a few modes in the wave guide. A limited number of quantities that can be measured or calculated tell you how these few modes behave and exactly what the system is doing.[37]

Rather than working from abstract theory, Schwinger began to use practical representations, simple circuits, which mimicked the desired field behavior. These circuits were *symbols* (what Schwinger would sometimes call "idealizations") rather than *actual explanations* of how things worked.

Historian Peter Galison has drawn attention to the fact that Schwinger was placed in a location where he assimilated some engineering culture. He argues that during his time at the Radiation Laboratory, Schwinger had constructed for himself a meeting point between physicists and engineers, and equivalently, between Maxwellian field theory and radio engineering.[38] This time in Boston was necessary for Schwinger to gain hands-on experience connecting data to theory. I argue that this work connects up nicely with source theory. Schwinger's work on source theory draws upon the practice he had at the Radiation Lab using the engineering method of studying input-output relations. A source is precisely that, an idealization which represents the creation of a particle "through the net balance between what enters and what leaves the collision."[39]

Schwinger credited his famed later work on renormalization to this period:

> The waveguide investigations showed the utility of organizing a theory to isolate those inner structural aspects that are not probed under the given experimental circumstances.... And it is this viewpoint that [led me] to the quantum electrodynamics concept of self-consistent subtraction or renormalization.[40]

Importantly, when Schwinger later denounced renormalization and operator field theory, he then used this same experience at the Radiation Laboratory to explain the motivation for source theory:

> I want to argue that we should adopt a pragmatic engineering approach. What we should *not* do is to try to begin with some fundamental

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[37] Schwinger, "Julian Schwinger's Approach to Particle Theory," 19.
[38] See Galison, *Image and Logic*, 820-828
[39] Schwinger, *Introduction and Selected Topics in Source Theory*, 231.
[40] Galison, *Image and Logic*, 826.

> theory and calculate. As we saw, this is not the best thing to do even when you have a fundamental theory [i.e. like Maxwell's equations in the Rad Lab], and if you don't have one [i.e. like in high energy physics], it's certainly the wrong thing to do.[41]

This period of Schwinger's life was formative in his later views on how to approach physics.

While at the Radiation Laboratory, Schwinger's isolationist tendencies revealed themselves. Though he was around the Radiation Laboratory, he worked mainly with a select few collaborators, such as Harold Levine and Nathan Marcuvitz. For the rest, he organized a lecture series. At nights, Schwinger would solve problems left for him on his desk, and wander the halls and solve problems left on chalkboards. Isolation was not specific to this period in Schwinger's life, but endemic to his being.

From early in his career in physics, Schwinger was afraid of domination. When Schwinger was sent by I.I Rabi in 1937 to Wisconsin to study with Breit and Wigner, he started doing most of his work at night so that he would not be "dominated."[42] He said the same thing about Oppenheimer when he went to Berkeley to work.[43] After he got married in 1947, after the war had ended, people noticed that Schwinger's isolationist tendencies had heightened. Historian Silvan Schweber notes:

> The contrast between Schwinger before and during the war and the later Schwinger merits comments. The warm and affectionate encomium of his prewar and wartime colleagues and acolytes is markedly different in tone from the criticisms of his students at Harvard. It is also interesting to note that while most of the papers Schwinger wrote before and during the war were collaborative efforts, the majority of his papers on work done after the war were written by himself... All this reflects his working style after the war: he becomes more and more a loner. There is a tragic aspect to Schwinger's life after 1950, for he becomes progressively more and more isolated from the physics community...Schwinger's personality was undoubtedly a factor. David Saxon has observed that "Schwinger always wanted to do everything for himself, by himself. And he would want to do it his own way. He insisted on doing it his own way."[44]

Isolation provided a way for Schwinger to remain independent—his work would remain protected from the influences of dominating personalities

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[41] Schwinger, "Julian Schwinger's Approach to Particle Theory," 19.
[42] Schweber, *QED*, 285. Also see pages 19 and 20 of the transcript "Reminisces of the Thirties," videotaped on 29 March 1984 and in *JSS*, Box 7, Folder 28.
[43] Ibid., 289.
[44] Ibid., 370-371.

and mainstream ideas. It is partly out of this ability to work outside of the mainstream, and his insistence of doing everything himself, that Schwinger was able to craft source theory.

Schwinger had already predicted that his textbook, published in 1970, would be a hard sell for physicists. In 1966 he was able to successfully apply source theory to pion physics, which he saw as an encouraging sign. However, he felt others were not appreciative of the significance of this accomplishment, and *Particles, Sources, and Fields* was written to remedy the situation.[45] The work concludes on a defensive but cautiously optimistic tone, with a short dialogue between Harold – an imaginary student – and Schwinger:

> *H*: How can it be the end of the book? You have hardly begun. There are any number of additional topics I should like to see developed from the viewpoint of source theory. And think of the field day you will give the reviewers, who usually prefer to list all the subjects not included in a volume rather that discuss what it does contain.
> *S*: Quite true. But we have reached the point of transition to the next dynamical level. And, since this volume is ready of a reasonable size, and many of the ideas of source theory are in it, if hardly fully developed and applied, it seems better to put it before the public as the first volume of a series. Hopefully, the next volume will be prepared in time to meet the growing demand for more Source Theory.[46]

Schwinger eventually did publish a second volume of *Particles, Sources, and Fields* in 1973 – but that was after a damning reception of the first.

Arthur S. Wightman, a Princeton physicist, began his condemnation of the book by stating the mathematical and intellectual demands the "Schwingerian code" places on a student will likely "baffle or hornswoggle." This comment is revealing. Schwinger refused to use diagrams of any sort in his book, preferring instead to express formalism through series of equations.

Moreover, *Particles, Sources, and Fields* advocated a complete rejection of operator field theory and *S*-matrix theory, both theories that, by this time, physicists had become familiar navigating. Instead, Schwinger proposed that readers accept not only the philosophical basis undergirding his theory *and* a new framework for working problems, but also required that readers learn to use his unfamiliar computational tools.[47] The reception of Schwinger's early work in source theory did not proceed the way Schwinger envisioned it. A peer reviewer for *Physical Review Letters* rejected one of Schwinger's early

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[45] Schwinger, *Particles, Sources, and Fields, Volume I*, *iii*.
[46] Ibid., 406.
[47] Mehra and Milton, *Climbing the Mountain*, 481.

papers partly because he saw the work as using the Lagrangian "in lowest order only." In the traditional operator field theory paradigm, Lagrangians were *operators*, thus having higher order corrections. But Schwinger's source theory fundamentally rejected the use of operators, and instead used the Lagrangian as a *function*.[48] Steven Weinberg said that he did not pick up source theory in the decade after its introduction because he found the conceptual framework "unfamiliar."[49] The reviewer Wightman was not willing to give up on the existing methods, and learn to navigate new waters, on the grounds that "the evidence offered for computational power of the source method is not convincing."[50] In brief, the costs did not outweigh the benefits. The rejection of *Particles, Sources, and Fields* was devastating; two of Schwinger's former students characterized the reaction to source theory as "nearly universally negative."[51]

Schwinger did not take Wightman's criticism lying down. Schwinger responded head-on to Wightman's review by sending a response to *Science*, critiquing the critique. In this retaliation, Schwinger addressed Wightman's specific concerns, and then pointed out that it was not source theory that was problematic, but rather the rigidity of Wightman's mathematical foundations that inhibited "real research."

> The reviewer's comments on it are quite predictable. He is simply not interested in the physically oriented source theory; he prefers his own mathematical viewpoint... The reviewer is not impressed with the computational power of source theory as illustrated in the book and, as Harold anticipated, wishes to examine calculations that are outside the province of the first volume. I do understand the desire to see more incisive tests of the method, and I am hard at work on the second volume... The most revealing indication of the constraints under which the reviewer labored is contained in the pronouncement of the last sentence: "...the theory is too plastic." Source theory is a working physicist's tool. It is flexible; it is adapted to the tentative probings that are the path of real research. To the overly mathematical, who can only understand a fixed structure, laid down in advance, all this is anathema.
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> I think it will not be long before source theory clearly shows its superiority through its ability to break with rigid preconceptions.[52]

The journal *Science* declined publishing Schwinger's response, though,

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[48] Julian Schwinger to George L. Trigg, editor of *Physical Review Letters* (March 1967), *JSS*, Box 17, Folder 1.

[49] Mehra and Milton, *Climbing the Mountain*, 474.

[50] A.S. Wightman, "The Source Method," *Science* 171 (1971): 889-890.

[51] Mehra and Milton, *Climbing the Mountain*, 481.

[52] "A Reply to a Book Review" in *JSS*, Box 10, Folder 11.

saying that the difference between Wightman and Schwinger was one of "judgment and opinion" on the theory itself, and thus required a different forum for discussion.[53]

In the face of an initial unfavorable reception, Schwinger did not stop his work on source theory. The "If you can't join 'em, beat 'em" epigraph in *Particles, Sources, and Fields* displays his willingness to go against the grain. One of his former students Jack Ng recalled: "He stuck staunchly to his source theory approach to the end. Some would charge him of stubbornness. Curiously, I think he would have gladly pled guilty to that. 'Stubborn? Who isn't?' he used to ask me."[54] Schwinger continued working on source theory for years. In one of his early works elaborating source theory after the publication of his textbook, Schwinger proposed the existence of dyons—dual-charged particles accorded fractional electric charges.[55] However, not only had these particles not been observed; there also was no direct experimental evidence to suggest their reality. Schwinger's phenomenological approach denounced the *S*-matrix model because it *assumed* that there was no substructure to the particles. Did the theory of dyons do something similar, assume the existence of a dyon substructure to particles when there was no direct experimental evidence? Part of Schwinger's phenomenological outlook was separating the speculative from the theoretical, and the article on dyons was no exception:

> A conceivable dynamical interpretation of the subnuclear world has been erected on the basis of the speculative but theoretically well-founded hypothesis that electric and magnetic charge can reside on a single particle. I hope that these suggestive, if inadequate, arguments will be sufficiently persuasive to encourage a determined experimental quest for the portal to this unknown new world of matter, for:
>
> > *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.*
> >
> > --Faraday[56]

The separation of theory and speculation is important in Schwinger's phenomenology. However, as the Faraday quotation reveals, speculation can

[53] *JSS*, Box 17, Folder 7.

[54] Y. Jack Ng, "Schwinging a Sorcerer's Wand: Julian and I," in Y. Jack Ng, ed., *Julian Schwinger: The Physicist, the Teacher, and the Man* (Singapore: World Scientific, 1996), 120.

[55] Julian Schwinger, "A magnetic model of matter," *Science* 165 (1969): 757-761.

[56] Ibid., 761.

be useful too. It can lead to experimental tests and perhaps to new theories. As we shall see, Schwinger came to embrace another particular speculation—a mechanism for cold fusion.

Conflicts in Physics

In 1977, a few years after he had left Harvard for UCLA, Schwinger presented a lecture on the convoluted history of the kinetic theory of matter – tracing the topic from its Greek origins to Einstein. Titled "Conflicts in Physics," this work reveals a belief that science is a process involving fallible human nature, a strong conformist culture, and vicious competition.[57] The lecture on the history of science not only reveals Schwinger's view of scientific process in the days of Boltzmann and Lord Rayleigh, but, more importantly, it is a thinly-disguised indictment the operation of science in 1977. The lecture provides a glimpse into what Schwinger saw "wrong" about the scientific community within which he operated, but it is also a statement about how science ought to operate.

The lecture begins by quoting a letter to the editor of *Science* about the importance of *open* controversies in science. Beyond simply denouncing the concealing of disputes within science, the letter noted that such concealment is harmful because it closes the true workings of science to the public:

> Science is a means of systematically challenging the concepts of reality and it is inevitable that those whose conceptions are challenged will become personally involved in controversy. Given the enthusiasm, commitment, and dedication that the practice of science demands, the existence of fights and rivalries can be taken as a sign of vitality in a field. Science's bad press will grow worse as long as the public continues to believe that scientific "truth" is found scattered about the landscape like so many Easter eggs and is merely picked up by cooperative, truth-seeking scientists. Scientific progress results from the constant competition of ideas, with the best ideas (and scientists) emerging as successful.[58]

For Schwinger, at the heart of it, science *is* about finding a proper

[57] In the introduction to Schwinger's popular science book, he notes "science is a human activity, with practitioners who share the strengths and weaknesses of all people, although not always in the same proportions." In Julian Schwinger, *Einstein's Legacy: The Unity of Space and Time* (New York: Scientific American Books, Inc, 1986), *xi*.

[58] "Conflicts in Physics," 1-2, in *JSS*, Box 28, Folder 14. Schwinger involved himself in educating the public about science by working on a BBC production and by writing a popular science book based on the BBC production titled *Einstein's Legacy*.

conception of reality. His phenomenological outlook provided him the means to do this—retaining the physical world in theories by a process of symbolic idealization. Fundamental to source theory was abstracting reality, the particle collisions used by experimenters to test nature, into mathematical formulas. And as evidenced by the introduction of source theory into a discipline where many other theories were present, science is about competition.

In the heart of the lecture, dealing with the discovery and eventual acceptance of the rise of the kinetic theory of matter, Schwinger notes two pieces of scientific work that had been ignored by their contemporaries, works that contained ideas that were to be eventually vindicated. John Herapath's 1820 paper "A Mathematical Inquiry in to the Causes, Laws, and Principal Phenomena of Heath, Gases, Gravitation, Etc." proposed a kinetic theory that could explain numerous physical phenomena. However the Royal Society did not publish the paper. The reason Schwinger cited: the paper was too speculative and without experimental justification. "Any scientist," Schwinger emphasized to his audience, "who has had to suffer the critical remarks of a referee of his paper will sympathize."[59] Speculation, Schwinger argues, is good for science, as it can lead the way to fundamental theories. The second paper was by John James Waterson, sent to the Royal Society in 1845, and also not published at that time. It contained a direct connection between temperature and energy. It was not until 1892 that Lord Rayleigh found and published the paper, with an apology, an episode with which Schwinger identified:

> "The history of this paper suggests that highly speculative investigations, especially by an unknown author, are best brought before the world through some other channel than a scientific society, which naturally hesitates to admit into its printed records matter of uncertain value. Perhaps one may go further and say that a young author who believes himself capable of great things would usually do well to secure the favourable recognition of the scientific world by work whose scope is limited, and whose value is easily judged, before embarking upon higher flights." These last remarks of Rayleigh apply equally well to the scientific establishment of today. A young author, or indeed an older one, who departs from conformity with the main stream of scientific opinion does so at his peril.[60]

Of course, we can see Schwinger presenting himself as taking the advice of the great Rayleigh. He established himself with the renormalization of quantum electrodynamics, and on the podium in Sweden, accepting the Nobel Prize, he began to envision his "higher flights."

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[59] "Conflicts in Physics," 4 in *JSS*, Box 28. Folder 14.
[60] "Conflicts in Physics," 8 in *JSS*, Box 28, Folder 14.

The elaboration of the histories of the two papers highlights the "pettiness of individual men and the arrogance of institutions" but they also raise the concept of the non-linearity of science, where a discarded notion or idea can eventually reemerge.[61] Schwinger cited Boltzmann exclaiming, "I am conscious of being only an individual struggling weakly against the stream of time. But it still remains in my power to contribute in such a way that, when the theory of gases is again revived, not too much will have to be rediscovered...One regrets almost that one must pass away before their decision."[62] Threaded throughout this text is a sense that unpopular ideas can eventually vindicate themselves, becoming central to the scientific community, regardless of their initial unfavorable reception. Schwinger must have felt the same with his source theory. His closing lines sum up his current view of science, his history lesson informed by these beliefs:

> If my history lesson has done nothing else, it should have reminded you that, during any given period in the evolving history of physics, the prevailing, main line, climate of opinion was likely as not to be wrong, as seen in the light of later developments. And yet, in those earlier times, with relatively few individuals involved, change did occur, but slowly...What is fundamentally different in the present day situation in high energy physics is that large numbers of workers are involved, with corresponding pressures to conformity and resistance to any deflection in direction of the main stream, and that the time scale of one scientific generation is much too long for the rapid pace of experimental discovery. I also have a secret fear that new generations may not necessarily have the opportunity to become familiar with dissident ideas.
>
> I can only echo the heart-felt cry of Boltzmann, "Who sees the future? Let us have free scope for all directions of research; away with dogmatism."[63]

A sense of despair for the direction of physics comes through in this passage. Perhaps it is his "secret fear" which motivated Schwinger's

[61] "Conflicts in Physics," 9 in *JSS*, Box 28, Folder 14.
[62] "Conflicts in Physics," 12 in *JSS*, Box 28, Folder 14. This idea was not new to Schwinger in 1977. He had made similar remarks earlier: "Now [in 1967], here then was the point which I began to appreciate, that it was possible—in fact, it was something desirable—to move against the current of what was then generally accepted thought, that what one's colleagues believed at a particular moment of time was not necessarily the actual, effective, eventual development of thought in the realm of physical theory." In Mehra Milton, *Climbing the mountain*, 456.
[63] "Conflicts in Physics", 18-19 in JSS, Box 28, Folder 14.

interest to engage with ideas unpopular with the mainstream, ideas such as cold fusion.

On 23 March 1989, Martin Fleishmann and Stanley Pons – two chemists at the University of Utah – held a press conference announcing the discovery of "cold fusion," the ability to create fusion at room temperature. Their simple apparatus required only some heavy water, a palladium cathode, a platinum anode, lithium salt, and a battery. The press had a field day with this "revolutionary" announcement, and it was not long before scientists around the globe were trying to recreate the experiment with what little knowledge they were able to gather from media accounts.[64]

One of these scientists was Julian Schwinger. Printed on 1 May 1989, a couple of weeks after being written, Schwinger's letter to the editor of the *Los Angeles Times* outlined a potential explanation for cold fusion—and a simple experiment to test it.[65] He cast a broad net, asking if "someone, with access to an apparatus producing heat and neutrons, [could] please look at the evolved gases to see whether Helium-4 is present? Should it be—and mindful of the large energy released in this reaction—are there sufficient numbers to account for the heat generated?" It is not completely surprising that Schwinger used the *Los Angeles Times* to voice his ideas. Pons and Fleishmann held their press conference *before* submitting their results to a peer-reviewed journal. Attempts at replication were confronted with simple problems such as determining the size of the electrodes, how long the experiment should run, and whether the lithium salt could be substituted.[66] Schwinger's initial interest in cold fusion was compelling enough to have him write the *Los Angeles Times*.

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[64] For general introductions to the cold fusion saga from the history of science standpoint, see chapter 4 of Harry Collins and Trevor Pinch, *The Golem: What You Should Know about Science, Second Edition* (Cambridge: Cambridge University Press, 1998); Chapter 4 of Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (Chicago: The University of Chicago Press, 1999); Bruce V. Lewenstein, "Cold Fusion and Hot History," *Osiris, 2nd Series* 7 (1992): 135-163; Bart Simon, "Undead Science: Making Sense of Cold Fusion after the (arti)fact," *Social Studies of Science* 29, no. 1 (1999): 61-85.

[65] Julian Schwinger, "Table Top Fusion" *Los Angeles* Time, May 1, 1989. The handwritten version – slightly different – is in *JSS*, Box 4, Folder 15. Eugene Mallove suggests in his account that because Schwinger could not get in touch with Pons, he resorted to turning to a public forum. In Eugene F. Mallove, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (New York: John Wiley and Sons, Inc., 1991), 81.

[66] Collins and Pinch, *The Golem*, 68. In fact, Collins and Pinch note that scientists were receiving their information from myriad informal sources such as email and telephone conversations. In the midst of the flood of requests for more information, after the press conference, Pons and Fleishmann's were accused of deliberate secrecy.

This letter was just the beginning of Schwinger's fascination with cold fusion. In his archived papers, collected after his death, there were numerous newspaper, magazine, and journal articles related to cold fusion.[67] The dates of these publications span until close to his death in 1994. Nearing the end of March, 1990, Schwinger attended the First Annual Conference on Cold Fusion (ICCF1) in Salt Lake City. And in December 1993, he had a paper read for him at ICC4. Schwinger, historically, did not like to sign petitions.[68] He, however, signed a petition to the Science, Space and Technology Committee of the House of Representatives, arguing for Congress to appropriate a significant amount of funding for further research—a minimum of $10 million.[69] At the very least, these facts illustrate is a passionate interest in cold fusion, one that outlived the media hype and most researchers interest in the subject matter.[70]

Julian Schwinger's early entrance into the discussion on cold fusion was in part due to his scientific curiosity – *how* cold fusion might occur – but as time went on, his fundamental conviction that a scientific issue should not be dismissed outright because of its unpopularity led him to use cold fusion as a forum to express his own contempt for some features of the existing scientific establishment. His philosophical phenomenology tempered his own scientific work on cold fusion, allowing him to use experimental evidence to point to a potential for cold fusion, and for forming hypotheses to explain the mechanism by which cold fusion operated.

Although Schwinger never staked a claim for or against the actual *reality* of cold fusion, he concerned himself with finding a plausible mechanism to explain the experimental data that had been generated.[71] "Ordinary" fusion reactions with heavy water (D-D reactions) yield neutrons, energy in the form of a γ-ray, ^3He, and ^4He. Critics of cold fusion noted that experiments did not yield neutrons nor energy – at least not in the amounts warranted by their analysis of the reaction. Schwinger, on the other hand, took another approach to the problem arguing that the reaction which drove the cold fusion was not the D-D reaction. Rather, since all heavy water is contaminated with ordinary water, there could be

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[67] *JSS*, Box 4.

[68] Mehra and Milton, *Climbing the Mountain*, 568.

[69] *JSS*, Box 4, Folder 10.

[70] See, for example, the study on communication during the cold fusion episode, including *figure 3* illustrating the number of media and scientific publications over time, in Bruce V. Lewenstein, "From Fax to Facts: Communication in the Cold Fusion Saga," *Social Studies of Science* 25, no. 3 (1995): 403-436.

[71] Schwinger, unpublished paper, "Cold Fusion Theory: A Brief History of Mine" *JSS*, Box 9, Folder 6.

a reaction between a proton and a neutron, yielding ^{3}He and a γ-ray of less energy than in the D-D reaction. In the experiments, however, this γ-ray is not detected. Schwinger's claim: the excess energy of cold fusion is transferred to the palladium lattice in the cathode in the apparatus. The lattice, if structured in a special state of high uniformity, can absorb the energy released in the fusion reactions, and "that energy might initiate a chain reaction as the vibrations of the excited ions bring them into closer proximity. This burst of energy will continue until the increasing number of irregularities in the lattice produce a shut-down."[72] In other words, cold fusion.

Schwinger framed his popular discussions on cold fusion by noting the problematic nature of imposing the situation of hot fusion onto that of cold fusion—something he charged the critics of doing.[73] In hot fusion, the Coulomb repulsion and the nuclear forces can be considered separately; in Schwinger's cold fusion, one cannot treat these two forces as separate entities, but rather as part of a single wavefunction.[74] Schwinger then uses arguments involving the wavefunctions for low energy protons and deutrons to construct a hypothesis for cold fusion he found plausible.

It is the plausibility that Schwinger emphasized, the hypothetical nature of his mechanism. His first journal publication on cold fusion was even titled "Cold fusion: a hypothesis" and he later wrote

> This is a primitive reaction to what may be a very sophisticated mechanism. And do not forget the failure of theory to predict, and then account for the phenomenon of high temperature superconductivity. I advance the idea of the lattice playing a vital role as a *hypothesis*. Past experience dictates that I remind you that a hypothesis is not something to be proved mathematically. Rather it is a basis for correlating data and for proposing new tests, which, by their success or failure, support or discredit the validity of the hypothesis. It is the essence of the scientific method.[75]

as well as

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[72] On page 5 in an lecture titled "A Progress Report: Energy Transfer in Cold Fusion and Sonoluminescence" in *JSS*, Box 8, Folder 6.

[73] See Schwinger's reply to *Physical Review Letters* which criticizes Referee C for his or her "inability to understand that the subject is COLD fusion, not HOT fusion," in *JSS*, Box 9, Folder 6.

[74] See Julian Schwinger, "Cold Fusion—Does it have a Future?" in M. Suzuki and R. Kubo, eds., *Evolutionary Trends in the Physical Sciences* (Berlin: Springer-Verlag, 1991).

[75] Schwinger, "Cold fusion—Does it have a future?" 174. Emphasis his.

> I am well aware of the tentative, provisional, nature of these considerations. But, in contrast with those who would dismiss the very possibility of cold fusion, here, at least, is an opening, a beginning of understanding. With it one may, some day, find the Holy Grail of Cold Fusion, which is accessible only to those of pure spirit. [76]

Who were those pure of spirit? For Schwinger, they were those who approached science in the same fashion that he did: phenomenologically. One tenet of his phenomenology was raising the importance of experiments. From the start, with his letter to the *Los Angeles Times*, Schwinger proposed experiments to test his theory, and he built his theories to explain experimental data. A second tenet was to separate that which is known from that which is speculation. Schwinger made it a point to highlight the tentative nature of his *hypothesis*. The use of speculation was not verboten, as is evidenced by Schwinger's speculation of dyons. However speculation had to remain just that, and not confused with fundamental knowledge. A second and more powerful example of the distinction between the known and unknown is the line that Schwinger drew between hot and cold fusion. Critics, he found, were extrapolating conditions of a higher energy domain into a lower energy domain. That extrapolation necessarily involves making the assumption that nature operates similarly in both regimes. This concern echoes one of Schwinger's critiques of quantum field theory: an operator relies on a large number of matrix elements (of energy and momenta) which lie outside the domain of experimental evidence. "Unavoidably," he claimed, "an operator field theory makes reference to phenomena in experimentally unexplored regions." [77] Schwinger speculated about cold fusion without losing his phenomenological outlook. In truth, it was his phenomenological outlook which provided him justification to even *consider* cold fusion. It was his increasing frustration with a community that could not see eye-to-eye on this matter which dominated his actions during this period.

Similar to the reception of his source theory, the reception of his hypotheses for the mechanism of cold fusion were negative. His first publication, "Cold fusion: A hypothesis" (sent in August 1989) was rejected in October from the prestigious *Physical Review Letters*. All three anonymous reviewers asked for more detail and explanation. One found the submitted article "at best an

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[76] "Dijon lecture" delivered on 2 February 1990, in *JSS*, Box 28, Folder 13.

[77] Julian Schwinger, "A Report on Quantum Electrodynamics" in M. Flato, C. Fronsdal, and K. Milton, eds., *Selected Papers (1937-1976) of Julian Schwinger* (Dordrect: D. Reidel Publishing Company, 1979), 382.

introduction to a hypothesis", another that "it is nearly without substance", and the last "strongly recommend[ed] that this paper should be rejected."[78] I quote at length from Schwinger's reply to the editor of *Physical Review Letters*:

> With one possible exception, the reviewers of my Letter have come close to, but not equaled, to arrogant stupidity of an earlier PR reviewer, who wrote:
>
> "I have not read this paper, but it must be wrong."
>
> What, pray, in my 55 years of not unsuccessful research justified such contempt? I submit that giving anonymity to narrow minded specialists grants them a license to kill.
>
> I want no more of this. Please inform whoever might be interested that I resign as a Member and Fellow of the APS [American Physical Society].
>
> You will, of course, return the copyright agreement that I signed; all rights now revert to me.
>
> Incidentally, the PACS entry (1987) 11.10 Mn can be deleted. There will be no further occasion to use it.
>
> Schwinger.[79]

In this reply, Schwinger was relying on his position in the physics community when discussing the reviewers's treatment (calling upon his "not unsuccessful research"). He took the referee reports personally. By renouncing his membership in the American Physical Society, Schwinger was in essence renouncing its peer review practices. His anger was so great that he felt the need to add to the letter the next day: "It was not my intention to reply to the referees. But the feelings of outrage at injustice did not go away. So, not for you, or them, but for me, as catharsis."[80] Importantly, Schwinger suggested that the third referee, the most damning of the three, be "ejected" because "All you can expect from him/her is the Party line."[81] Unsavory ideas – *hypotheses* – attacked simply because of an expectation to conform was simply unacceptable.

His visceral reaction to *Physical Review Letters* was not only reminiscent of

[78] *JSS*, Box 9, Folder 6.
[79] Julian Schwinger to G. Wells (18 October 1989), *JSS*, Box 9, Folder 6.
[80] Julian Schwinger to G. Wells (18 October 1989), *JSS*, Box 9, Folder 6. To Eugene Mallove, Schwinger wrote that "Although I anticipated rejection I was staggered by the heights (depths?) to which the calumny reached," in Mallove, *Fire from Ice*, 129.
[81] *JSS*, Box 9, Folder 6.

his reaction to his source theory review; Schwinger himself drew a connection when he asked for the Physics and Astronomy Classification Scheme entry 11.10 to be deleted. That was, in fact, his source theory entry in the PACS index.

Schwinger was able to get the rejected paper published in *Zeitschrift für Naturforschung*. However, his publication troubles did not end there. The first of what was to be three papers titled "Nuclear Energy in an Atomic Lattice" (NEAL I) was sent to another German journal, *Zeitschrift für Physik D*, which similarly generated three highly negative reviews. Unlike *Physical Review Letters*, the editor sent Schwinger a letter noting that "Normally I would have to reject the manuscript unless a substantial modification satisfying the referees could be made. However, the present case is very special and you certainly realise the delicacy of the situation."[82] What made the present case special for *Zeitschrift*'s editor was the prestige of the author himself.[83] As a compromise, the editor included the unorthodox disclaimer before the article:

> Reports on cold fusion have stirred up a lot of activity and emotions in the whole scientific community as well as in political and financial circles. Enthusiasm about its potential usefulness was felt but also severe criticism has been raised. If in such a situation one of the pioneers of modern physics starts to attack the problem in a profound theoretical way we feel that it is our duty to give him the opportunity to explain his ideas and to present his case to a broad audience. We do, however, emphasize that we can take no responsibility for the correctness of either the basic assumptions and the validity of the conclusions nor of the details of the calculations. We leave the final judgment to our readers.[84]

The disclaimer was only to be used once. NEAL II and NEAL III were also rejected by *Zeitschrift für Physik D* reviewers – and this time they were not published, with or without disclaimer.[85]

After this episode, Schwinger sent most of his publications to the

[82] *JSS*, Box 28, Folder 22.

[83] Even though the editor of *Physical Review Letters* rejected Schwinger's article, even he made a special effort to explain his actions. In reply to the angry letter that Schwinger dashed off to the *PRL*, the editor himself wrote a special reply explaining in detail the reasons for rejection and noting that "You are a scientist whose contributions to fundamental physics are so important, and whose work I have personally viewed with such admiration, that I especially wanted to try to explain our actions to you, to apologize where appropriate, and, I hope, to convince you to reconsider the drastic actions stated in your letter," in *JSS*, Box 28, Folder 13.

[84] Julian Schwinger, "Nuclear Energy in an Atomic Lattice I," Z. *Phys D* 15 (1990): 221.

[85] Mehra and Milton, *Climbing the Mountain, 552.*

Proceedings of the National Academy of the Sciences.[86] He used the forum in the 1950s when working on symbolic atomic measurement, in the 1970s to expound upon source theory, and in the 1990s to present some extended hypotheses on cold fusion. "The pressure for conformity is enormous," Schwinger remarked in a lecture on cold fusion, "The replacement of impartial reviewing by censorship will be the death of science."[87]

Conclusion

The similarities between Schwinger's work on cold fusion and his work on source theory are apparent. In both he worked against the grain of the mainstream community, received negative reviews, and both illuminate Schwinger's increasing despair with the scientific establishment, in particular, a censoring peer-review system. However these are superficial similarities, and by asking the questions of why and how to each, we can hope to understand Schwinger and the form of his actions better.

A confluence of events opened up a space where Schwinger espoused his more radical ideas in the latter half of his life. With Schwinger's involvement in the Radiation Laboratory during World War II, he was first introduced to an approach to science that would characterize his later work: phenomenology. The crisis in physics in the 1960s, replete with Schwinger's own disgust of the operator field theory's distance to reality, allowed Schwinger to apply the phenomenological approach to particle physics, by making the particle the principle object in the theory. The rising numbers of physics graduate students after World War II provided Schwinger, along with others, the publishing resources needed to codify source theory. The numerous theories put forth by many authors made it possible, but also more difficult, for Schwinger's source theory to become accepted.

Characteristic of the latter half of Julian Schwinger's life is his increasing adherence to a phenomenological outlook. Source theory was the ultimate embodiment of this approach—basing the source concept on an idealized experiment. Countering this conservative approach to physics, however, is a more radical component. Schwinger's physical and intellectual isolation

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[86] Throughout most of his career, Schwinger had used the Proceedings as a forum to "put down in public literature [an idea] but not run through the danger of having to confront a referee. I was sure any referee would say what are you doing this form, this is not publishable. I wanted it recorded somewhere and in those days anybody belonging to the National Academy could submit papers and they would be published. So I made extensive use for a while of that liberty to get across what I had [to say without having] to argue [with] other people's ideas about what should or should not be published." Quoted in Mehra and Milton, *Climbing the Mountain*, 344.
[87] Schwinger, "Cold fusion—Does it have a future?" 175.

from his colleagues resulting from a stubborn refusal to be dominated bred an independence that allowed his departure from the mainstream and stick "stubbornly" to his ideas. Perhaps this was also due to the hope of the eventual vindication of these theories, like the papers he discussed in the lecture "Conflicts in Physics." In addition, Schwinger was an advocate of the use of *speculation* (as long as it was kept distinct from theories) as a means to generate possible theories. Schwinger's vision of science embraced about openness and competition, and a multiplicity of ideas. Schwinger's hypothesis for the mechanism of cold fusion illuminates this belief. His belief in the power of speculation alongside his refusal to be dominated forms the crux of the more radical portion of Schwinger's philosophy. The conservative and radical components of Schwinger's philosophy of science work in concert with each other, yielding innovation by allowing for novel hypotheses to be considered outside of the mainstream, but grounding innovation to experiment with phenomenology.

These two episodes do more than reveal the deep-seeded commitment that Schwinger had to his brand of phenomenology. They also reminds us that science is not, as Schwinger noted, a set of Easter eggs to be found and picked up. Rather it is defined by a collection people; science is a human endeavor. The anger, frustration, and elation that accompany scientific work are as important to the practice of science as philosophy; they shape the individual who shapes science. The negative reaction to source theory assisted in forging a man of strong conviction who worked outside of the mainstream. This status afforded Schwinger a critical eye through which to see the functioning of science. He became a man who questioned how science did operate and a man who decided how it should operate. Through his engagement with cold fusion, he showed these beliefs in action.

Perhaps in a bitter-sweet epilogue, Schwinger's belief in the eventual vindication of an unpopular theory has come at least partially true. Source theory, though not picked up in its original form, eventually struck a chord with Stephen Weinberg in his work pioneering effective field theory, a program which has been popular for the past two decades.[88] Philosopher Tian Yu Cao has noted the similarities between Weinberg and others' works on effective field theory (EFT) with source theory: "first, their denial of being fundamental theories; second, their flexibility in being able to incorporate new particles and new interactions into existing schemes; and third, the capacity of each of them to consider non-renormalizable interactions."[89] EFTs are strongly

[88] Stephen Hartmann, "Effective Field Theories, Reductionism and Scientific Explanation," *Studies in the History and Philosophy of Modern Physics* 32, no. 2 (2001): 267-304.

[89] Cao, *Conceptual Developments*, 351 and more generally section 11.4.

phenomenological, and pliable in the sense that they can deal with various energy regimes. However EFTs, like QFTs, do break from Schwinger's phenomenology in one significant way: they are an operator field theory, which means that they have to deal with arbitrarily high energies. So even though EFTs can describe phenomena at different energy ranges, they have to take into account contributions from unexplored higher energy processes. But even though his vindication was partial, his legacy is maybe even greater. Source theory has disappeared, but for many, his phenomenology remains.

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