Abstract: The influential theoretical physicist Julian Schwinger renounced operator field theory – a theory he assisted in developing in the 1940s – and S-matrix theory on philosophical grounds. He felt that the dominant theories made unwarranted assumptions about the nature of reality, and would hinder the development of particle physics. In 1966 he proposed an alternative, source theory, which he believed overcame the philosophical problems. Although this theory was rejected by the physics community, Schwinger held to it steadfast until his death. In 1989, Schwinger embarked on another endeavor which was to be disowned by the physics community at large: he advocated further research in cold fusion. Schwinger’s physical and intellectual isolation – partly reflecting his developing philosophy of science – coupled with his rising position in physics, created a space for him to work against the mainstream in the 1960s, when there was a crisis in the high energy physics community. This rebelliousness – spurned on by his philosophy and reception to his source theory concept – continued until his 1994 death. These two episodes also reveal both a conservative and radical strain to how Schwinger felt science should operate.

In the mid 1970s, the widespread acceptance of quantum field theory by the particle physics community ushered 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 frustration, another prominent theorist put forth a third alternative: source theory.

In 1966 Julian Schwinger published a short paper in the Physical Review titled “Particles and Sources.” This paper provided an introduction to a program that was to crystallize his personal philosophical outlook of theoretical physics (and, more generally, science). He staunchly adhered to this philosophy until his death in 1994. Unlike many of

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his other articles, this one—as was his entire source theory program—was lost to the blackboxing annals of science. Perhaps more significantly, the Schwinger who advocated source theory, in many ways distinct from the Schwinger pre-source theory, was lost in the annals of history. Mention of Schwinger post-1966 is scarce in the historical literature. One goal of this paper is to bring the latter years of Schwinger’s life back into focus, by understanding the philosophy undergirding source theory, the way that Schwinger argued for source theory, and the situation in particle physics during the 1960s which allowed for Schwinger to put forth a new theory.

A story similar to source theory can be told with Schwinger’s involvement with cold fusion late in his life. Cold fusion was at the forefront of Schwinger’s thoughts. He wrote journal articles, collected hundreds of newspaper and article clippings, and even terminated his membership to the American Physical Society, all 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 the source theory episode reveals deep philosophical commitments in Schwinger’s scientific worldview, so can the cold fusion episode. A second goal of this paper is to understand how Schwinger’s hypothesis of cold fusion fits into his view of how science should be practiced.

Read in concert with each other, source theory and cold fusion provide lenses with which to investigate the life of an increasingly isolated hero of quantum electrodynamics. They show how a scientific community can pit a person against the mainstream scientific currents of the day. Battles in science happen every day. The

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Schwinger story does not end in vindication (at least not yet), of a David improbably defeating a Goliath.

Section 1: Source Theory

On 11 December 1965, Julian Schwinger lectured to a filled auditorium in Stockholm on the history of relativistic quantum field theory. In this, his Nobel lecture, Schwinger concluded with an optimistic statement for the future of quantum field theory – expressing his belief that *phenomenological* relativistic quantum field theory would be the path that will lead to a bright future.\(^3\) Schwinger was to rapidly develop this program that he had been toying with in the coming years. However instead of being the path to the future, the theory he dubbed “source theory” was to become the path not taken.

Source theory can be read as Schwinger’s rejection to his own previous work in physics – work which led him to that very stage in Stockholm. But even though source theory never was to take off in the way that its creator had hoped, it does not mean that it is not worth examining. First, and most importantly, even though source theory never became an important theory in the larger community, it was an important theory for Schwinger. So important indeed that even until his death, Schwinger never relinquished the theory, like he had done operator field theory. Source theory provides a window to Schwinger’s changing philosophy of physics, and to Schwinger himself. Why might a physicist abandon a theory which had pioneered and which had given him respect and authority? What was it about this particular historical moment that made this abandonment necessary for Schwinger? What might Schwinger’s actions during this tumultuous episode speak about Schwinger as a person? And how did Schwinger attempt

to spread his theory? Second, from a methodological perspective, to ignore all the theories that do not become dominant in a science is tantamount to writing a history of the victors – a purely teleological account. Teleology is an intractable problem for historians of science, who must necessarily be concerned with the question of how the present state of science came about. We only have one outcome (present science) and some teleology is inevitable. But it does not mean that one can forget the tales of the others. Knowing what ideas did not get picked up can sometimes illuminate how other did. 

Lastly, Schwinger himself reminds us of another reason that forgotten theories deserve to be studied. As we shall see, he clung to the belief that a theory unpopular in a particular scientific climate can eventually emerge to become dominant. Discarded theories can be “rediscovered.”

Discontent

The successful renormalization of quantum electrodynamics caused jubilation among the participants of the physics community. Previously the particle physics community was plagued with the problem of “infinities” which arose when calculating the electromagnetic mass of an electron. Experimental evidence made the problem palpable. Willis Lamb, using surplus World War II equipment, measured a shift in the energy levels of hydrogen atoms in the 2s and 2p states. Classically, the energy of the two states should have been equal; instead he measured a slight but significant difference. Schwinger developed a theory which explained this difference, by using a perturbation technique. Freeman Dyson described the initial enthusiasm which characterized those

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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 1.5

Schwinger’s old mentor I.I. Rabi wrote Hans Bethe, upon hearing of the close matching of experimental results to Schwinger’s theoretical predictions, “God is great”; Bethe responded “It is as exciting as in the early days of quantum mechanics."6 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.

However this enthusiasm was not shared by everyone, nor would it last. During the mid-1950s there was – in the words of Steven Weinberg – a “collapse in confidence.” Some prominent physicists felt that renormalization was a mere mathematical trick, an ugly procedure, and philosophically suspect. More importantly to researchers at the time, programs to extend quantum field theory (henceforth, QFT) to two additional fundamental forces, the strong and weak, were met with great difficulty. By 1960, historian Helge Kragh notes that faith in QFT was at a nadir.7 The popular alternative

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7 For a description of the state of crisis that was felt in physics during this time period, see 336-339 in Helge Kragh, Quantum generations: A history of physics in the twentieth century (Princeton: Princeton University Press, 1999).
formulation to QFT was $S$-matrix theory—and updated version of the original scattering matrix theory developed by Werner Heisenberg in 1925.\textsuperscript{8}

Schwinger was not unaware of this division among his colleagues. He queried in the early 1960s:

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.\textsuperscript{9}

The passage illustrates that at this time, even 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 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 problems of its own rather than being the best way of representing the field situation.\textsuperscript{10}

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

\textit{Phenomenological Framing of Source Theory}

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 QFT) and $S$-

\textsuperscript{8} For the view of how QFT and $S$-matrix theory were viewed during this period, see chapter 7 in James T. Cushing, \textit{Theory construction and selection in modern physics: The $S$ matrix} (Cambridge: Cambridge University Press, 1990), especially pgs. 169-173.


matrix theory.\textsuperscript{11} If the physics community at large had ascribed to a single theory—and there was no “crisis”—it would have proved more difficult for Schwinger to put forth his alternative. Considering that source theory relied mainly on philosophical differences, rather than being able to explain phenomena that were previously unexplainable, it would be even more unlikely. Schwinger could put forth source theory with a realistic hope for acceptance because of the “collapse in confidence.” In his textbook – to become the first in a series of three – Schwinger positioned source theory in between the prominent two theories.\textsuperscript{12} Those who subscribed to the source theory program would gain all the advantages of a simpler theory with none of the drawbacks of the philosophically unsound alternatives.

Operator field theory, for Schwinger, was highly problematic. The particle had been relegated to a “stable or quasi-stable excitation of the fields.”\textsuperscript{13} In other words, the particle took second seat to the operator fields. Experimental physicists, however, connected to the particles themselves, not the fundamental fields which “generated” them. In addition, the theory presupposed its correctness at distances smaller than had been experimentally tested.\textsuperscript{14} Furthermore, operator field theorists made an additional assumption regarding the interaction of three (or two) fundamental fields to make a baryon or meson. “In other words,” Schwinger declares, “I make here the rather serious

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\item\textsuperscript{11} 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. I do not use the term “crisis” in the Kuhnian sense. Rather, I use it in a loose sense to indicate a loss of confidence prompting the development of numerous theories.
\item\textsuperscript{12} Julian Schwinger, \textit{Particles, sources, and fields, volume I} (Reading, MA: Addison-Wesley Publishing Company, 1970).
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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.\textsuperscript{15}

\textit{S}-matrix theory held different, but just as significant, problems for Schwinger. At
the heart, \textit{S}-matrix theory placed the \textit{S}-matrix itself as fundamental. One could only,
Schwinger spurned, “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.”\textsuperscript{16} It
relinquished the QFT’s space-time description for a momentum space description. And
because the \textit{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 \textit{S}-matrix (where all particles generate all other particles), it \textit{precluded} the
possibility that there is a deeper level to understanding of particles. This unwarranted
assumption – that particles do not have an internal structure – cuts off the “openness” of
scientific investigation. Though he acknowledged both QFT and \textit{S}-matrix theory could
prove correct (though probably not), he did not want to have these possibly flawed
assumptions directing future research.\textsuperscript{17}

The means for bypassing the problems underlying the dominant theories was to
turn 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 of 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 number of new particles including the $\eta$, the $\omega$, the
$\Xi^0$, the $Y_1^+(1385)$, the $K^*(890)$, the $Y^{*0}(1405)$, the $\Xi^*(1530)$, and the $\Omega^*(1672)$.\footnote{This list of particle was taken from Peter Galison, \textit{Image and logic: A material culture of microphysics} (Chicago: The University of Chicago Press, 1997), 319. The subscripts give the particle’s isospin, the asterisk shows the particle is in an excited state, and the parenthetical number is the mass in MeV. See also Andrew Pickering, \textit{Constructing quarks: A sociological history of particle physics} (Chicago: The University of Chicago Press, 1984), 48-50.} 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.\footnote{Peter Galison, \textit{Image and logic: A material culture of microphysics} (Chicago: The University of Chicago Press, 1997), 370.} How to unify
this giant mass of data with theory became a fundamental concern for physicists.\footnote{Peter Galison raises the question of how theorists and experimentalists communicate – and how “phenomenological” physicists can act as mediators between the two. In Peter Galison, \textit{Image and logic: A material culture of microphysics} (Chicago: The University of Chicago Press, 1997): 641-668. I do not focus on this issue in this paper, though seeing if Schwinger is part of a “trading zone” would make an interesting topic of further research.}

The preface to a conference proceedings on phenomenology at Caltech declared
the goal of phenomenological work was to close the gap between theory and experiment.
To do this, the phenomenologist must construct a simple model to embody important
theoretical ideas, and then compare the model with relevant data. This process, 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 can
find experimental anomalies that the theories could not explain, thus leading to new areas
of fruitful research.\footnote{C.B. Chiu, G.C. Fox, and A.J.G. Hey, eds., \textit{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} To illustrate, the editors drew the following “Feynman diagram”:

[insert “Feynman diagram” representation]
The physics community loosely ascribed the adjective “phenomenological” to work which did some comparison work between theory and experiments. Phenomenologists merely noted the “widening wedge between theories and experiment” and hoped to overcome that.\textsuperscript{22}

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.\textsuperscript{23}

His phenomenology 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.”\textsuperscript{24}

The method of Schwinger’s phenomenological approach is to begin with a simple phenomenon and create a phenomenological theory. Then one must extrapolate outside the domain and await experimental confirmation. Experimentation was integral to this program – all hypotheses (extrapolations) needed to be tested. Instead of being a “trickle down” theory, assuming the correctness of a grand superstructure, this program advocated building up.

More than just guiding the researcher to better theories or interesting experiments, Schwinger believed that phenomenological work was crucial because it separated

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speculation from theory. His work on source theory highlighted the fact that operator field theory and S-matrix theory were based on unacknowledged assumptions.

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 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.\textsuperscript{25}

With the assistance of his phenomenological program, his theory will never preclude a future theory because of assumptions. And though Schwinger does acknowledge the use of speculation in physics, he makes it a point to keep speculations as speculations, and nothing more. With his version of phenomenology, Schwinger practiced a conservative form of physics.\textsuperscript{26}

\begin{quote}
What are sources and how do they encapsulate Schwinger’s philosophy? Sources are operationally defined via the situations they are studied—experimental collisions.\textsuperscript{27}
\end{quote}

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\textsuperscript{25} Julian Schwinger, \textit{Particles, sources, and fields, volume I} (Reading, MA: Addison-Wesley Publishing Company, 1970), 37
\textsuperscript{26} I am 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” on 252, in Silvan S. Schweber, \textit{QED and the men who made it: Dyson, Feynman, Schwinger, and Tomonaga} (Princeton: Princeton University Press, 1994).
\textsuperscript{27} 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” on 229 in \textit{Introduction and selected topics in source theory} (Braunschweig: Friedr. Vieweg & Sohn, 1977). 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” in Paul C. Martin, “Julian Schwinger—Personal Recollections” in Y. Jack Ng, \textit{Julian Schwinger: The physicist, the teacher, and the man} (Singapore: World Scientific, 1996), 88.
\end{flushright}
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.”

A source $S$ is used to represent a collision where particles are created. If $S$ is large, the probability that a particle is created is large, and vice versa. The source, when quantified, becomes an idealization of the experimental procedure used to produce a particle.

Textbook

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.” 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 formulation which was mathematically distinct from the two formulations which were commonly used and understood, but it also provided a way to enroll younger physicists into a particular methodological and philosophical program. Schwinger’s strong personal convictions for the future of source theory, and the inadequacy of the existing models, were revealed by the language used in the preface: he refused to give a historical account – providing priority to ideas and techniques – because “it would have been too distracting if constant

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29 Julian Schwinger, *Particles, sources, and fields, volume I* (Reading, MA: Addison-Wesley Publishing Company, 1970), iii. This belief was not unique to Schwinger. Geoffrey Chew, the innovator of “nuclear democracy,” notified readers of his published lecture notes that “it is . . . unnecessary to be conversant with the subtleties of field theory, and a certain innocence in this respect is perhaps even desirable. Experts in field theory seem to find current trends in $S$-matrix research more baffling than do nonexperts.” Quoted on 256 in David Kaiser, “Nuclear democracy: Political engagement, pedagogical reform, and particle physics in postwar America,” *Isis* 93 (2002): 229-268.

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reference to *techniques for which obsolescence is intended* had accompanied the development of the new approach.”\(^{30}\)

In fact, spreading the gospel of the novel source theory was so important that in his first letter querying publisher Addison-Wesley’s interest in becoming his publisher, Schwinger made four demands: “all possible speed in publication,” “freedom from arbitrary editorial interference,” “widespread adverising,” and “low price.” This book was his *piece de resistance*, his “highly personal statement” on particle physics. But more importantly, in this letter Schwinger highlighted 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.\(^{31}\)

During the period of the 1950s and 1960s, textbook publishers were publishing an increasing number of textbooks and lectures, with the demand generated by the rise in graduate-level enrollments in physics. 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 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.”\(^{32}\) 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

\(^{31}\) Julian Schwinger to Melbourne W. Cummings, 26 November 1969, in Collection 371, Box 17, Folder 1. This was not an uncommon practice at this time.
“techniques.”\textsuperscript{33} Many of those publishing were staking their claim in the future of physics. Again we can see how the situation in the post-war physics community created an opening for the publication of new textbooks. Not only were publishers approaching physicists to write textbooks, but the rise in new theories allowed those with different views to express them.

Rhetorically, \textit{Particles, Sources, and Fields} was not a standard textbook. As we have seen, Schwinger demanded that it be free from arbitrary editorial influence. What, then, did his stylistic conventions convey? The answer becomes clear from the epigraph: “If you can’t join ‘em, beat ‘em”. His book is an offensive posturing against the scientific establishment. Through the economy of language, these few words reveal a position that Schwinger was to abide by for the much of the rest of his career. Schwinger was not “making nice” nor backing down. Instead, he was setting himself up for conflict. This was not a text written with the intent to go over well with the larger scientific establishment; indeed both operator field theorists and $S$-matrix theorists were deemed part of the problem. The advocates of these theories were “warping” uninitiated student minds beyond repair. This book was designed to save them.

\textit{Origin of Source Theory and Isolation}

Though Schwinger was discontent with the complexities of operator field theory, source theory did not have to necessarily result. We must wonder, then, what experiences and resources was Schwinger drawing on which led to his phenomenological stance. At least some of the origins of this outlook can be traced to Schwinger’s war work.\textsuperscript{34} In

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\item \textsuperscript{33} See chapter 7 in David Kaiser, \textit{Drawing theories apart: The dispersion of Feynman diagrams in postwar physics} (unpublished).
\item \textsuperscript{34} Julian Schwinger, “Julian Schwinger’s approach to particle theory,” \textit{Scientific research} \textbf{4} (17) (18 August 1969), 19.
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1942, while a professor at Purdue University, Schwinger was recruited to the MIT Radiation Laboratory that summer by none other than physics luminary Hans Bethe.

The group Schwinger headed was charged with developing a usable account of microwave networks. Because existing methods were rendered useless due to the high frequency of microwaves, Schwinger had to start his work from the basic and fundamental Maxwell equations – which he was dismayed to realize contained unnecessary information. “As far as any particular problem is concerned,” he later wrote, “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.”\(^{35}\) Rather than working from abstract theory, Schwinger began to use practical representations, simple circuits, that 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 made some important observations from this period. Schwinger was placed in a location where he assimilated some of the input-output engineering culture. Here it must be noted the similarity between the input-output engineering model and source theory. A source is an idealization which represents the creation of a particle—which is characterized “through the net balance between what enters and what leaves the collision.”\(^{36}\) Galison points out 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.\textsuperscript{37} This was necessary for Schwinger to gain hands-on experience connecting data to theory.

He credited his later famed 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.”\textsuperscript{38} But importantly, when he denounced renormalization and operator field theory, Schwinger then used this 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 \textit{not} do is to try to begin with some fundamental theory and calculate. As we saw, this is not the best thing to do even when you have a fundamental theory \textit{[i.e. like Maxwell’s equations in the Rad Lab]}, and if you don’t have one \textit{[i.e. like in high energy physics]}, it’s certainly the wrong thing to do.\textsuperscript{39}

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

While at the Rad Lab, Schwinger’s isolationist tendencies revealed themselves. Though he was around the Rad Lab, he worked mainly with a select few collaborators, like Harold Levine and Nathan Marcuvitz, while 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. Once, while solving a problem left for him by A.J.F. Siegert, Schwinger solved a problem incorrectly – by accidentally using a definite integral of a Bessel function, copied from a reference book, rather than an

\textsuperscript{37} See Peter Galison, \textit{Image and logic: A material culture of microphysics} (Chicago: The University of Chicago Press, 1997), 820-828
\textsuperscript{38} Quoted in Peter Galison, \textit{Image and logic: A material culture of microphysics} (Chicago: The University of Chicago Press, 1997), 826.
indefinite integral. When he saw this error, he decided never to copy formulas from books, but rather derive them from first principles.\(^{40}\) This is unsurprising, because throughout his entire career, Schwinger often would start a problem from scratch, without first seeing how others had solved it. 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 being dominated. 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.”\(^ {41}\) He said the same thing about Oppenheimer when he went to Berkeley to work.\(^ {42}\) After he got married in 1947, after the war had ended, people had 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.”\(^ {43}\)

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


Reception

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; *Particles, Sources, and Fields* was written to remedy the situation. It concluded 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.

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 was anathema to using diagrams of any sort in his book. He instead preferred formalism expressed through series of equations.

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46 Schwinger and Richard Feynman both proposed different methods to renormalize QED. Schwinger’s was highly formalistic, while Feynman’s made extensive use of intuition and diagrams. Schwinger found the diagrams unacceptable because they obscured the fundamental calculations. This would hinder the development of the theory. (Freeman Dyson showed the two formulations were mathematically equivalent, but Schwinger was not concerned with the *correctness* of Feynman’s methods as much as their conceptual
Moreover, this book advocated a complete rejection of operator field theory and S-matrix theory, both theories that, by this time, physicists had become familiar navigating. Instead, he was proposing that readers accept not only the philosophical basis undergirding his theory and a new framework for working problems, but also requiring that readers learn to use unfamiliar computational tools.\textsuperscript{47} The reading of Schwinger’s early work in source theory was not consistent with the way Schwinger envisioned it. A peer reviewer for \textit{Physics Review Letters} rejected one of Schwinger’s early papers partly because he saw the work as using the Lagrangian “in lowest order only.” In the traditional operator field theory paradigm, Lagrangians were \textit{operators}, thus having higher order corrections. But Schwinger’s source theory fundamentally rejected the use of operators, and instead used the Lagrangian as a \textit{function}.\textsuperscript{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”.\textsuperscript{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


\textsuperscript{48} Julian Schwinger to George L. Trigg, editor of Physical Review Letters (March 1967), UCLA Special Collections (Collection 371, Box 17, Folder 1).

evidence offered for computational power of the source method is not convincing.”\textsuperscript{50} The costs did not outweigh the benefits. This was true for more than a single reviewer; two of Schwinger’s former students characterized the reaction to source theory as being “nearly universally negative.”\textsuperscript{51}

*Sticking with it*

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. As one of his former students 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.”\textsuperscript{52} Schwinger continued working on source theory for years after. 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.\textsuperscript{53} These particles had not been observed; there 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—by assuming the existence of a substructure to which there was no direct experimental


\textsuperscript{51} Jagdish Mehra and Kimball A. Milton, *Climbing the mountain: The scientific biography of Julian Schwinger* (Oxford: Oxford University Press, 2000), 481. It is interesting to note, in this period of tumult in the physics community, that Arthur Wightman was working on his own response—axiomatic field theory. Schwinger found this approach too constructive: “I regard it as a mistake to try to axiomatize—You have said no new phenomena will ever be found that lie outside this framework and that struck me as a quite absurd approach to what is obviously an open universe with new things to be found.” On 455 in Jagdish Mehra and Kimball A. Milton, *Climbing the mountain: The scientific biography of Julian Schwinger* (Oxford: Oxford University Press, 2000).

\textsuperscript{52} Y. Jack Ng, “Schwinging a sorcerer’s wand: Julian and I,” in Y. Jack Ng, *Julian Schwinger: The physicist, the teacher, and the man* (Singapore: World Scientific, 1996), 120.

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

The separation of theory and speculation important in Schwinger’s phenomenology. However, as the Faraday quotation reveals, speculation can be useful too. It can lead to experimental tests and perhaps to new theories. As we shall see, Schwinger would come to embrace another particular speculation—a mechanism for cold fusion.

**Section 2: Conflicts in Physics**

In 1977, Schwinger presented a lecture tracing the convoluted history of the kinetic theory of matter – tracing the topic from its Greek origins to Einstein. Titled “Conflicts in Physics,” this work presents a view of science which historians of science embrace – one where impartial, passive scientists uncover truths about nature. Rather it reveals a belief that science is a process involving fallible human nature, a strong conformist culture, and vicious competition. The lecture on the history of science does not only tell us about Schwinger’s view of how science had operated in the days of Boltzmann and Lord Rayleigh, but it is a barely-disguised indictment of how science

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55 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,” on xi in Julian Schwinger, *Einstein’s legacy: The unity of space and time* (New York: Scientific American Books, Inc, 1986). Within this book is a section titled “The Conflict” about the conflicting natures of Newtonian mechanics and Maxwellian electrodynamics, highlighting the importance of conflict for Schwinger.
operated in 1977. It provides a glimpse into what Schwinger saw “wrong” about the scientific community he operated within, but 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. Besides simply denouncing the concealing of disputes within science, the letter noted that this behavior is harmful 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.\(^56\)

At the heart of it, for Schwinger, science *is* about finding a proper 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 which had been ignored by their contemporaries, works which 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 it was not published by the Royal Society. 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.” Speculation, Schwinger argues, is good for science, as it can lead the way to fundamental theories. The second paper was sent to the Royal Society by John James Waterson in 1845—and not published at that time. Contained within was a direct connection between temperature and energy. It was not until 1892, when Lord Rayleigh found and published the paper, with an apology, one which Schwinger identified with:

“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.

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.”

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.

Schwinger cited Boltzmann exclaiming, “I am conscious of being only an individual

57 “Conflicts in Physics”, 4, in UCLA Special Collections, Collection 371, Box 28, Folder 14.
58 “Conflicts in Physics”, 8, in UCLA Special Collections, Collection 371, Box 28, Folder 14.
59 “Conflicts in Physics”, 9, in UCLA Special Collections, Collection 371, Box 28, Folder 14.
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.” Threaded throughout this text is a sense that unpopular ideas can eventually
vindicate themselves, become 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.”

A sense of despair for the direction of physics comes through in this passage. Perhaps it
is his “secret fear” which motivated Schwinger’s interest to engage with ideas unpopular
with the mainstream, ideas such as cold fusion.

Section 3: Cold Fusion

Julian Schwinger inserted himself into the discussion on cold fusion from the very
early days of interest. His decision to pick up the study of cold fusion was in part due to

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60 “Conflicts in Physics”, 12, in UCLA Special Collections, Collection 371, 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.” On 456 in Jagdish Mehra and Kimball A. Milton, Climbing the mountain: The
61 “Conflicts in Physics”, 18-19, in UCLA Special Collections, Collection 371, Box 28, Folder 14.
his scientific curiosity – how cold fusion might occur – but as time went on, his fundamental conviction that science 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 belief in phenomenology tempered his own scientific work on cold fusion, allowing him to use experimental evidence to point to a potential for cold fusion, and form hypotheses to explain the mechanism by which cold fusion operated.

Schwinger’s involvement with 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.62

One of these scientists was Julian Schwinger. Printed on 1 May 1989, a couple of weeks after being written, Schwinger wrote a letter to the editor of the Los Angeles Times outlining a potential explanation for cold fusion—and a simple experiment to test it.63 He


63 Julian Schwinger, “Table Top Fusion” Los Angeles times (1 May 1989). The handwritten version – slightly different – is at UCLA Special Collections, Collection 371, 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. On 81 in Eugene F. Mallove, Fire from ice: Searching for the truth behind the cold fusion furor (New York: John Wiley and Sons, Inc., 1991).
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. What we can glean from this article is that Schwinger’s initial interest in cold fusion was compelling enough to have him write the Los Angeles Times.

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. 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 [find citation in Mehra and Milton]. 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

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64 Harry Collins and Trevor Pinch, The golem: What you should know about science (Cambridge: Cambridge University Press, 1998 [1993]): 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.
65 UCLA Special Collections, Collection 371, Box 4.
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.\textsuperscript{67}

\textit{Phenomenology and Cold Fusion: Hypothesis}

Though never staking a claim for or against the actual \textit{reality} of cold fusion, Schwinger concerned himself with finding a plausible mechanism to explain the experimental data that had been generated.\textsuperscript{68} “Ordinary” fusion reactions with heavy water (D-D reactions) yield neutrons, energy in the form of a $\gamma$-ray, $^3\text{He}$, and $^4\text{He}$. Critics of cold fusion noted that experiments did not yielding 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 a reaction between a proton and a neutron, yielding $^3\text{He}$ and a $\gamma$-ray of less energy than in the D-D reaction. In the experiments, this $\gamma$-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

\textsuperscript{66} UCLA Special Collections, Collection 371, Box 4, Folder 10.
\textsuperscript{67} 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 (3) (August 1995): 403-436.
\textsuperscript{68} He began an unpublished paper titled “Cold fusion theory: A brief history of mine” with “As Polonious might have said: ‘Neither a true-believer nor a disbeliever be.’ From the very beginning in a radio broadcast on the evening of March 23, 1989, I have asked myself not whether Pons and Fleischman are right—but whether a mechanism can be identified that will produce nuclear energy.” Found in UCLA Special Collections, Collection 371, Box 9, Folder 6.
closer proximity. This burst of energy will continue until the increasing number of irregularities in the lattice produce a shut-down.\textsuperscript{69}

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.\textsuperscript{70} 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.\textsuperscript{71}

Schwinger then uses arguments involving the wavefunctions for low energy protons and neutrons 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 was later to write

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 \textit{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.\textsuperscript{72}

as well as

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.\textsuperscript{73}

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\textsuperscript{69} On 5 in an lecture titled “A progress report: Energy transfer in cold fusion and sonoluminescence” in UCLA Special Collections, Collection 371, Box 8, Folder 6.

\textsuperscript{70} The first reference I found to this distinction is in Schwinger’s reply to \textit{PRL} which criticizes Referee C for his or her “inability to understand that the subject is COLD fusion, not HOT fusion” (UCLA Special Collections, Collection 371, Box 9, Folder 6).

\textsuperscript{71} See Julian Schwinger, “Cold fusion—Does it have a future?” in M. Suzuki and R. Kubo, eds., \textit{Evolutionary trends in the physical sciences} (Berlin: Springer-Verlag, 1991).


\textsuperscript{73} “Dijon lecture” delivered on 2 February 1990 in UCLA Special Collections, Collection 371, Box 28, Folder 13.
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 evidence 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 critique 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.”\(^74\) 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.

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Problems with Reviewers

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 introduction to a hypothesis”, another that “it is nearly without substance”, and the last “strongly recommend[ed] that this paper should be rejected”.75 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.76

In this reply, Schwinger was relying on his position in the physics community (though downplaying it immensely) 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

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75 UCLA Special Collections, Collection 371, Box 9, Folder 6.
76 Julian Schwinger to G. Wells (18 October 1989), UCLA Special Collections, Collection 371, Box 9, Folder 6.
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.”

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.” 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 his reaction to his source theory review; Schwinger himself drew a connection when he asked for the Physics and Astronomy Classification Scheme (PACS) 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 I” (NEAL I) was sent to another German journal, *Zeitschrift für Physik D*, which similarly generated three highly negative reviews. Unlike *Physics 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.”

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77 Julian Schwinger to G. Wells (18 October 1989), UCLA Special Collections, Collection 371, 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 Eugene F. Mallove, *Fire from ice: Searching for the truth behind the cold fusion furor* (New York: John Wiley and Sons, Inc., 1991), 129.

78 UCLA Special Collections, Collection 371, Box 9, Folder 6.

79 UCLA Special Collections, Collection 371, Box 28, Folder 22.
As a compromise, the editor included the highly 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.\(^{81}\)

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.\(^{82}\)

After this episode, Schwinger sent most of his publications to the Proceedings of the National Academy of the Sciences. In fact, 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

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\(^{80}\) 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 UCLA Special Collections, Collection 371, Box 28, Folder 13).


\(^{82}\) One reviewer said in reference to NEAL III, “in all due respect to Prof. Schwinger, I do not trust the physical implications of this paper. The mathematics seems alright, but the physical picture is fantastic. No convincing conclusions can be drawn regarding the necessity and particular role of the Pd-lattice... it seems highly improbable that the results presented in this paper are correct.” In UCLA Special Collections, Collection 371, Box 28, Folder 22. Mehra and Milton note another undated letter to an editor in the Schwinger archives, reading: “I am getting too old to put up with nonsensical flack from wet behind the ears referees who know and admit nothing beyond the latest bandwagon. I am well aware that charges of discrimination and prejudice also come from the lunatic fringe. It is up to you to have the common sense to know the difference,” on 552 in Mehra and Kimball A. Milton, Climbing the mountain: The scientific biography of Julian Schwinger (Oxford: Oxford University Press, 2000). Littered throughout his writings on cold fusion are comments about critics.
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.\textsuperscript{83}

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.”\textsuperscript{84}

Conclusion

The similarities between Schwinger’s work on cold fusion and his work on source theory are apparent. Both worked against the grain of the mainstream community. Both resulted in him getting negative reviews. Both illuminate his increasing despair with the scientific establishment, with 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 better.

A confluence of events opened up a space where Schwinger could espouse 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 for 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

\textsuperscript{83} Quoted on 344 in Mehra and Kimball A. Milton, \textit{Climbing the mountain: The scientific biography of Julian Schwinger} (Oxford: Oxford University Press, 2000).

\textsuperscript{84} On 175 in Julian Schwinger, “Cold fusion—Does it have a future?” in M. Zuzuki and R. Kubo, eds., \textit{Evolutionary trends in the physical sciences} (Berlin: Springer-Verlag, 1991).
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.

The latter half of Julian Schwinger’s life is characterized by an increasing adherence to a phenomenological outlook. Source theory was the ultimate embodiment of this approach—basing the source concept on an idealized experiment. This conservative approach to physics, however, is countered by a more radical component. Schwinger’s physical isolation from his colleagues and his intellectual isolation—both resulting from a stubborn refusal to be dominated—bred an independence which allowed for him to depart from the mainstream and stick “stubbornly” to his ideas. Perhaps this was also due to the thought that these theories would eventually be vindicated, 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. Science should be about openness and competition, about 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 form the crux of the more radical portion of Schwinger’s philosophy. The conservative and radical components of Schwinger’s philosophy of science do not work in opposition to each other. Rather, they work in concert with each other, yielding innovation (by allowing for novel hypotheses to be considered outside of the mainstream), but grounding it to experiment (with phenomenology). To understand Schwinger as purely a phenomenologist is missing a crucial half of his philosophy.

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85 The idea of science operating as a tension between conserving forces and opposing forces is not new to the history of science. Michael Polanyi espoused a similar viewpoint, noting that any scientific idea is
judged on plausibility, scientific value, and originality, where the first two are conservative forces—
working within the existing paradigm—and the third works against it. In chapter 4 of Marjorie Grene, ed.,