Studies in History and Philosophy of Modern Physics 50 (2015) 70–94

Contents lists available at ScienceDirect

Studies in History and Philosophy of Modern Physics

journal homepage: www.elsevier.com/locate/shpsb

QED and the man who didn't make it: Sidney Dancoff and the infrared divergence

Alexander S. Blum

Max Planck Institute for the History of Science, Boltzmannstraße 22, 14195 Berlin, Germany

ARTICLE INFO

Article history:
Received 19 September 2014
Received in revised form
12 February 2015
Accepted 25 March 2015
Available online 24 April 2015

Keywords:
Quantum field theory
Dancoff, S.
Renormalization
Quantum electrodynamics
Scattering
Infrared divergence

ABSTRACT

Sidney Dancoff's paper "On Radiative Corrections for Electron Scattering" is generally viewed in the secondary literature as a failed attempt to develop renormalized quantum electrodynamics (QED) a decade early, an attempt that failed because of a mistake that Dancoff made. I will discuss Dancoff's mistake and try to reconstruct why it occurred, by relating it to the usual practices of the quantum field theory of his time. I will also argue against the view that Dancoff was on the verge of developing renormalized QED and will highlight the conceptual divides that separate Dancoff's work from the QED of the late 1940s. I will finally discuss how the established view of Dancoff's paper came to be and how the reading of this specific anecdote relates to more general assessments of the conceptual advances of the late 1940s (covariant techniques, renormalization), in particular to their assessment as being conservative rather than revolutionary.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Modern Physics*

1. Introduction

Sidney Dancoff's 1939 paper "On Radiative Corrections for Electron Scattering" (Dancoff, 1939) is famous for being wrong. And not just simply wrong, but history-changing wrong. To understand this, one needs to know that most of the secondary literature on the quantum electrodynamics (QED) and quantum field theory (QFT) of the 1930s and early 1940s treats this period merely as a prelude to the development of renormalized QED in the late 1940s, as a period in which physicists were unable to deal with the infinities appearing in their calculations and consequently made little to no progress in the development of a quantum theory of fields. From this vantage point, Dancoff's work is viewed as a failed attempt at developing renormalization techniques already a decade earlier.

Schweber (1994) in his major work on the history of quantum electrodynamics characterizes Dancoff's paper as the single investigation (before the formulation of renormalized QED) that attempted to "amalgamate all the previous insights in order to obtain a divergence-free formulation of hole theory". But, so the narrative continues, Dancoff made a rather trivial mistake, obtained a divergent result, and thus concluded, as others had before him and would after him, that quantum electrodynamics was intrinsically flawed and would always deliver nonsensical, infinite results in higher approximations. Schweber explicitly states that, had Dancoff not made his mistake, or had somebody else noticed it, "the difficulties of QED might have been resolved much earlier". This evaluation is shared by Weinberg (1995), who states that Dancoff's results implied the impossibility of removing the infinities of QED by renormalization or subtraction methods, thereby retarding the renormalization program, which had begun in a very rough form already in the mid-1930s. Similar statements are also made by Cao (1997) and Mehra and Rechenberg (2001). They have even made it into Dancoff's Wikipedia entry.

A somewhat more detailed study of Dancoff's work was performed by Aramaki (1987), but also only in the context of the later development of renormalization by Tomonaga. Aramaki's answers are unsatisfactory on several accounts. He gives no explanation for Dancoff's mistake – it is simply a silly oversight, a view I will argue against in this paper. Also, he shies away from the question of whether Dancoff would have arrived at renormalized QED had he not made his mistake. Instead, he only considers the more abstract question whether someone could have arrived at renormalized QED already in 1939, a question which he answers in the affirmative. This is, however, a much weaker statement than the ones found in the other secondary literature quoted above. A sole dissenting voice is that of

E-mail address: ablum@mpiwg-berlin.mpg.de

http://dx.doi.org/10.1016/j.shpsb.2015.03.008
1355-2198/

Darrigol (1982), who raises doubts concerning the usual reading of the Dancoff story .

In clarifying the Dancoff story, more is at stake than the mere debunking of a historical anecdote, because it epitomizes a general notion in the (rather scarce) historiography of QFT: That the development of covariant methods and renormalization in the late 1940s was a conservative move, where nothing really new entered physical theory, and the techniques of quantum theory were only, finally, correctly applied. The physicists of the 1930s, so the general reading, had failed to see the obvious, due to a mix of lacking experimental input and a romantic yearning for another revolution in physics, to follow up and complete the relativity and quantum revolutions. Dancoff's work is then seen as exemplifying this tendency of 1930s physics, because supposedly all the physical principles had been in place and only a calculational error doomed the effort. Certainly, the ultimate solution to the divergence crisis of QFT turned out to be not nearly as radical as some of the failed proposals brought forth in the 1930s. But a re-reading of the Dancoff anecdote allows us to see more clearly the conceptual difficulties that needed to be overcome for the "victory of conservatism", as Freeman Dyson (1965) has called the triumph of covariant renormalized QED in the late 1940s.[1]

In this paper, I will give a detailed analysis of Dancoff's paper. Rather than studying it merely through the lens of later work, I will contextualize it in the theoretical physics of its time, as part (and even the culmination) of the investigation of a difficulty not immediately connected to the problems later addressed by renormalized QED, namely the difficulty of the infrared divergence. Placing Dancoff's work in this context allow us to understand why Dancoff performed his study of higher-order corrections in hole theory in the first place: After all it is generally assumed that the motivation to perform such intricate calculations was only present in the late 1940s, after the actual experimental discovery of what could be interpreted as such higher-order effects in the hydrogen spectrum (Lamb Shift) and the magnetic moment of the electron.

This contextualization will further allow me not only to reconstruct how Dancoff came to make his mistake, but also to correct the established view that Dancoff's work was meant to resolve the problems of QED in general. This leaves us (at the end of Section 4.1) with the question of how this established view came to be. I will address this question in the final sections of this paper: On one hand, I will show which conceptual advances were necessary for a re-evaluation of Dancoff's work in the late 1940s, thereby arguing against the claim that Dancoff would have directly ended up with renormalized QED in 1939 if he hadn't made his mistake. On the other hand, I will attempt to show how the physicists who performed this re-evaluation, at the same time immediately began developing the narrative of the missed chance that Dancoff's work represented, thereby also fostering the impression that renormalized QED was only a conservative reformulation of earlier theories, adding nothing really new to the well-established concepts of quantum theory.

For the time being (i.e., for the first sections of this paper), these questions relating to later developments will be set aside, and I will discuss the pre-history of Dancoff's paper, describing the practices, problems and limitations of 1930s QED in their own right. I have already mentioned that there was no direct empirical motivation for Dancoff's work: He performed a detailed study of the radiative corrections to the scattering of an electron in an external potential despite the fact that no deviations from the well-established relativistic scattering cross sections (calculated at leading order in the fine structure constant α, or, equivalently, the squared electron charge e^2) had been observed.

But in fact there had been worries about such deviations 10 years earlier, when Neville Mott (1929) calculated the scattering of a relativistic electron with spin in a Coulomb potential, using the newly established Dirac equation and obtaining the scattering cross section that now carries his name. He was unsatisfied with his result, since it did not seem to agree with scattering experiments of β rays by aluminum, performed by Chadwick and Mercier (1925). These experiments observed too much scattering for all, but especially for small angles. This did not develop into a major anomaly, and Mott's scattering formula is taught in introductory high energy physics courses to this day. However, it did motivate Mott to suspect that the discrepancies might be removed by taking into account radiative corrections. The investigation of this question, which he published in Mott (1931), forms the starting point of our story.

Mott arrived at the conclusion that radiative corrections would be too small to account for the observed deviations, and things might just have stayed at that. However, he encountered a new difficulty in his calculations, the so-called infrared divergence. It was the further investigation of this difficulty that led to Dancoff's investigation eight years later. I will thus begin by giving a history of the infrared divergence.

2. The infrared divergence

2.1. The infrared divergence appears

The infrared divergence appeared, as far as I can tell for the first time, already eight years before Mott's paper, even before the advent of quantum mechanics, let alone QED, in the work of Friedrich Hund. For his PhD thesis, Hund had been studying the energy loss of electrons in a rarefied gas, in order to explain the recently discovered Ramsauer effect. His calculations had been entirely classical; but, shortly before Hund was to hand in his thesis, his advisor James Franck had prodded him to investigate the energy loss through radiation using Bohr's (quantum) correspondence principle. After some days of contemplation, Hund objected. If one assumed that radiation was only emitted one quantum jump at a time in units of $h\nu$, one arrived at a contradiction: Since the classically calculated intensity does not go to zero for zero frequency, one would end up with an infinite probability for the emission of a low energy quantum. But Franck and a newly arrived post-doc, Werner Heisenberg, brushed aside Hund's objections: Heisenberg argued that there was no problem with a large number of low-energy quanta being emitted. And so, this infrared difficulty was merely mentioned as an aside in the paper that grew out of Hund's dissertation (Hund, 1923), and was apparently forgotten, in particular by Heisenberg (Jähnert, 2015).

None of the later works on the infrared divergence cites this work of Hund. If Hund's work had been more widely known, one could have already anticipated that the problem would reappear when considering the same physical situation in QED. After all, the only way to perform actual calculations in QED was the use of perturbation theory, based on the assumption that the probability for the emission or the absorption of a photon would always be small – in contrast with the realization of Hund and Heisenberg that a large number of low-energy photons would generally be emitted in the scattering of an electron. This difficulty was discovered by Mott, when calculating the radiative corrections to electron scattering.

Mott's calculation was based on Dirac's radiation theory (Dirac, 1927). We thus briefly need to discuss how Dirac's theory is related to actual quantum electrodynamics, which forms the framework for all of the work discussed in the remainder of this paper. Although

[1] The conservatism of the renormalization revolution is also stressed by Weinberg (1977), where this conservatism is proposed as a general characteristic of the history of quantum field theory, to be found also in the development of renormalizable gauge theories in order to deal with nuclear interactions.