Alexander Blum

Statement

and

Readings

Quantum Field Theory in the 1940s and 1950s

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Abstract

I will discuss a paper by Sidney Dancoff from 1939, which in the secondary literature is generally viewed as a failed attempt to develop renormalized quantum electrodynamics (QED) a decade early, an attempt that failed because of a mistake that Dancoff made. In my presentation, 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).

QED AND THE MAN WHO DIDN'T MAKE IT

Alexander Blum

Draft
(Do Not Circulate)

Sidney Dancoff's 1939 paper "On Radiative Corrections for Electron Scattering" [14] 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.

Sam Schweber in his major work [65] on the history of quantum electrodynamics (QED) characterizes Dancoff's paper as the single investigation (before the formulation of renormalized QED) that attempted to "amalgamate all the 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 [72], 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 [11] and Mehra and Rechenberg [45]. They have even made it into Dancoff's Wikipedia entry.

A somewhat more detailed study of Dancoff's work was performed by Aramaki [2], 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 which 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, but only very abstractly considers the 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.

In this paper, I will give a significantly more 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 my section 3.1) with the question of how this established view came to be. I will address this question in the final sections of this paper: On the 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 Danoff'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, ten years earlier, there had been worries about such deviations. In 1929, Neville Mott 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 [47]. He was, however, unsatisfied with his result, since it did not seem to agree with scattering experiments of β rays by aluminum, performed by Chadwick and Mercier in 1925 [12]. 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 1931 [48] 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.

1 The infrared divergence

1.1 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 handing 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 [31], and was apparently forgotten, in particular by Heisenberg [35].
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 of 1927 [17]. 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 somewhat peripheral at first glance, this discussion also gives some first insights into the origin of Dancoff's mistake.
Dirac's theory had been envisioned by its creator as a provisional, not entirely relativistic theory of radiation, describing the emission and absorption of light quanta. It was unclear how this would relate to a fully relativistic theory of the electromagnetic field. Such a theory was then developed in the years 1929/30 by Pauli, Heisenberg, Oppenheimer and Fermi [27, 50, 21], leading to a somewhat surprising conclusion: Relativistic QED was basically equivalent to Dirac's radiation theory. One simply added to Dirac's interaction term between radiation and matter a Coulomb interaction term, which, for reasons of relativistic invariance, not only included the instantaneous electrostatic interaction between the charged particles in the system, but also the divergent Coulomb self-interaction of each particle. In other words, one could split the fully relativistic, quantized electromagnetic field of QED into a transversal radiation part, which was equivalent to Dirac's quantized radiation field, and an unquantized Coulomb interaction. In the course of the 1930s it was realized that this separation could be understood as a gauge transformation to what is nowadays known as the Coulomb gauge [30]. We do not know, whether Mott was aware of these new developments in QED, but it played no

important role for him. The Coulomb interaction did not appear in his calculations (except as the external scattering potential of the nucleus), since there was only one electron in the physical system under study, and hence no Coulomb interaction with other electrons. There was of course the divergent Coulomb self interaction of the electron, which could only be neglected at the cost of losing relativistic invariance. This term will play a decisive role in my investigation. Mott's calculation was however non-relativistic, as he used the Schrödinger, instead of the Dirac, equation to describe the electron. This is somewhat surprising, given the fact that he had originally used the Dirac equation to calculate his scattering formula. In the paper, Mott was in fact somewhat ambiguous about the relativistic validity of his approach and talked about velocities going to infinity in his final result. Be that as it may, he did not use the Dirac equation, perhaps because the problem of the negative energy states scared him off. Thus, even if he considered the problem of the Coulomb self-energy, he was entirely justified in neglecting it, as simply an infinite constant in the Hamiltonian, with no effect on the dynamics.

In his calculation, Mott cleanly separated the scattering process from the radiative corrections: The scattering problem without radiation was taken to be solved by a wave function F_E (calculated using the Born method), which described both the incoming and the scattered wave. The radiative corrections were then considered as corrections to this wave function F_E caused by the small interaction between the electron and the transversal radiation field. So as not to have to introduce too many different conventions in this paper, I will give this interaction term in a notation somewhat different from Mott's. The difference in Mott's conventions, as compared with later ones, stems from two main points, which he inherited from Dirac's 1927 work: First, Mott used number and phase operators instead of annihilation and creation operators. And second, Dirac had assumed that the radiation field only appeared in discrete modes (a mode s being defined by a specific angular frequency ω_s, a polarization vector $\boldsymbol{\lambda}_s$ and a propagation vector $\mathbf{q}_s$), but offered no prescription on how to enforce this discretization. His expressions thus include the unspecified quantity σ_s denoting the number of modes in an infinitesimal neighborhood of the mode s. In later work, a specific discretization procedure is used, where the electromagnetic potential is quantized in a box of finite volume Ω and boundary conditions are imposed (such as periodic boundary conditions or the vanishing of the potential on the boundary). The interaction term then instead contains the volume Ω. Both σ_s and Ω are supposed to disappear in all actual physical results, but the use of the latter makes the calculation more transparent, since it does not depend on the mode. This makes it easier to keep track of the dependence on the other properties of the mode, such as on the frequency ω_s. With all this taken into account, the perturbation Hamiltonian describing the interaction between electron and radiation takes the form

$$H_{\text{int}} = -\frac{e}{mc}\left(\mathbf{A}(\mathbf{x})\cdot\mathbf{p}\right) = -\frac{e}{m}\sum_s \sqrt{\frac{2\pi\hbar}{\omega_s\Omega}}\left(a_s e^{-i\mathbf{q}_s\cdot\mathbf{x}} + a_s^\dagger e^{i\mathbf{q}_s\cdot\mathbf{x}}\right)\left(\boldsymbol{\lambda}_s\cdot\mathbf{p}\right) \tag{1}$$

where e, m, $\mathbf{x}$ and $\mathbf{p}$ are the electron's charge, mass, position operator and momentum operator, respectively, and the electromagnetic vector potential $\mathbf{A}(\mathbf{r})$ has been Fourier decomposed into its discrete modes s using a_s and $a_s^\dagger$, the annihilation and creation operators, which induce the absorption and the emission of a photon in the mode s, respectively. The expansion parameter for the perturbative calculation is taken to be the electron charge e (or rather the dimensionless quantity $\sqrt{\alpha} = e/\sqrt{\hbar c}$). The corrected wave function ψ is then of the form (modulo normalization factors):

$$\psi = F_E + e\sum_s \psi_s + e^2\left(\psi_0 + \sum_{s,t}\psi_{st}\right) + \mathcal{O}\left(e^3\right) \tag{2}$$

where the term ψ_s gives the probability for a radiated photon in the mode s, the term ψ_{st} has two emitted photons in the modes s and t and the term ψ_0 has no photons, i.e., describes the scenario in which perturbatively a photon is emitted and reabsorbed. Now of course Mott was not interested in corrections to the scattering amplitude, but in corrections to the cross section. In particular, he was interested in corrections to the inclusive cross section for electron scattering, i.e., he did not care about emitted photons, as these were not observed in the experiments he was considering. This inclusive cross section thus depended on all the terms in the perturbative expansion of ψ, independent of the number of photons. However, when calculating the cross section (which involves taking the absolute square of the wave function above), only those terms in ψ interfere which have the same number of photons in the same modes. There are thus two distinct corrections to the cross section at first order (that is order α), and I will be calling them radiative corrections of the first and second type. The first is proportional to $|\psi_s|^2$, summed over all modes s allowed by energy conservation (i.e., with frequencies less than the electron's kinetic energy divided by Planck's constant), and corresponds to inelastic scattering, where the electron emits an unobserved photon into the mode s during the scattering process. The second is proportional to the real part of $F_E\psi_0^*$ and also contains a sum over modes, since ψ_0 contains contributions from intermediate (i.e., virtually emitted and reabsorbed) photons of all modes, in this case not limited by energy conservation considerations. This second correction gives corrections to elastic, radiationless scattering from the interaction between the electron and its own radiation field without the emission of an actual, potentially observable photon.

Mott was aware of both types of radiative corrections, but mainly focused on those of the first type. These he calculated in great detail, but the modern reader can already see the decisive points from the perturbation Hamiltonian above:[1] The radiative correction to the cross section (i.e., the squared amplitude) will be suppressed not only by the fine structure constant $e^2/(\hbar c)$, but also by $(v/c)^2$, since the perturbation Hamiltonian is also proportional to the particle's velocity (the momentum operator divided by the mass m). Mott concluded that this double suppression will make these radiative corrections negligible in any case: even if the velocities are high, they will still be on the order of one percent. Mott further concluded that they could thus not be responsible for the apparent discrepancies. He observed, however, one difficulty: The correction to the cross section will also be proportional to $1/\omega_s$ (as one can also see from the perturbation Hamiltonian), and for small frequencies this will more than compensate the smallness of charge and velocity. In fact, when integrating over all allowed modes to obtain the full radiative correction, one obtains a logarithmic divergence. Mott recognized that the problem was that for low frequencies perturbation theory was no longer applicable. He concluded that his result for the cross-section with photon emission was thus only valid above a minimal radiated frequency. He however gave no indication as to what might happen below that frequency, apparently assuming that the cross-section, rather than diverging,

[1] The modern reader is consequently also able to spot the infrared divergence in papers, where it was not noticed by the authors themselves. Already the first paper on quantum electrodynamics by Heisenberg and Pauli contains the infrared divergence, most explicitly in equation 106 on page 50 [26], another indicator that Heisenberg had entirely forgotten the discussion with Hund. Pauli on the other hand, as we will see, was aware of the problem, only a few years later.

should actually drop off rather fast. Just as it had eight years earlier, the infrared divergence remained as an unsolved problem, a problem, however, that Mott did not consider to be fundamental.
What about the second type of radiative correction? As already indicated, Mott was well aware of this, but was not able to calculate it. He simply stated that "the integrals are so complicated that we have not attempted it", and argued on general grounds that they would be of the same order as the radiative corrections of the first type. He did not mention, and it is not clear whether he was aware of the fact, that these contributions would be just as infrared divergent as those of the first kind. He also did not mention, and here we can be pretty sure that he was not aware of the fact, since he would have had to consider this a serious problem, that these corrections of the second type are also divergent in the ultraviolet, since the sum over modes is not cut off at a maximal frequency in this case. It was only gradually becoming clear around this time that QED would generally lead to such ultraviolet divergent results at order α, divergences which could not be removed as easily as the Coulomb self-energy divergence. These ultraviolet divergences formed a major center of attention in quantum field theory research in the next years. As for the infrared divergence, those who were aware of it, tended to share Mott's opinion that it posed no fundamental difficulty. But that conviction did not get rid of it, and it continued popping up once in a while over the next few years.

1.2 The infrared divergence stays

If the infrared divergence would only appear in radiative corrections of the sort Mott was considering, the problem most probably would have remained entirely under the radar for quite a while. However, it also appears when the emission of the photon is not just considered as an unobserved correction to the radiationless scattering cross section, but forms the actual physical process one is interested in, as had been the case in Hund's work. For the case of the radiation emitted by an electron scattered by an external potential, this emitted radiation goes by the name of *bremsstrahlung*, and had been an active area of experimental and theoretical investigation for quite some time. In 1931, the same year as Mott's calculation, a major investigation of the *bremsstrahlung* spectrum was provided by Arnold Sommerfeld [69], who had studied this question already two decades earlier in the early days of quantum theory. His investigation was further expanded in a paper by his student Otto Scherzer in 1932 [63].
In the last section of this paper, Scherzer touched upon the question of the scattered electron, which is emitting the radiation, the question also considered by Mott. Scherzer first calculated the correlations between the *bremsstrahlung* and the trajectory of the scattered electron, but concluded that these calculations could not yet be tested experimentally, since no experiments had been performed in which such correlations were measured. All experiments were either interested in the scattered electrons (i.e., Mott's problem) or the emitted radiation (i.e., *bremsstrahlung*). For completeness, and because he did not agree with Mott's conclusions about the infrared divergence, Scherzer then briefly investigated the question of the scattered electron by itself, independent of the emitted radiation, i.e., considered the emitted radiation as a radiative correction to the radiationless scattering. Coming from the question of *bremsstrahlung* as he did, he entirely neglected radiative corrections of the second type, in which no photon is emitted. Consequently, he arrived at the same infrared divergence that Mott had observed, but interpreted it in a different way: He did not believe that it was perturbation theory that broke down for small frequencies, but that the infrared divergence was

comparable to the divergence of the Rutherford scattering for small angles. One should be able to solve it, by representing the incoming electron as a wave packet and not as a plane wave.[2]
Wolfgang Pauli was also aware of the problem of the infrared divergence and set his new assistant Viktor Weisskopf to work on it when he arrived in Zurich. Shortly after taking up his new position, Weisskopf wrote to Niels Bohr (November 7 1933; AHQP, BSC, M/f No. 26):

> Ich beschäftige mich jetzt mit ein paar Fragen der Strahlungstheorie bei der Emission kleiner Quanten, wo der Anschluss an die klassische Theorie nicht ganz klar aus dem Formalismus hervorgeht.

and years later, after having returned to the problem with Weisskopf's successor Markus Fierz, Pauli wrote to Weisskopf:

> ...ich [...] kündige einen Bericht an über "Die Emission langwelliger Lichtquanten in der Strahlungstheorie". Dies ist ja ein Problem, an welchem Sie in Zürich eine Zeitlang gehangen sind und es wird Sie daher sicher interessieren darüber zu hören.

We can not say with certainty, what became of Weisskopf's attempts to deal with the infrared divergence. However, it seems quite probable that they are what is referred to in the following anecdote that Weisskopf recounted to Thomas Kuhn and John Heilbron (AHQP interview, 10 July 1965):

> The first time I came to see him [Pauli], I knocked at the door - no answer. He was in a very bad mood at that time; the whole period was a difficult one for him for personal reasons. When he didn't answer, after a few minutes I opened the door and he was sitting at the other end of the room. He figures and figures and figures and says "no" all the time with his head - something didn't work probably - and I said, "Herr Professor"; but his only response was, "Warten, warten, warten," "Wait, wait, wait." So I stood there in this disagreeable situation for ten minutes, and then he said, "Ah, [you] are Weisskopf; yes, you will be my assistant. I will tell you that I wanted to take Bethe...;" And then comes the story [Weisskopf had mentioned before that Pauli had only decided not to take Bethe, because Bethe was now into solid state physics, which Pauli found disagreeable]. The sequel to it is that he gave me some problem - I really don't know what it was - and after a few weeks I showed him what I had done; he was very dissatisfied with it and he said: "Ich hätte doch Bethe nehmen sollen."

The problem resurfaced again in Leipzig, in 1936, when it appeared in the dissertation work of Werner Heisenberg's PhD student Bernhard Kockel (Heisenberg to Pauli, February 2 1936).

[2]This may be true. But, as Edgar Weinmann later remarked, this does not mean that the scattering cross section is *correct* for small angles, it has merely been made finite by changing the physical assumptions.

Kockel was investigating processes in which high energy cosmic radiation creates an electron-positron pair, accompanied by the emission (or absorption) of an additional photon,[3] and had realized that the probability of emitting a low energy photon diverged. This rediscovery of the infrared divergence found Heisenberg in a receptive state of mind. In the same letter in which he informed Pauli of his discovery, he also outlined his thoughts on the future of quantum electrodynamics in the following manner:

> At the moment I have no particular opinion on how the theory will look one day; only lots of attempts which so far have led nowhere. I think, for the time being, all we can do is to investigate the consequences of the current theory and learn from that for the future.

The infrared divergence offered just such a possibility to investigate a problem in the current QED that was not directly tied to the troubling ultraviolet divergences. Intrigued, Heisenberg also contacted Gregor Wentzel and Felix Bloch. Bloch had worked extensively on the energy loss of high energy cosmic rays through emission of radiation and pair creation. As it turned out, he had also recently encountered the infrared divergence.
In Bloch's case the primary process was not scattering or pair creation, but nuclear β-decay [6]. Weak radiation accompanying the emission of the electron had recently been observed experimentally, as mentioned in a paper by Julian Knipp and George Uehlenbeck [38], dealing with the same question, in which they also call this phenomenon "inner bremsstrahlung", to highlight the analogy with regular ("outer") *bremsstrahlung*. Bloch obtained a satisfactory result for the spectrum of the emitted radiation, but realized that his conclusions could not be true for arbitrarily low frequencies, since here the probability for the emission of a photon became infinite. Bloch identified this as a generic breakdown of perturbation theory when calculating the emission of radiation accompanying some other, primary process, which could in principle happen without radiation.
Heisenberg agreed that perturbation theory was to blame. In the classical radiation theory, the intensity of the emitted radiation did not go to zero for vanishing frequency. One should therefore expect, from correspondence considerations, that an infinity of low-energy (soft) photons be emitted in the quantum theory, a process patently not describable in the regular perturbation theory in which one photon is emitted at a time. These conclusions of Heisenberg's (which were equivalent to those of Hund 13 years earlier) were however published only as a side remark in the paper that grew out of Kockel's dissertation [40]. Heisenberg's attempts to repair the difficulty once and for all failed, as witnessed by a manuscript of his on the subject, which simply breaks off (AHQP, M/f No. 45, Sect. 12-001). Instead, the question was solved independently by two of Heisenberg's students at the time, Arnold Nordsieck (on a short visit to Leipzig on a Rockefeller scholarship [1, 37]) and Markus Fierz, both of whom however only came around to do this at the next station in their careers, with the help of their new supervisors. These new supervisors had also both already encountered the infrared divergence and worried about it: Nordsieck continued on to work with Bloch in Stanford, while Fierz joined Pauli in Zurich.

[3] Kockel's research forms the beginning of Heisenberg's interest in such multiple cosmic ray processes, which would later focus on the question whether they could be described by showers, i.e., cascades of single emission and creation processes described by perturbation theory, or by explosions, i.e., events in which multiple particles are created simultaneously in a non-perturbative manner.

1.3 The infrared divergence removed

Before going to Stanford, Arnold Nordsieck discussed inner *bremsstrahlung* with Knipp and Uehlenbeck, further sensitizing him for the problem of the infrared divergence. When he arrived with Bloch, he was apparently dead set on solving the problem. Bloch recalled (Interview with Charles Weiner, 15 August 1968):

> He came here and said: This is a scandal. There is this problem, and one should be able to solve it. So he convinced me, and we worked together on that.

Fierz also, before leaving for Zurich[4], encountered the infrared divergence in work of his own: Here the primary process accompanied by the emission of radiation was inverse β decay [22]. We have no records on how the the work of Pauli and Fierz on the infrared divergence started, but given the fact that Pauli had already let Weisskopf work on the question years earlier and that the problem was now apparently becoming "hot", Pauli probably needed as little convincing as Bloch. Only recently, he had (in a letter to Kronig from 20 November 1935) expressed a view on the future of quantum electrodynamics quite similar to that of Heisenberg's:

> [T]heoretical Physics is doing very badly right now and I would grasp at any straw offered to me...

In any case, Bloch and Nordsieck were faster and presented their solution of the problem in February 1937 at the Washington Conference on Theoretical Physics. Another Indicator that the problem was "hot" can be found in the letters which the participants sent to the organizers after the conference. In a letter dated 2 March 1937, Wendell Furry wrote:

> ...the thing which interested me most personally was Professor Bloch's discussion of his work in radiation-theory. The problem in question was one in which there had been considerable recent interest, not without some confusion; and the opportunity to hear a discussion of what seems to be a valid way of treating it was quite valuable. (Niels Bohr Library and Archives, Washington Conference on Theoretical Physics records)

Bloch and Nordsieck published their work in a paper submitted in May 1937 [7]. If Pauli is to be believed, he and Fierz were basically on the same track (Pauli to Heisenberg, June 10 1937). And indeed, the paper presented by Pauli and Fierz presented in October in Bologna at the Galvani bicentennial (upon which they expanded somewhat in the published version), formed both a clarification and an extension of the calculations by Bloch and Nordsieck [55]. Whenever talking about the general approach, employed by both pairs, I will take the liberty of using the abbreviation BNPF for the names of the four authors.
To solve the question of the infrared divergence, BNPF returned to the physical scenario in which it had originally appeared in Mott's work: The primary process is the scattering of an electron in an external potential, which is accompanied by the emission of radiation. The gist of the BNPF solution is then, that, since a perturbative treatment of the interaction between electron and radiation does not work for low frequency modes, an exact solution of this

[4] Heisenberg had recommended him to Pauli as a new assistant. Letter from Heisenberg to Pauli, May 23 1936.

problem is necessary for these modes. The external potential is then considered as a small perturbation of this exact solution. When compared to Mott, the role of exact unperturbed Hamiltonian and perturbation are switched: The interaction of the electron with its own radiation field (at least with the low frequency modes) becomes the primary process, perturbed by the presence of an external potential. The calculation thus consists of two parts: Finding an exact solution for the interaction of electron and radiation, and then perturbing this solution with an external potential to solve the scattering problem. I will treat these two parts separately in the following two subsections.

1.3.1 Solving the Radiation Problem

Exactly solving the problem of an electron interacting with radiation was no trivial task: After all, Dirac's radiation theory and QED had so far only allowed for perturbative solutions. The decisive idea was to employ a canonical transformation, which separated the electron's transversally polarized self-field from the freely propagating photons.
The starting points of the calculations of Bloch/Nordsieck and Pauli/Fierz are slightly different: Pauli and Fierz directly started with the non relativistic (i.e., Schrödinger) Hamiltonian, while Bloch and Nordsieck took the relativistic Dirac Hamiltonian and made several non-relativistic approximations along the way. This makes the calculations and conventions of Pauli and Fierz more transparent, and I will thus follow them in describing the general BNPF scheme. I will then turn to the differences brought about by the use of the relativistic Dirac Hamiltonian in the work of Bloch and Nordsieck and address how they relate to Dancoff's later error.
In the non-relativistic framework of Pauli and Fierz, the Hamiltonian H of the electron interacting with radiation takes the form

$$H = \frac{\mathbf{p}^2}{2m} - \frac{e}{mc}\left(\mathbf{A}(\mathbf{x})\cdot\mathbf{p}\right) + \frac{1}{8\pi}\int\left(\mathbf{E}_{\mathrm{tr}}^2(\mathbf{x}') + \mathbf{H}^2(\mathbf{x}')\right)d^3x' \tag{3}$$

where $\mathbf{H}$ is the magnetic field and $\mathbf{E}_{\mathrm{tr}}$ is the *transversal* electric field (with a vanishing divergence, i.e., fulfilling the Maxwell equations in vacuum). This distinction requires a brief explanation. First of all, I have here adopted the notation of Pauli and Fierz, where only the electric field carries a special index marking it as transversal. Both the magnetic field and the vector potential contain only the transversal components. For the magnetic field this is trivially the case, since it always has a vanishing divergence, even if charges are present; for the vector potential the transversality expresses itself in the fact that when decomposing it into its Fourier modes (cf. equation 1), one assumes only the transversal modes to be present. So the Hamiltonian describes an electron interacting with a potential and the transversal electromagnetic field. What about the other components of the electromagnetic field?
As already mentioned in section 1.1 above, one can express the interaction with the full quantized electromagnetic field instead as an interaction with the transversally polarized (radiation) field and an instantaneous Coulomb interaction. This transformation, which established the connection between QED and Dirac's radiation theory was commonplace by 1937, as witnessed by its presentation in Heitler's influential 1936 textbook [29]. It eliminated the longitudinal components of the electromagnetic field and replaced them with the electron's

Coulomb field, interacting both with other charged particles and with the electron itself.[5] Pauli and Fierz started (as was commonplace) with a Hamiltonian in which the longitudinal modes had already been eliminated and the divergent Coulomb self-interaction (for that is all that is left of the longitudinally polarized field in the one-electron problem) had been dropped, so that only the interaction with the transversal field remained.
Returning to the Hamiltonian above, we recognize the kinetic energy of the electron (first summand) and the energy of the radiation field (third summand). The second summand is the interaction term already used by Mott (equation 1).[6] BNPF, however, used a somewhat different expansion of the vector potential. Instead of expanding in complex plane waves with annihilation and creation operators as coefficients, they expand in real sine and cosine functions, with coefficients Q_s and P_s which can be interpreted as the position and the momentum of a harmonic oscillator associated with the mode s. The interaction term then reads[7]

$$H_{\text{int}} = -\frac{e}{mc}\left(\mathbf{A}(\mathbf{x})\cdot\mathbf{p}\right) = -\frac{2e}{m}\sum_s \sqrt{\frac{\pi\hbar}{\omega_s\Omega}}\left(P_s\cos(\mathbf{q}_s\cdot\mathbf{x}) + Q_s\sin(\mathbf{q}_s\cdot\mathbf{x})\right)\left(\boldsymbol{\lambda}_s\cdot\mathbf{p}\right) \tag{4}$$

while the energy of the radiation field takes the form

$$H_{\text{rad}} = \frac{1}{8\pi}\int\left(\mathbf{E}^2_{\text{tr}}(\mathbf{x}') + \mathbf{H}^2(\mathbf{x}')\right)d^3x' = \frac{1}{2}\sum_s (P_s^2 + Q_s^2)\hbar\omega_s \tag{5}$$

The reason for switching to oscillator phase space instead of the occupation number space of annihilation and creation operators, is that the decisive transformation is more easily implemented in the former: It can simply be described as a canonical transformation in a classical phase space, rather than as a unitary transformation of the operator algebra acting on the wave function Hilbert space. In fact, written in this way, as Bloch and Nordsieck state, the necessary canonical transformation to solve the system of the electron interacting with its radiation field immediately suggests itself. To demonstrate this, I adopt one more simplification, used also by Pauli and Fierz, namely, to take the position of the (point-like) electron as the origin of the coordinate system and thereby replace all sines with 0 and all cosines with 1. The full Hamiltonian then takes a very simple form:

$$H = \frac{\mathbf{p}^2}{2m} - \sum_s \hbar\omega_s P_s\left(\mathbf{a}_s\cdot\mathbf{p}\right) + V(\mathbf{x}) + \frac{1}{2}\sum_s (P_s^2 + Q_s^2)\hbar\omega_s \tag{6}$$

[5] Actually, which degrees of freedom disappear due to this transformation depends on the exact quantization procedure. I have described the procedure of Heisenberg and Pauli in which the time-like polarized components of the electromagnetic field are already eliminated through the fixing of the (temporal or Weyl) gauge. In the more popular Fermi method of quantization, time-like and longitudinally polarized components, which are correlated through the Lorenz gauge condition, are simultaneously eliminated by the described transformation.

[6] An additional term, proportional to the square of the vector potential, is neglected by Pauli and Fierz. It has no analog for Bloch and Nordsieck, who start with the Dirac equation, which is linear in the electron momentum.

[7] To arrive at this equation, both Bloch/Nordsieck and Pauli/Fierz employed a definition of annihilation and creation operators which is slightly different from the standard convention (used for example in Wentzel's Handbuch article [79], which Bloch and Nordsieck cite) in that the role of momentum and position are exchanged, i.e., $a_s = \frac{1}{\sqrt{2}}(P_s + iQ_s)$. The reason for the use of this non-standard definition of annihilation and creation operators escapes me.

where, following BNPF, I have absorbed most of the constants into the polarization vector by using

$$\mathbf{a}_s = \frac{2e}{m}\frac{1}{\hbar\omega_s}\sqrt{\frac{\pi\hbar}{\omega_s\Omega}}\boldsymbol{\lambda}_s \tag{7}$$

This is the Hamiltonian of a set of harmonic oscillators (the modes of the radiation field) disturbed by an external force (the motion of the electron). One can thus feasibly reconstruct, how BNPF arrived at their results, and indeed, Heisenberg wrote to Pauli after reading Bloch and Nordsieck's paper: "Their solution to the problem is really surprisingly simple, and I am embarrassed that I did not find it myself." (12 June 1937) The general idea of treating the radiation problem in this form, can already be found in Heitler's textbook, where he writes: "The emission of light appears here as the excitation of a harmonic oscillator (light) by a periodic external force (electron)..." (p. 54). All that needs to be done to find an exact solution, is to complete the square for the oscillator momenta P_s, i.e., by performing a translation in phase space:

$$P_s = P'_s + (\mathbf{a}_s \cdot \mathbf{p}) \tag{8}$$

The interaction term then disappears from the Hamiltonian and is replaced by a term quadratic in the "disturbing potential", i.e., in the electron momentum:

$$-\frac{1}{2}\sum_s (\mathbf{a}_s \cdot \mathbf{p})^2 \hbar\omega_s \tag{9}$$

The oscillators and the external force are thereby completely separated. One has the Hamiltonian for a set of undisturbed harmonic oscillators, which are interpreted as the modes of the freely propagating radiation, and the Hamiltonian of a free electron, with an additional term proportional to its momentum squared, which was interpreted as the contribution of the transversal self-field to the electron's self energy. There was however the difficulty that this transversal self-energy diverges. And as opposed to the Coulomb self-energy it is not simply a constant contribution to the energy, which has no observable effect. Such a term quadratic in the electron momentum can rather be interpreted as a correction to the electron mass. A modern-day reader may now be tempted immediately to jump to the conclusion that such a correction to the mass is of course not directly observable either, and can thus simply be absorbed into the bare mass m to give the observed mass. This is a central idea in mass renormalization. One however only has to look at Pauli and Fierz's treatment of this term to realize that, although they effectively performed a mass renormalization by dropping this term, the concept was not yet present in their work. They introduce a cut-off frequency ω_1 and postulate that modes with higher frequencies do not interact with the electron. This cut-off is explicitly introduced into the theory as the physical assumption that the electron charge is spread out. The extended electron then has a strongly suppressed interaction with those modes whose wavelengths are smaller than the size of the electron. With this cut-off the electromagnetic mass becomes small and can consequently be neglected. Pauli and Fierz thus do not[8] introduce the cut-off to make the electromagnetic mass finite (regularization) and to

[8]As Schweber [65] appears to claim on p. 89. Miller [46], p. 82, on the other hand, agrees with my analysis.

then absorb it into the bare mass or explicitly subtract it (renormalization), they actually introduce the cut-off (along with a makeshift physical interpretation) to make it small and then neglect it. With the transversal self-energy neglected, the Hamiltonian is then simply that of a free electron and free light quanta, without any interaction. This allows for a simple solution for an energy eigenstate ψ: It is a plane wave with a given momentum $\mathbf{k}$ for the electron, multiplied with an infinite product of harmonic oscillator solutions (Hermite polynomials $H_n(q)$) with a given number of quanta n_s for each mode s of the free radiation field:

$$\psi = e^{\frac{i}{\hbar}\mathbf{k}\cdot\mathbf{x}'} \prod_s H_{n_s}(Q_s) \tag{10}$$

This solution can then be perturbed through the introduction of an external potential $V(x)$ to solve the scattering problem. Before turning to this second part of the BNPF calculation, I will briefly explain the differences between the Bloch-Nordsieck and the Pauli-Fierz treatments, arising from the fact that Bloch and Nordsieck started from the relativistic Dirac Hamiltonian. Just like Pauli and Fierz, Bloch and Nordsieck also began with the Hamiltonian for an electron interacting only with the transversal, radiation components of the electromagnetic field, i.e., they had also already eliminated the longitudinal degrees of freedom. As opposed to Pauli and Fierz, they did not take knowledge of this procedure for granted, and actually cite Pauli's 1933 Handbuch article [54], in which the elimination of the longitudinal modes is described. In this article, Pauli stressed that dropping the Coulomb self-interaction violated relativistic invariance, since this divergent term was not Lorentz invariant. But Bloch and Nordsieck anyway implemented further non-relativistic approximations, so they did not have to worry about this fact too much. The electromagnetic mass term of Pauli and Fierz, given in equation 9 then appeared in the paper of Bloch and Nordsieck simply as a term quadratic in the electron velocity. Just as Pauli and Fierz had done, Bloch and Nordsieck introduced a cut-off, motivated by the fact that they were only trying to solve the problem of the emission of low frequency photons, and were therefore justified in ignoring the contributions from high-frequency modes. The transversal self-energy is then small and can be neglected.
As opposed to Pauli and Fierz they did not identify this term with a contribution to the mass, and in fact they would not have been able to do so: By itself this term in the relativistic theory is not a mass term. To obtain a divergent, relativistic, electromagnetic mass term for a Dirac electron, one also needs to take into account the divergent Coulomb self-interaction. In the non-relativistic theory, the Coulomb self-interaction can be dropped as an unobservable, constant contribution to the energy, while the transversal self-interaction can be identified, as done by Pauli and Fierz, with the kinetic energy contribution of the electromagnetic mass. In a relativistic theory, on the other hand, as Pauli had mentioned in his Handbuch article, one cannot simply drop the Coulomb interaction, because it is not a relativistic invariant (in the same way that energy is not a Lorentz scalar). Instead, one can keep it, modify it according to hole theory, and then combine it with the transversal self energy to form a relativistically invariant, divergent contribution to the mass. This was not realized by Bloch and Nordsieck (or anybody else), and it did not make much of a difference to them, since they were not aiming for a full relativistic calculation. Why then do I put so much emphasis on this? Because Dancoff made precisely the same mistake as did Bloch and Nordsieck, only that he was aiming at such a relativistic calculation: He dropped the Coulomb self interaction, and then obtained expressions similar to those of Bloch and Nordsieck, which by themselves cannot be interpreted as the effect of the electromagnetic mass of the electron. I will return to this point in

more detail, when discussing Dancoff's paper. For the time being, however, I return to the BNPF calculation, specifically to the second part in which the obtained solution is perturbed by an external scattering potential.

1.3.2 Solving the Scattering Problem

To solve the scattering problem, and thereby the problem of the infrared divergence, an external perturbing potential $V(x)$ is added to the original Hamiltonian of equation 4.[9] This potential term recouples the electron and the radiation field in the following way: The transformation of the oscillator momenta is not yet canonical - one also needs to transform the position variable of the electron so that it commutes with P'_s. This transformation is again simply realized as a translation in position space:

$$\mathbf{x} = \mathbf{x}' - \hbar \sum_s \mathbf{a}_s Q_s \tag{11}$$

The perturbation term in the Hamiltonian is then

$$V(\mathbf{x}) = V(\mathbf{x}' - \hbar \sum_s \mathbf{a}_s Q_s) \tag{12}$$

which involves both the electron and the radiation mode coordinates. This perturbation then gives transitions between solutions of the exactly solved free system (electron plus radiation field), i.e., it gives a scattering of the electron along with the emission and absorption of an arbitrary amount of photons. BNPF were thus able to treat the emission of an infinite amount of low frequency photons already at first order in perturbation theory, and thereby circumvent the difficulty that Heisenberg had identified in regular radiation theory, that it was unable to handle the apparent infinity of emitted photons in the low frequency modes. The specific transition they considered was a scattering of the electron from a momentum $\mathbf{k}$ to a momentum $\mathbf{k}'$ along with the emission of an unspecified number of photons n_s in each mode (they assumed no photons to be present in the initial state). The corresponding matrix element could be factorized, by separating the part which involved an integration over the electron coordinates from that which required an integration over the coordinates of the radiation modes. The first factor essentially depended on the form of the potential. For a Coulomb scattering potential it was simply the regular Rutherford scattering amplitude without radiative corrections. The second factor, itself an infinite product of transition matrix elements $(0,\mathbf{k}|K|n_s,\mathbf{k}')$ for each mode, then gave the corrections to Rutherford scattering from the presence of the radiation field. It did not depend on the form of the potential (hence the use of the notation K for the transition element, the use of V being restricted to the first factor) and took the form:

$$\prod_s (0,\mathbf{k}|K|n_s,\mathbf{k}') = \prod_s \int e^{i(\mathbf{k}'-\mathbf{k})\cdot\mathbf{a}_s Q_s} H_{n_s}(Q_s) H_0(Q_s) dQ_s \tag{13}$$

[9] I now return to discussing the BNPF procedure in the simpler, non-relativistic formulation of Pauli and Fierz. For this second part of the calculation, there are no noteworthy differences between the two treatments.

The integral can be calculated and the square of the result then gives a multiplicative correction to the Rutherford scattering cross section, calculated by BNPF to be:[10]

$$\left|\prod_s (0,\mathbf{k}|K|n_s,\mathbf{k}')\right|^2 = \prod_s \frac{1}{n_s!} w_s^{n_s} e^{-w_s} \tag{14}$$

where $w_s = \frac{1}{2}[(\mathbf{k}'-\mathbf{k})\cdot\mathbf{a}_s]^2$. Two conclusions can now be drawn from this result: First of all, BNPF could show that this factor vanishes if only a finite number of light quanta are emitted, due to the exponential suppression. They had thus shown that any scattering process was always necessarily accompanied by the emission of an infinite number of soft photons, thus establishing the correspondence connection with classical radiation theory, which stated that the intensity did not go to zero for radiation of zero frequency, and after 14 years finally solving the problem initially formulated by Hund. The second question was whether the radiation of these soft photons altered the scattering cross section obtained when neglecting radiation. Bloch and Nordsieck showed that this was not the case. They do not give an explicit calculation, but this is how I have reconstructed their argument: To obtain the full inclusive cross section with the emission of soft photons taken into account, one had to sum over the cross sections for a whole set of possible final light quantum configurations $\{n_i\}$. These configurations are a specification of how many final photons are in each mode s. Since Bloch and Nordsieck were only interested in the effect of low-energy photons, they neglected the energy and momentum of the emitted soft photons, and summed over a subset of photon configurations $\{n_i\}'$, with no restrictions from energy and momentum conservation and instead with the single restriction that there are no photons in modes above a certain frequency, where the photons can no longer be considered soft. One then gets for the multiplicative correction to the radiationless scattering cross section:

$$\sum_{\{n_i\}'} \prod_s \frac{1}{n_s!} w_s^{n_s} e^{-w_s} = \prod_{s'} \sum_{n_{s'}=0}^{\infty} \frac{1}{n_{s'}!} w_{s'}^{n_{s'}} e^{-w_{s'}} = \prod_{s'} e^{w_{s'}} e^{-w_{s'}} = 1 \tag{15}$$

where I denote by s' those modes that are to be considered soft, i.e., for which Bloch and Nordsieck allowed there to be photons in the final state. Since the multiplicative correction then turned out to be trivial, Bloch and Nordsieck concluded that the emission of soft photons, with negligible energy and momentum, did not alter the radiationless cross section. Together with Mott's perturbative calculation, which was valid for the emission of high energy photons, one could now definitely conclude that radiative corrections to the scattering process would always be small. The apparent infrared divergence had been solved by applying non-perturbative methods for the interaction with low-frequency radiation. One could stop here, and that is how the story of the infrared divergence is often told: It was the only divergence that was successfully eliminated in the 1930s, because it was not directly related to the ultraviolet divergences, which could only be cured by renormalization procedures. Often echoed is Weisskopf's sentiment that

> This [...] "infrared catastrophe" can be avoided by describing this limit with the help of classical fields, as Felix Bloch and Arnold Nordsieck showed in their im-

[10] Note that the square of the infinite product of course gives an infinite product of squares. I point out this trivial point, since it took me a while to figure it out.

portant paper of 1937. It put an end to any worries about this kind of infinity. [78, 72]

But things are not that simple. There is something odd about Bloch's and Nordsieck's final calculation. First of all, it does not depend at all on where one draws the border between soft and hard photons, and indeed Bloch and Nordsieck do not specify where they would set such a border. Furthermore, the calculation allows for an infinity of photons in the highest frequency soft mode, while allowing no photons at all in the lowest frequency hard mode.[11] The general problem was thus that it was still entirely unclear how the perturbative calculation for hard photons and the Bloch-Nordsieck calculation for soft photons were related, and what one could hence say about the transition region. So, although it had been shown that in both domains the radiative corrections could be expected to be small, it was the total disconnect between the calculations in these two domains which kept the interest in radiative corrections to electron scattering alive. By pursuing this question further, Pauli and Fierz went beyond the work of Bloch and Nordsieck, thereby discovering a whole new difficulty, which would lead directly to Dancoff's 1939 paper.

2 The Pauli-Fierz divergence

2.1 The Pauli-Fierz divergence appears

There is a certain amount of confusion in the secondary literature concerning the role of the Pauli-Fierz paper. Schweber [65] (p.89) states that Pauli and Fierz clarified the connection between classical radiation theory and the Bloch-Nordsieck procedure, even though it was the Bloch-Nordsieck procedure that had recovered the connection between the classical and the quantum theory.[12] Cao [11] (p. 192) makes the ludicrous claim that the Pauli-Fierz paper was a reaction to the work of Braunbek and Weinmann, which only appeared half a year later and was made in complete isolation (see below). Miller [46] (p.82), finally, only states that Pauli and Fierz reworked the Bloch-Nordsieck calculation more rigorously, but gives no real explanation for why they should have been interested in doing this. There was in fact a very specific point which Pauli and Fierz were interested in, and this concerns the above-mentioned disconnect between the calculations for hard and for soft photons. It had occurred already to Heisenberg, after receiving the Bloch-Nordsieck paper, that the connection to the high-frequency end of the emission spectrum was still lacking. He wrote to Pauli (12 June 12 1937):

Es wäre vielleicht auch lohnend, das Ende des Spektrums auszurechnen - was ja

[11] It may in fact be that Bloch and Nordsieck actually considered *only* photons with zero frequency to be soft. This is how Pauli and Fierz interpreted their calculation, which, as I have already stated, is not given explicitly. This interpretation, however, in no way removes the general problem.

[12] That they interpreted their method in terms of the classical theory in their paper probably goes back to a comment made by Gregory Breit at the Washington Conference. In his post-conference letter (2 March 1937, NBLA), Bloch wrote: "When I reported in the Tuesday session about Nordsieck's and my work on the proper field of the electron and its bearing on radiative phenomena, it was pointed out in the discussion by Breit that those essentially new features that occurred in the treatment of long wave-lengths may be understood in the light of classical theory. "

> Bloch-Nordsieck noch nicht getan haben - aber sicher auch recht umständlich. Willst Du das rechnen lassen?

A few weeks later, Pauli wrote in a letter to Weisskopf concerning his work on the infrared divergence (20 July 20 1937):

> Ein großer Fortschritt ist übrigens, während unsere Arbeit noch im Gange war, von *Bloch* und *Nordsieck* bei diesem Problem erzielt worden. Andererseits haben diese Verfasser die Formeln nicht so weit diskutiert, dass man sehen kann, wo die bisherige Theorie etwas falsches gibt.

Pauli was thus interested in clarifying the connection between Mott's old perturbative calculation and the Bloch-Nordsieck method. The straightforward way to do this, was to attempt to extend the scope of the Bloch-Nordsieck method to hard photons. This required dropping the assumption of negligible energy for the photons radiated off. Pauli and Fierz thus picked up just where Bloch and Nordsieck had ended, with the calculation with which I also ended the preceding section.
As soon as the energy of the photons is no longer negligible, the scattering becomes inelastic, and the energy of the electron in the final state is no longer the same as the initial energy. The energy E lost through radiation during scattering is then an additional parameter of the scattering, comparable to the scattering angle. One is then interested in the differential cross section as a function of E. This functional dependence appears in the multiplicative correction to the elastic (Rutherford) cross section, which Pauli and Fierz consequently denoted by $S(E)$. The non-trivial energy dependence enters equation 15, when one only sums over those light quantum configurations whose energy adds up to the energy lost by the electron. The multiplicative correction to the scattering amplitude then takes the form:

$$S(E) = \sum_{\{n_i\}} \delta\left(E - \hbar \sum_s n_s \omega_s\right) \prod_s \frac{1}{n_s!} w_s^{n_s} e^{-w_s} \tag{16}$$

where $\delta(x)$ is the Dirac Delta Function, ensuring energy conservation. With all physical restrictions moved into the Delta Function, the sum could be taken over all light quantum configurations and the the product over all modes. Here a surprising thing happened: This expression (evaluated by replacing the Delta Function by its Fourier integral) is zero, implying a vanishing cross section. And the only way to remedy this, was to introduce a maximal cut-off frequency ω_1, as Pauli and Fierz already had had to do for the electromagnetic mass, that is to limit the product over modes to those modes with frequencies below a certain limit.
This was surprising, since one would assume that high energy photons should not contribute to the cross section: After all their emission was ruled out by energy conservation. Bloch and Nordsieck had had to place an additional limit on the emitted frequency, since their calculation only allowed for soft photons. But now, with energy conservation taken into account, such a restriction should no longer have been necessary. Still, the fact remained that the expression for $S(E)$ contained a product over *all* modes, and if one did not limit this with a cut-off frequency, one obtained the nonsensical result of a vanishing cross section. And things got even worse: If one consequently introduced such a cut-off, the resulting cross section depended on the value of the cut-off and even on the way how it was introduced. Already for the

electromagnetic mass, Pauli and Fierz had implemented the cut-off by assuming the charge of the electron to be spread out. This could be done in different ways, e.g., by assuming a uniformly charged sphere or an exponential decay of the charge density. Explicitly performing the calculation in the latter case, they obtained a finite result:

$$S(E)dE = e^{-\frac{E}{\hbar\omega_1}}\left(\frac{E}{\hbar\omega_1}\right)^C C\frac{dE}{E} \tag{17}$$

where

$$C = \frac{2}{3\pi}\frac{e^2}{\hbar c}\frac{(\mathbf{k}-\mathbf{k}')^2}{m^2c^2} \tag{18}$$

Pauli had obtained this result as early as July 1937, when he communicated it to Weisskopf. But it was a lot harder to interpret than the reassuring Bloch-Nordsieck result that the emission of soft photons simply did not change the cross section. As he wrote to Weisskopf, if the cut-off frequency and the charge distribution actually had a physical interpretation, i.e., if the scattered object was indeed extended, then the result might well be true:

> I also believe that the theory discussed here corresponds to the actual situation for extended test bodies in the quantum range: If somebody were able to precisely determine the very small energy loss of a charged body (difficult in practice, but possible in principle), he would be able to determine the size of the body using this theory.

But as he added in a footnote:

> Only the gods know how it is for actual electrons.

It was clear to him that the whole problem was somehow related to the well-known (high-energy) divergences of QED. This could formally be seen, by expanding $S(E)$ in powers of C:

$$S(E)dE \approx e^{-\frac{E}{\hbar\omega_1}}\frac{dE}{E}(C + C^2\ln(\frac{E}{h\omega_1}) + ...) \tag{19}$$

This expansion was equivalent to the regular perturbation expansion, since C is proportional to $\alpha = \frac{e^2}{\hbar c}$. If one set the cutoff frequency to infinity, the leading order term of the above expansion was simply 1. One thus obtained regular Rutherford scattering. The second term in the perturbation expansion diverged, just as it did, e.g., for the self energy of the electron. But Pauli felt that his results differed from the usual appearance of divergences in QED in a distinct way - their appearance here was only "*formal verständlich*" (Letter to Weisskopf). To be specific, he was apparently somewhat confused concerning the origin of the divergence in this case. He was of the opinion that the regular calculations (i.e., Mott's calculation and the works on *bremsstrahlung*) did not contain an ultraviolet divergence. Since the work of BNPF corrected these works (which were deemed to be correct for hard photons) only through the contribution from soft photons, it was of course very confusing why one should suddenly encounter an ultraviolet divergence, or, equivalently, a dependence on the ultraviolet cutoff. He expressed his confusion concerning this point in his letter to Weisskopf:

> The surprising part is now: *for all those questions to which the old theory could not give a correct answer, the cut-off frequency ω_1 plays an important role, even though one is dealing with short waves (small quanta).*

and shortly afterwards in a letter to Klein (9 August 9 1937):

> I can now show that for charged test bodies with a finite extension a the theory can be improved by removing the [infrared] divergence, but that the point at which the old theory becomes wrong crucially depends on the magnitude of this extension (or on the corresponding "cut-off frequency" $\nu_1 = c/a$). This was surprising since – as opposed to the case of the self-energy – one is dealing with *long* waves. Still, just like the self-energy, this divergence cannot be removed in a relativistically invariant manner for point-like bodies.

Although he found his results strange, Pauli was convinced that they were correct and pointed out, in a new way, the fundamental inadequacy of QED: It was by now well established that the ultraviolet divergences, which Mott had still been blissfully unaware of, would appear in any QED calculation at higher approximations (see for example [59]). Others were more skeptical at first (Pauli to Bohr, 31 August 1937, reports on Heisenberg's initial skepticism). Bloch, who visited Pauli in the winter of 1937/1938, appears to have been the most skeptical, but eventually even he had to accept that the divergence did not result from some false approximation. After this stay he wrote to Kramers (January 24 1938; AHQP, M/f No. 9, Sect. 1-008):

> After a hard battle I finally had to give up my objections to the calculations of Pauli and Fierz; their physical implications are however still quite unclear to me...

To some part, the confusion stemmed from the assumption that the regular, perturbative calculation did not lead to an ultraviolet divergence. This was however, as we have seen, a mistake: Mott had not calculated radiative corrections of the second type, and thus not obtained the ultraviolet divergence. And investigations of the *bremsstrahlung* had considered the process of radiative scattering as a process distinct from radiationless scattering (an approach now, incidentally, invalidated by the result of BNPF that there was no scattering without the emission of soft photons), and had thus not even considered these ultraviolet divergent radiative corrections. Pauli and Fierz, on the other hand, had unwittingly taken the radiative corrections of the second type into account and thus obtained an ultraviolet divergence. This point was somewhat clarified a few months later by an unknown PhD student from Tübingen, as we will see in the next section.[13]

[13] Despite this clarification, Niels Bohr was still wrestling with the separation of the high- and low-energy difficulties almost ten years later, prompting Pauli to have his assistant Res Jost write a paper on radiative corrections to Compton Scattering [36], which, he felt, finally made this separation "clearly visible" (Letter from Pauli to Aage Bohr, 30 March 1947.)

2.2 The Pauli-Fierz divergence stays

We know very little of Edgar Weinmann, the only biographical information comes from a short note at the end of his doctoral thesis [73]. He was born in 1914 in Ehingen, Württemberg, the son of a school headmaster. Starting in 1932, he studied physics and mathematics at the universities of Munich, Stuttgart, and Tübingen, where he wrote his dissertation under the auspices of Professor Werner Braunbek, who had only recently received an associate professorship there, as the successor of Alfred Landé. Braunbek had already worked on the scattering of high energy cosmic ray protons, with [10] and without [9] the emission of radiation. It was most probably in this context that he had become aware of the work of Bloch and Nordsieck, even though he himself had never used QED and radiation theory in his work: His treatment of the emission of radiation had been purely kinematical. We cannot reconstruct how Weinmann got started on a further analysis of the Bloch/Nordsieck procedure, but one can infer the relative isolation in which his work was performed from the fact that he appears to have been entirely unaware of the work of Pauli and Fierz, which had been published already in March (more than three months before Braunbek and Weinmann submitted their joint paper) in the Italian journal *Il Nuovo Cimento*. In the paper, co-authored by Braunbek and Weinmann, which grew out of Weinmann's dissertation [8], Pauli and Fierz are not cited, even though the question which Weinmann pursued was very similar to theirs: How are the different calculations of Rutherford scattering with the emission of radiation related?
Since for Weinmann, this was the subject of his dissertation, he pursued it in a very systematic manner, and indeed managed to reach an understanding of the relation between high- and low-frequency calculations which went beyond the results of Pauli and Fierz. Weinmann began his thesis work, and it seems that he actually learned radiation theory in this manner, by collecting all the high frequency calculations on radiative corrections to scattering and *bremsstrahlung* he could get a hold of,[14] and comparing conventions and approximations to bring them into a unified form, which he could then compare with the results of Bloch and Nordsieck. Since all the high frequency calculations gave additive corrections to the Rutherford cross section of order α, Weinmann also expanded the multiplicative correction of Bloch and Nordsieck

$$\prod_s \frac{1}{n_s!} w_s^{n_s} e^{-w_s} \tag{20}$$

in this small parameter, or rather in w_s, which is proportional to the fine structure constant. However, the Bloch/Nordsieck result contains the factor $w_s^{n_s}$, which is already of a defined order in α, namely of order α^N, where N is the total number of light quanta emitted, which is taken by Bloch and Nordsieck to be infinite in any case. The only way to perform an expansion in α, and in particular to obtain the first-order term in this expansion, was to limit the number of final light quanta. Braunbek and Weinmann thus assumed that there were a finite number of final light quanta with non-negligible energy and an infinity of emitted light quanta with zero energy, which did not affect the total cross section, as shown by the results of Bloch and Nordsieck. The leading term in their expansion is then trivially 1 (no quanta emitted, exponential function approximated as 1). There are two terms of order α, the first with

[14] Mott and Scherzer have already been mentioned, but Weinmann also studied the works of Bethe/Heitler [3] and Sauter [62] who had not encountered the infrared divergence, as they were only interested in the emission of radiation.

one emitted quantum (the exponential again approximated as 1), the second with no emitted quanta, from the expansion of the exponential. These two terms correspond to what I have called radiative corrections of the first and second type.
Weinmann realized that the first term with the emission of one light quantum corresponded exactly to the results of Mott and the other high frequency calculations, infrared divergence and all. The second term, which is a correction to elastic scattering, since no light quanta (with finite energy) are emitted, however, also turned out to be infrared divergent, and the two infrared divergences precisely cancelled. At the same time however, this second term which was not restricted by energy conservation, was also ultraviolet divergent, and thus responsible for the divergence encountered by Pauli and Fierz.
Weinmann's perturbative reformulation of the results of Bloch and Nordsieck[15] thus established the following origin for the ultraviolet divergence: The corrections to elastic scattering contained integrals up to the cut-off, while the corrections from inelastic scattering only contained integrals up to the maximal frequency allowed by energy conservation.[16] Braunbek and Weinmann gave no hints on how to calculate this second term perturbatively, without doing the non-perturbative Bloch/Nordsieck calculation and only then expanding the final result. They apparently did not yet realize that Mott would have obtained the same result as them if he had properly calculated the radiative corrections of the second type. Others were however soon to notice the implications of Weinmann's results. Weinmann himself played no further part in the history of QED: He became a schoolteacher in the black forest.[17]

2.3 The Pauli-Fierz divergence removed

In the meantime, Bloch was still pondering over the physical significance of the results of Pauli and Fierz. As he had written to Kramers, he now believed that these results were correct. His original argument against them, can be reconstructed from a letter to Bohr (12 December 1937): In the classical theory, there can be no influence on the scattering process from radiation modes whose frequencies are smaller than the inverse scattering time. There is thus an automatic ultraviolet cutoff. Pauli convinced him that this argument was no longer valid in the quantum theory, and Bloch consequently remarked to Bohr:

> I now really see no more a priori reasons to doubt Pauli's result and now also believe that one needs to interpret it in the following manner: Besides the features well-known from classical theory, in the quantum theory the finiteness of the electron radius also appears in other places, and Pauli's result is an illustration of this fact. (AHQP, BSC, M/f No. 17)

But even though the classical argument against the result had been removed, it was still not clear how to interpret it. One month later, Bloch wrote to Bohr (January 24, 1938):

[15] Incidentally, in his 1936 manuscript mentioned earlier, Heisenberg had attempted to solve the infrared divergence in a similar manner.

[16] This explains why Bloch and Nordsieck had ended up with no corrections at all - their calculation can be taken to imply taking into account all possible emitted light quanta, not just those energetically allowed, as discussed above. The two divergent integrals then exactly cancel, both at the high- and low-frequency ends, leaving only the leading order term.

[17] Many thanks to Karin Arnold from the Tübingen university library for helping me track down Weinmann's dissertation and to Dr. Michael Wischnath from the Tübingen university archives for helping me find out what became of Weinmann after his PhD.

> I do not want to tire you with speculations concerning the result of Pauli and Fierz; indeed its meaning is still very obscure, but I hope to be able to think about it some more in due time, and to let you know my thoughts when that time has come. (AHQP, BSC, M/f No. 17)

At some point within the next year, Bloch must then have hit upon the idea that the Pauli-Fierz divergence, although technically correct, was simply an artefact due to the non-relativistic approximation. Indeed, the reader may well have wondered about how the Pauli-Fierz divergence is to be interpreted in a modern understanding. After all, Pauli and Fierz had performed a full mass renormalization: They had eliminated the Coulomb self-energy (which in the non-relativistic theory is not a contribution to the mass) and the transverse electromagnetic mass. Why then did they still encounter an ultraviolet divergence? The reason is that the non-relativistic theory is, in a way, non-renormalizable. It should be noted, that the theory considered by Pauli and Fierz is not what is today called non-relativistic QED (NRQED). NRQED is an effective field theory, in which both electron and photon energies are cut off around a value of mc^2. The theory of Pauli and Fierz however is a somewhat inconsistent non-relativistic theory, in that the electron is treated entirely non-relativistically (with a Schrödinger equation), while it interacts with a fully relativistic radiation field. In this theory, logarithmic ultraviolet divergences will appear, even after mass renormalization (which is the only renormalization that is necessary, since there are no positrons, and hence no vacuum polarization or charge renormalization). This can also be seen in Bethe's much later non-relativistic (in the same sense as with Pauli and Fierz) calculation of the Lamb shift of 1947 [4], where there is also a logarithmic divergence which needs to be removed by an explicit cut-off.
Bloch was thus, in hindsight, entirely right in assuming that the Pauli-Fierz divergence should vanish in a relativistic calculation. A simple argument for this view was later presented in Dancoff's paper: The emission of high frequency light quanta causes a noticeable recoil momentum and thus an increase in the effective, relativistic mass of the electron. This momentum is however entirely in the direction of emission - the transversal momentum is unchanged by the emission process. The coupling to the electromagnetic field is proportional to the transversal velocity. An increase in the relativistic mass, with no parallel increase in the transversal momentum, means an effective reduction of the transversal velocity, and thus a suppression of the coupling. And since the divergence encountered by Pauli and Fierz was only logarithmic any such suppression should be sufficient to make the cross section finite. In 1939, Bloch, together with Oppenheimer, thus set himself to calculating the radiative corrections to Rutherford Scattering in a relativistic framework, using both the scalar Klein-Gordon wave equation (recently brought back into fashion by Pauli and Weisskopf [58]) and Dirac's relativistic electron wave equation. There was however a major problem: How could one generalize the BNPF procedure for solving the radiation problem to a fully relativistic setting?
It is quite possible that Bloch and Oppenheimer tried their hand at this, but failed. But Braunbek and Weinmann had implicitly shown how this calculation could be performed without the BNPF procedure. If one solved both the radiation problem and the scattering perturbatively, the infrared divergences would cancel at each order. There was thus not really the need for a full solution of the radiation problem, as BNPF had thought. Bloch and Oppenheimer could instead construct approximate solutions of the radiation problem and then perturbatively calculate scattering transitions between these approximate solutions. Such an approximate solution for the radiation problem could be obtained with regular perturbation theory:

$$\psi = \psi_{\text{free}} + e\sum_s \psi_s + e^2\left(\psi_0 + \sum_{s,t}\psi_{st}\right) + \mathcal{O}\left(e^3\right) \tag{21}$$

This looks a lot like Mott's expansion, equation 2, however the expansion is now around a free particle (i.e., without interaction even with radiation), with wave function ψ_{free} and not around F_E, which was already a solution of the scattering problem.
As a brief, but essential aside, it should be noted here that Dancoff later dubbed the term ψ_0 in the above expansion "renormalization term." At zeroth order in the perturbation expansion, ψ_{free} is normalized to one. This is spoiled by the terms ψ_s, which do not interfere with ψ_{free}. Taking just the first two terms in the above expansion into account, the wave function is, since there are no interference terms, normalized to:

$$|\psi|^2 = |\psi_{\text{free}}|^2 + e^2\sum_s |\psi_s|^2 \tag{22}$$

which is larger than one, since the free wave function is already normalized to one. The term ψ_0, however, does interfere with ψ_{free}, since it describes a state with the same number of light quanta (none). The interference terms are also of order e^2 and can cancel the term above which had spoiled the normalization, thus re-normalizing the total wave function. This has nothing to do with the concepts of mass and charge renormalization. It is also not the same as the later notion of wave function renormalization: In modern parlance, this refers to the renormalization of a second-quantized operator, while Dancoff was merely considering a probability amplitude here. This reading is confirmed by Schwinger [66]:

> Incidentally, on reading Dancoff's paper recently, I was somewhat astonished to see the word *renormalization*. But the context here was not mass or charge renormalization; it was the additional terms that maintain the normalization of the state vector. (Emphasis in the original)

We will return to this decisive linguistic point later on.
In analogy to the BNPF approach, the scattering by an external potential V only enters these calculations as a further perturbation, effecting transitions between solutions of the radiation problem. The scattering matrix element, for the transition from an initial state ψ^i (momentum $\mathbf{k}$) to a final state ψ^f (momentum $\mathbf{p}$) is then calculated as

$$\langle\psi^i \mid V \mid \psi^f\rangle = \langle\psi^i_{\text{free}} \mid V \mid \psi^f_{\text{free}}\rangle + \alpha[\langle\psi^i_s \mid V \mid \psi^f_s\rangle + \langle\psi^i_0 \mid V \mid \psi^f_{\text{free}}\rangle + \langle\psi^i_{\text{free}} \mid V \mid \psi^f_0\rangle] + \mathcal{O}(\alpha^2) \tag{23}$$

since in this formulation (as opposed to the BNPF formulation) the scattering potential can not create or annihilate light quanta, i.e., it can only cause transitions between states with an identical set of light quanta. Calculating these transition matrix elements is then simple: The ψ_s and ψ_0 are themselves expressed via solutions of the free electron wave equation, and thus all these calculations are equivalent to the Rutherford scattering of a free electron. The challenging part was to calculate the expansion of the initial and final wave functions, equation 21. Here one met an additional difficulty of the relativistic calculation, besides the non-availability of a generalization of the BNPF procedure: Both the Klein-Gordon and the

Dirac wave functions could not simply be taken as one-particle probability amplitudes.
Things were simple if one only considered the positive energy solutions of these equations - one then did not have to deal with antimatter and particle creation and annihilation. It was in this simplified scenario - only positive-energy solutions, Klein-Gordon and Dirac equations treated as one-particle equations - that Bloch, together with Oppenheimer, tested his hypothesis that the Pauli-Fierz divergence would disappear in a relativistic calculation. And indeed: not only did the cancellation of the infrared divergence also work in relativistic quantum mechanics, they also found that the contributions to the cross-section from the high-frequency light quanta no longer diverged.
Again this result looks surprising at first: I have explained why the Pauli-Fierz divergence disappears in the relativistic theory, but shouldn't the usual ultraviolet divergences from the electromagnetic mass reappear? After all, Bloch and Oppenheimer (and Weinmann, for that matter) had not employed the BNPF procedure, and hence had not performed a mass renormalization. There are two answers to this question: On the one hand, this ultraviolet divergence did reappear, but only when taking into account negative energy states, as we will see in a moment, when discussing Dancoff's calculation. On the other hand, Bloch and Oppenheimer had dropped some divergent terms: The unperturbed scattering cross section is proportional not only to the squared scattering matrix element (the dynamics), with which we have been primarily concerned so far, but also to the density of final states (the kinematics). To obtain the order α corrections to the cross section, one thus not only has to consider the corrections to the squared matrix element, but also those to the density of states. And since the electromagnetic self-energy diverges at order α, so should the density of states. Bloch and Oppenheimer ignored this divergence, and only took the zeroth order value for the density of states.
This is a non-trivial step: The problematic divergences of QED habitually appeared in the order α corrections in perturbation theory. In most practical calculations only the leading order terms were calculated, which generally provided decent agreement with experiment. Braunbek and Weinmann had now shown that these problematic higher-order corrections had to be taken into account in order to cancel the infrared divergence - at the cost, however, of introducing the ultraviolet divergences. Bloch and Oppenheimer's idea was now that one could drop the (ultraviolet divergent) higher order contributions to the density of final states (as one always did) and only keep the higher order corrections to the scattering matrix element. And indeed, the latter were sufficient to cancel the infrared divergence in the resulting cross section. They had thus performed a separation of the higher order effects of radiation theory into two categories: The ultraviolet divergent terms that effected the final density of states and could safely be dropped, and the terms in the scattering matrix elements that one needed to cancel the infrared divergence. In a way this simple trick is equivalent to BNPF dropping the electromagnetic mass term given in equation 9 - all that this operator does in the non-relativistic formalism is provide a divergent correction to the density of states. This is due to the fact that the non-relativistic self-energy terms, the Coulomb self-interaction and the electromagnetic mass term (equation 9), are both diagonal in the momentum representation, i.e., they can not cause transitions between incoming and outgoing plane waves. They therefore do not contribute the scattering matrix element, and only appear in the scattering cross section through the density of final states.
This is no longer the case, when one includes negative energy states, as I will show in a moment. Bloch and Oppenheimer, however, had convinced themselves that the Pauli-Fierz divergence vanished in the relativistic theory. It now only remained to fill out the details of

the relativistic calculation, by including the negative energy states. Apparently assuming that this would go through without major problems, Oppenheimer assigned this task to his PhD student Sidney Dancoff.

3 Dancoff's divergence

Sidney Dancoff was born in Philadelphia in 1913, son of Morris and Ida Glazer Dancoff. Morris Dancoff had emigrated from Russia in 1905/06, where he had grown up in a Ukrainian shtetl. The family moved to Pittsburgh in the early 1920s, where Sidney was the only one of his siblings to go to college. After receiving a bachelor's degree (1934) from Carnegie Tech and a master's degree (1936) from the University of Pittsburgh, he moved to California, where he became Oppenheimer's PhD student at UC Berkeley [43].[18] His thesis work consisted of rather unconnected topics, and was consequently handed in in 1939 as a cumulative dissertation[19] with the rather nondescript title "Three Problems in Quantum Mechanics". The three problems concerned (a) the energy levels of the radioactive nucleus He^5, (b) internal conversion, i.e., the ionization of an atom by the electromagnetic decay of its nucleus, and, finally, (c) the task given to him by Bloch and Oppenheimer concerning the relativistic radiative corrections to scattering.

It is now finally time to delve into the details of Dancoff's calculation. His task was to include the negative energy states into the relativistic scattering calculation. Since the negative energy states were associated with anti-particles and pair creation, both for the case of a Dirac (hole theory) and Klein-Gordon (Pauli-Weisskopf theory) particle, one would at first glance assume that this would necessitate the use of second-quantized quantum field theory, or at the very least many-body quantum mechanics. However, already in the first paper on hole theory [18], Dirac had shown that it was possible, in some cases, to use the results of the one-particle calculation, even when dealing with intermediate negative energy states. For the simple case considered by Dirac, Compton scattering, the one-particle calculation with unoccupied negative energy states was in fact equivalent to the full hole-theoretical calculation, if one simply disallowed final negative energy states.[20] This was no longer the case for more complicated processes: Of course it was no longer true for actual pair creation processes, which could not be described in the one-particle theory, but even such a process could be viewed as a collision between the photon and a single electron in the Dirac Sea - the momentum of the final positron was then simply the momentum of the initial electron in the single-particle picture, or rather of the hole it left behind.

But things really got difficult in higher order approximations, where one had to deal with virtual pair creation and the emission and reabsorption of virtual photons. This was the kind of stuff Dancoff had to deal with in tackling radiative corrections up to order α. And surprisingly, despite the huge triumph of hole theory with the discovery of the positron in 1932, there were not too many precedents for such calculations. This is mainly due to the fact that there was no empirical motivation for higher-order calculations: Just as for the scattering problem treated

[18] Apparently he was referred to Oppenheimer by his Pittsburgh professor David Rittenhouse Inglis, with whom he learned nuclear physics and relativity (Oral History Interview with Inglis by Steve Heims, 9 May 1977).

[19] This was apparently not that uncommon with Oppenheimer—Philip Morrison also handed in a cumulative dissertation with Oppenheimer one year later, as Oppenheimer felt that it was not worth the bother to write a whole new thesis (Oral History Interview with Morrison by Charles Weiner, 7 February 1967).

[20] A nice proof of this statement was provided by Wentzel in his textbook on quantum field theory [80, 183-185].

by Dancoff, most other processes involving relativistic electrons, such as Compton scattering or pair production, were also nicely described by leading-order calculations, using, where necessary, a single-particle description instead of actual hole theory. There had in fact been only two major investigations of higher-order effects in hole theory, both of them not dealing directly with experimental results.

The first of these investigations was concerned with the question of vacuum polarization and was really an investigation into the tenability of the physical picture of the Dirac Sea. When first postulating hole theory, Dirac had stated that the infinite sea of negative energy electrons would be unobservable due to its uniformity. But after the discovery of the positron, the question was soon raised what happens when a local electromagnetic field disturbs this uniformity, and it quickly turned out that even if the uniform charge distribution of the Dirac Sea was subtracted, an infinite "vacuum polarization" was still to be expected (see, e.g., a letter from Rudolf Peierls to Pauli from 17 July 1933). Several studies of this disturbing feature of hole theory had been performed in the years 1934-1936. It was shown, most importantly by Heisenberg [25] and Weisskopf [76], that the effects of the vacuum polarization could always be subtracted in a consistent and relativistically invariant manner, as long as the field causing the polarization could be treated classically. This was the case for the external scattering potential in Dancoff's calculation, and Dancoff well aware of the works on vacuum polarization, treated these divergent terms correctly in his paper.

Dancoff mentioned these vacuum polarization terms only briefly in a footnote towards the end of the paper. There is some confusion in the secondary literature concerning this point. Schweber's reading is slightly unclear: In this footnote Dancoff also refers to "Coulomb interaction", and one can read Schweber (page 614, footnote 13) as implying that Dancoff is here talking about the terms he did in fact neglect and casually denying their relevance. However, the Coulomb interaction Dancoff was referring to there is the Coulomb interaction with the virtual pairs created by the external scattering potential, i.e., with the polarized vacuum. Mehra goes even further, explicitly claiming that Dancoff's mistake consisted in not taking into account the vacuum polarization terms [44, 60]. We will see later on how this specific misunderstanding arose along with the general reassessment of Dancoff's paper.

The second major investigation of higher-order effects in hole theory consisted of a study by Weisskopf in 1934 (another Pauli task), as to whether the inclusion of hole-theoretic effects had any effects on the infinities of QED, in particular on the self-energy of the electron [75]. This work is even closer to Dancoff's, as it is the only one to use a quantized electromagnetic field (and thus virtual photons) in the context of hole theory. Ironically, Weisskopf's paper is also well-known for containing a mistake. In Weisskopf's case, however, this really was a silly calculational error, which needs only a sentence to explain, instead of a whole paper: In performing the substitution $\mathbf{k} \rightarrow -\mathbf{k}$ in an integral, he had erroneously assumed that the magnitude $|\mathbf{k}|$ would also change sign. This error was consequently pointed out to Weisskopf by Wendell Furry, and promptly corrected [74], leading Weisskopf to the result that taking into account hole theory significantly changed the one-particle result, as the self-energy now only diverged logarithmically instead of quadratically.

It is, however, for a different reason that Weisskopf's paper is central to our discussion: As it is the only comparable investigation into higher order effects in hole theory, it is the only standard by which we can assess the wrongness of Dancoff's work in the context of the theoretical physics of its day. This is, however, further complicated by a, at first sight merely technical, fact: Weisskopf's paper used second-quantized many-body theory. Dancoff's paper is very brief, and basically only gives the results, so we can not say for sure whether Dancoff went

through the procedure of calculating in the full second-quantized theory. His results, however, give the impression of having been calculated basically in a one-particle framework, with only slight modifications from hole theory. For even though the full equivalence between one-particle and hole theory calculations no longer holds when taking into account radiative corrections, a fairly simple relation can be established between the two. As an example let us consider the term $\langle \psi_0^i \mid V \mid \psi_{\text{free}}^f \rangle$ in the transition matrix element above, equation 23. In a one-particle theory, ψ_0^i is generated by the incoming electron (of momentum $\mathbf{k}$) first emitting and then re-absorbing a photon in any mode s with momentum $\mathbf{q}_s$. The intermediate state of the electron has a defined momentum $(\mathbf{k}-\mathbf{q}_s)$, but can either have positive or negative energy - since it is the difficulty of the negative energy states we are interested in, we will only be considering the second option, which we denote by $(\mathbf{k}-\mathbf{q}_s)^-$. Back in a positive energy state after the virtual emission and absorption of the photon, the electron is then scattered by V into its final state, ψ_{free}^f, with momentum $\mathbf{p}$. The full transition matrix element is thus proportional to the three matrix elements of the intermediate processes of emission $\mathscr{E}$, absorption $\mathscr{A}$, and scattering:

$$\langle \psi_0^i \mid V \mid \psi_{\text{free}}^f \rangle \sim \sum_s \frac{\langle \mathbf{k} \mid \mathscr{E} \mid (\mathbf{k}-\mathbf{q}_s)^- \rangle \langle (\mathbf{k}-\mathbf{q}_s)^- \mid \mathscr{A} \mid \mathbf{k} \rangle}{[E_k - (-E_{k-q_s} + c|\mathbf{q_s}|)]^2} \langle \mathbf{k} \mid V \mid \mathbf{p} \rangle \tag{24}$$

where the energy denominator in perturbation theory is given by the squared difference between the energy of the initial state (E_k, the energy of an electron with momentum $\mathbf{k}$) and the energy of the intermediate state ($-E_{k-q_s}$, the energy of a negative energy electron with momentum $(\mathbf{k}-\mathbf{q}_s)$, plus the energy of a photon in the mode s).
What does the corresponding process look like in hole theory? Consider the following: An electron in the Dirac sea, with negative energy and momentum $\mathbf{k}+\mathbf{q}_s$, i.e., in the state $(\mathbf{k}+\mathbf{q}_s)^-$, spontaneously emits a photon in the mode s going over to the positive energy state with momentum $\mathbf{k}$. Since there is already an (incident) electron with momentum $\mathbf{k}$ present, this second electron must have opposite spin. The incident electron then absorbs the virtual photon, going into the (now vacant) state $(\mathbf{k}+\mathbf{q}_s)^-$. The second electron, that was initially in that state, is then scattered by the external potential into the final state $\mathbf{p}$, and is identified with the outgoing electron. Combining these three individual, one-electron transitions into a transition matrix element for the whole process, we obtain:

$$\langle \psi_0^i \mid V \mid \psi_{\text{free}}^f \rangle \sim \sum_s \frac{\langle (\mathbf{k}+\mathbf{q}_s)^- \mid \mathscr{E} \mid \mathbf{k} \rangle \langle \mathbf{k} \mid \mathscr{A} \mid (\mathbf{k}+\mathbf{q}_s)^- \rangle}{[(E_k - E_{k+q_s}) - (E_k + E_k + c|\mathbf{q_s}|)]^2} \langle \mathbf{k} \mid V \mid \mathbf{p} \rangle \tag{25}$$

where the energy denominator again gives the squared difference between the initial (incident electron with momentum $\mathbf{k}$ and Dirac sea electron with momentum $\mathbf{k}+\mathbf{q}_s$) and the intermediate (two positive energy electrons with momentum $\mathbf{k}$ and a photon in the mode s) energies, where of course the energies of all the Dirac sea electrons which are not involved in the process are the same in the initial and intermediate states and therefore cancel. Since for each mode s with momentum $\mathbf{q}_s$ there is also a corresponding mode with momentum $-\mathbf{q}_s$, one can simply make the replacement[21] $\mathbf{q}_s \rightarrow -\mathbf{q}_s$ in the expression above, if one also exchanges the matrix elements for emission and absorption.[22] One then obtains:

[21] When working with a continuous spectrum, this corresponds to a substitution of the integration variable. Incidentally, this is precisely the same substitution, in the same context, which Weisskopf botched in 1934.

[22] As can be seen from equation 1 the two corresponding operators differ only in the sign of $\mathbf{q}_s$ and, of course,

$$\langle \psi_0^i \mid V \mid \psi_{\text{free}}^f \rangle \sim \sum_s \frac{\langle \mathbf{k} \mid \mathscr{E} \mid (\mathbf{k}-\mathbf{q}_s)^- \rangle \langle (\mathbf{k}-\mathbf{q}_s)^- \mid \mathscr{A} \mid \mathbf{k} \rangle}{[-E_{k-q_s} - E_k - c|\mathbf{q_s}|]^2} \langle \mathbf{k} \mid V \mid \mathbf{p} \rangle \tag{26}$$

The transition matrix element in hole theory thus differs from that in the one-particle theory (equation 24) only through a sign in the energy denominator. Dancoff could thus perform his entire calculation in a one-particle framework, with intermediate negative energy states, changing only the sign in the energy denominator. The above heuristic derivation is of course somewhat sloppy: Besides the fact that a second process, with the two electrons exchanged, needs to be taken into account, to account for both possible spin orientations, this is of course no proof that such a modified equivalence relation exists for all intermediate configurations. Dancoff did not even offer such a heuristic proof, and when Tomonaga and his collaborators redid Dancoff's calculation in 1947/1948 as an explicit many-particle problem, they consequently chided him:

> Thus the final result is in this respect identical with the one-body treatment of Dancoff [...] which is not strictly justified before its equivalence with [the] many-body treatment is verified. [34, 329]

But they could find no actual mistake in this part of Dancoff's calculation. It is quite possible that Dancoff had in fact verified this equivalence, but felt no need to expand on it, given his unsatisfactory final results, to which I will turn in a moment. If this is the case, his approach was indeed quite ingenious - Oppenheimer certainly was impressed with Dancoff's calculation, and later wrote of it in a letter of recommendation (13 May 1940):

> He [Dancoff] gave the most complete solution we have, going far beyond that of Pauli and of Bloch and Nordsieck, on the quantum theory of the the effect of radiation reaction in collisions, that was a difficult and expert piece of work. [51, 211-212]

In any case, Dancoff never explicitly said that he was performing a one-particle calculation - Tomonaga and his collaborators only inferred this from the form in which he stated his results, and I share their interpretation. In his prose, however, Dancoff took care always to use a hole-theoretical picture. The process I have discussed above, is thus described by him as having an electron-positron pair in the intermediate state.[23]
No matter how his actual calculations looked, this part of Dancoff's work[24] went through just fine and no new divergences appeared. The calculation was, however, still incomplete.[25] A

in one including a creation and the other an annihilation operator. This latter difference is however irrelevant, since the relevant matrix element is simply 1 in this case, for both operators.

[23] It should be noted that, in the modern understanding, the above process is part of a Feynman diagram with no virtual pairs - it is simply viewed as the emission and reabsorption of a photon by an electron. No specification needs to be made about whether the intermediate state of the electron has positive or negative energy. The reformulation of QED thus led in some cases to a new understanding of which virtual particles are present in the intermediate states. This is demonstrated nicely in Aramaki's paper, where the relevant Feynman diagram is related to the different intermediate states considered by Dancoff.

[24] His B terms, the terms already calculated by Bloch and Oppenheimer being the A terms.

[25] Empty spaces fill me up with holes.

further type of process had to be considered: Here (adopting the simpler one-particle language) the incident electron goes into a negative energy state due to the interaction with its own radiation field, and is then scattered by the external potential back into a positive energy state, or the incident electron is scattered by the potential into a negative energy state and then lifted back into the final positive energy state by its radiation field. The essential characteristic of these additional terms (Dancoff's *C* terms) is that the external potential scatters the electron from a negative to a positive energy state, or vice versa. They are thus characterized by Dancoff (in hole theory language) as "involving pair production and annihilation by the scattering potential."
It was in calculating these terms that divergences entered Dancoff's calculations. He obtained the "fortuitous" results that the divergence of the radiative corrections depended essentially on the form of the external scattering potential: They diverged for Dirac particles, no matter what the potential, while they diverged for Pauli-Weisskopf particles only for (Lorentz) scalar potentials, but not for (Four-)Vector potentials. This was at first viewed as a further example of the intrinsic flaws of quantum electrodynamics, perhaps even their worst manifestation to date. As Oppenheimer and Bethe would write in 1946:

> His [Dancoff's] result was that these terms do not in general converge and that relativistic corrections could not be counted on to alter qualitatively the Pauli-Fierz result. The details of Dancoff's result further suggest that the theory made no sense at all. Thus the precise character of the infinite terms depends on whether the scattering potential is a four-vector or a world scalar and whether the spin of the scattered particle is 0 or $\frac{1}{2}$. It is not possible to believe that in a problem involving only charges moving with low velocity these deductions can have any relation to reality. [5, 452]

Only two years later, Dancoff's result was considered incorrect, after being re-evaluated not only by the Tomonaga group, but also by students of Oppenheimer's. It must be stressed that the results were not just considered wrong, because Dancoff had not applied the new renormalization techniques and therefore encountered an infinite result. Applying this standard, almost every paper on quantum electrodynamics from the 1930s would have to be considered wrong. No, Dancoff's mistake is supposed to have gone beyond the general wrongness of contemporary QED. I claim that there are three distinct ways in which Dancoff's paper might be or has been considered wrong:

1. Dancoff's paper was correct, given the goal he had set himself and the calculational methods at his disposal; however, it ignored certain aspects, which were irrelevant at the time, but later became important to translate Dancoff's results into renormalized QED.

2. Dancoff's paper was incorrect, given the goal he had set himself and the calculational methods at his disposal; however, the mistake he made did not significantly affect his conclusions.

3. Dancoff's paper was incorrect, given the goal he had set himself and the calculational methods at his disposal; the mistake he made significantly affected his conclusions.

I will investigate in the following sections, in which sense Dancoff's paper can be considered wrong.

3.1 Dancoff's divergence appears

It is unavoidable, when analyzing the wrongness of Dancoff's paper, in the above senses, to be anachronistic and interpret his calculations in the light of a modern understanding of quantum electrodynamics. I will thus take a look at the radiative corrections to scattering from the standpoint of renormalized QED, avoiding, however, the language of Feynman diagrams, which would only obscure the relation between modern-day QFT and the QED of the 1930s. From the standpoint of mass renormalized QED, when calculating the scattering transition matrix elements, one needs to add an additional term to the Hamiltonian, the mass counter-term $-\delta m$. I will later on discuss in somewhat more detail how the precise form of this term is determined by demanding that the self-energy of a free electron take on the finite values mc^2, where m is the observed mass of the electron. Once the counter-term has been derived (up to a certain order in perturbation theory), it plays the role of a regular (albeit divergent) term in the Hamiltonian for the calculation of physical processes, such as the scattering process considered by Dancoff. If the theory is renormalizable, i.e., if all divergences can be absorbed in a renormalization of the mass, the infinities from the mass counter-term should exactly cancel (up to the given order in perturbation theory) the infinities encountered in an unrenormalized calculation, such as Dancoff's, leading to finite predictions for observables such as cross sections.

What is the effect of the mass counter-term on the calculation of the scattering? I have already mentioned that in the non-relativistic theory of Pauli and Fierz, with the Schrödinger Hamiltonian for the electron, the mass appears only in the kinetic energy term $\mathbf{p}^2/2m$, which is diagonal in the momentum representation and thus does not cause any transitions in a perturbative calculation. The divergent mass only appears in the infinite phase space density of final states, which Bloch, Oppenheimer and Dancoff had dropped. Things are different in a relativistic theory, both for Klein-Gordon and Dirac particles. Consider the case of a Dirac electron. The mass term in the Hamiltonian is of the form βmc^2, or $\gamma_0 mc^2$ in modern notation. Although diagonal in momentum space, just like the non-relativistic term, it has a non-trivial Dirac matrix structure, and can thus (in a one-particle language) induce transitions between states with equal momentum but with an opposite sign of the energy.[26] One would thus expect in an unrenormalized calculation to encounter divergent terms involving transitions, induced by the particle's own radiation field, from positive to negative energy states (or vice versa) with the same momentum. These divergent terms could then be cancelled by the corresponding terms induced by the mass counter-term. And indeed, all of Dancoff's divergent terms include just such a transition between states of equal momentum but with a different sign for the energy.[27]

The transitions calculated by Dancoff can, however, not be identified as the effect of the di-

[26] A similar statement can be made for the Klein-Gordon mass term. For simplicity, I will focus on the Dirac electron in the following.

[27] It is for this reason that Dancoff obtained the strange result that the appearance of the divergences depended on the transformation properties of the scattering potential. In the divergent terms, the electron goes from $\mathbf{k}$ to $\mathbf{k}^-$, i.e., makes a transition of the type described above, and is then scattered from $\mathbf{k}^-$ to $\mathbf{p}$, or is scattered from $\mathbf{k}$ to $\mathbf{p}^-$ and then makes a radiative transition to $\mathbf{p}$. In any case, it is scattered by the external potential into a state with an energy equal in magnitude but opposite in sign (since energy is conserved in the overall process, we have $E_k = E_p$). The scattering matrix element for such transitions vanishes for a scalar particle scattered by a four-vector potential. Hence, all divergent terms vanish for a Klein-Gordon particle in such a potential, and the resulting cross section is finite. One has in this case an "accidental" disappearance of the divergences without renormalization.

vergent, electromagnetic mass. And we have already, at the end of section 1.3.1, seen why this is not the case: Dancoff had neglected the divergence due to the electron's Coulomb self-interaction. But isn't the Coulomb self-interaction just a divergent constant? Was that not precisely the reason why it had been so casually dropped at the very beginning of the papers of Bloch/Nordsieck and Pauli/Fierz? The answer is that this is no longer true in hole theory. As opposed to the radiative transitions considered above, where the switch to hole theory simply involves changing a sign in the energy denominator, the Coulomb self-interaction is qualitatively changed in hole theory, from an ignorable infinite constant to a divergent term generating non-trivial transitions.

Why did Dancoff not include this term in his calculations? It needs to be stressed that including the Coulomb self-interaction would not have cancelled the divergences encountered by Dancoff.[28] Its inclusion would only have allowed him to combine all the divergent terms into a mass term, which could then have been neatly cancelled by a mass counter-term. There is no reason to believe that Dancoff actually contemplated this possibility, and only then realized that it was not feasible, i.e. that he considered the use of mass renormalization techniques only to find them inadequate. I will attempt to further prove this point in the next sections. In this case, adding the Coulomb self-interaction would not have significantly changed Dancoff's conclusions, it would only have added a further, uninterpretable divergent term. If one assumes that Dancoff was aware of this, and simply felt no need to mention it in his paper, his work could be considered wrong in the first (and weakest) sense given above.

There are, however, good reasons to assume that Dancoff in fact simply overlooked the transitions caused by the Coulomb self-interaction. To the best of my knowledge, they had appeared in the literature only once, and there only implicitly and in passing, in Weisskopf's 1934 calculation of the electron self energy. As I have already mentioned, Weisskopf's calculation was an explicit many-body calculation, using the second-quantized electron wave function

$$\psi(\mathbf{r}) = \sum_q a_q \varphi_q(\mathbf{r}) \tag{27}$$

where the sum goes over all energy eigenstate solutions of the free Dirac equation φ_q, both with positive and with negative energies, and the a_q are the corresponding electron annihilation operators. The creation operators only show up in the hermitian conjugate wave function $\psi^\dagger$ - this hole-theoretic second quantization is thus different from modern-day second quantization of the Dirac electron using positron creation and annihilation operators.[29] To calculate the electrostatic part of the self-energy E^S in hole theory, Weisskopf had set:

$$E^S = \frac{1}{2}\int\int \frac{[\rho(\mathbf{r}) - \tilde{\rho}(\mathbf{r})][\rho(\mathbf{r}') - \tilde{\rho}(\mathbf{r}')]}{|\mathbf{r} - \mathbf{r}'|} d\mathbf{r}\ d\mathbf{r}' \tag{28}$$

where ρ is the second-quantized electron charge density $e\psi^\dagger\psi$, and $\tilde{\rho}$ is the charge density of the hole theoretic vacuum:

$$\tilde{\rho}(\mathbf{r}) = e\sum_{\bar{q}} \varphi^\dagger_{\bar{q}}(\mathbf{r})\varphi_{\bar{q}}(\mathbf{r}) \tag{29}$$

[28] Schweber (p. 90) is mistaken in his assumption that they would have.

[29] The two quantization methods are related by a Bogoliubov transformation.

where the sum is over all the negative energy solutions of the free Dirac equation, i.e., over all those states which are occupied in the hole-theoretic vacuum. After introducing the quantity

$$A_{qrst} = e^2 \int\int \frac{\left\{\varphi_q^\dagger(\mathbf{r})\varphi_r(\mathbf{r})\right\}\left\{\varphi_s^\dagger(\mathbf{r}')\varphi_t(\mathbf{r}')\right\}}{|\mathbf{r}-\mathbf{r}'|} d\mathbf{r}\ d\mathbf{r}' \tag{30}$$

Weisskopf could then write the electrostatic self-energy operator as

$$2E^S = \sum_{qrst} A_{qrst} a_q^\dagger a_r a_s^\dagger a_t - 2\sum_{qr} a_q^\dagger a_r \sum_{\bar{s}} A_{qr\bar{s}\bar{s}} + \sum_{\bar{s}\bar{t}} A_{\bar{s}\bar{s}\bar{t}\bar{t}} \tag{31}$$

Since he was interested in the self-energy of the electron in a first perturbative approximation, Weisskopf went directly to the diagonal elements of this operator, and arrived at the result that the electrostatic self-energy is only logarithmic in hole theory. The operator E^S, however, also has non-diagonal elements. The relevant transitions discussed above, between negative and positive energy states with equal momentum in an effective one-particle theory, correspond to the creation or annihilation of an electron-positron pair without the emission or absorption of radiation, as this is nothing else than the transition of a negative energy electron in the Dirac sea to a positive energy state with equal momentum (or vice versa). And, indeed, in hole theory such processes are induced by the off-diagonal elements of the first summand in the operator E^S.[30] This was not addressed by Weisskopf, nor have I found any references to this fact in the literature of the 1930s. To realize it, Dancoff would have had to go beyond his effective one-particle calculation and carefully study Weisskopf's paper.

There is in fact a historical precedent for Dancoff's mistake, directly related to Weisskopf's paper, as pointed out by Schweber: Wendell Furry had only spotted Weisskopf's original sign error, since he was at the same time performing a similar calculation of the electron self-energy in hole theory, together with Franklin Carlson [64, 69-70]. Carlson and Furry spotted Weisskopf's blunder, but had themselves made a much greater mistake, a mistake that prefigures Dancoff's mistake of five years later: They had recalculated only the transverse self-energy, simply assuming that the electrostatic Coulomb self-energy would be unchanged in hole theory. But even after Weisskopf's paper had in turn corrected this erroneous assumption, the physical reasons for the modification of the Coulomb interaction in hole theory were still relatively unclear. A physical interpretation of the hole-theoretic modification of the Coulomb self-interaction (which was entirely lacking in Weisskopf's 1934 paper) was presented by Weisskopf in 1939, shortly after Dancoff published his results (he interpreted the reduction of the divergence in hole theory as the result of the smearing of the electric charge of the electron from the change it causes in the distribution of the vacuum electrons), but also here Weisskopf only referred to the self-energy, not to the radiationless creation of virtual pairs [77]. It is therefore plausible to assume that Dancoff was not aware that the Coulomb self-interaction would cause transitions potentially relevant to his calculation, and consequently entirely ignored it, even though, and this is important in order to judge Dancoff's calculation as wrong in the second (stronger) sense, a knowledge of these transitions was implicitly available in

[30] For a transition from a state $\mathbf{k}^-$ to a state $\mathbf{k}$ there are two contributions: One has q identified with $\mathbf{k}$ and t identified with $\mathbf{k}^-$, while $r = s$ is an intermediate positive energy state, the other has r identified with $\mathbf{k}^-$ and s with $\mathbf{k}$, while $q = t$ is an intermediate negative energy state. The contributions thus have a different structure for positive and for negative intermediate states, leading to a non-vanishing amplitude, as opposed to a "one-particle calculation" with a vacuum in which the Dirac Sea is empty.

published work that Dancoff certainly was aware of.
This assumption that Dancoff really overlooked the transitions induced by the hole-theoretic Coulomb self interaction is further backed by the fact that if he had been aware of them, he would have had no a priori reason to believe that the divergences stemming from these transitions might not alter his divergent result and cancel those divergences he had obtained. Without mass renormalization, the inclusion of the electrostatic terms would not have changed his conclusions, but would certainly have strengthened them, by making his results relativistically invariant. There is thus no reason to believe that Dancoff would have not mentioned the divergent Coulomb terms, had he included them in his calculations. Combined with the fact that one can reasonably assume that he was not aware of their potential relevance, I believe it is safe to state that Dancoff was wrong in the (stronger) second sense defined above. We could leave it at this. However, if we recall the statements referred to in the introduction, we see that there is a general agreement in the secondary literature that Dancoff was in fact wrong in the third (strongest) sense, that had he included the Coulomb terms, he would have ended up with a finite result. To understand how these statements come about, we need to go beyond Dancoff's work and look at the history of its reception.

3.2 Dancoff's divergence stays

After completing his PhD work at UC Berkeley, he took up a teaching position at the University of Illinois, Urbana-Champaign, where he would spend the main part of the rest of his short life, with the exception of a brief stay with Pauli in Princeton and his work at the Chicago Metallurgical Lab of the Manhattan project. Dancoff died in 1951, only 37, of lymphoma, less than four months after learning he had cancer, leaving behind his wife Martha and two small daughters. Only very few people knew of his illness before he died (Obituary, NBLA). One of them was his PhD advisor Oppenheimer (Letter from Dancoff to Oppenheimer, 16 May 1951, Oppenheimer Papers).
The question of the infrared divergence took on an interesting role in the following years. During the war, two major proposals were brought forth on how to remove the divergence difficulties of QED, by Paul Dirac [16] and by Walter Heitler and his assistant Huan Wu Peng [28]. With most prominent theoretical physicists getting more and more involved in war work,[31] there were initially very few responses to these propositions: Only Wolfgang Pauli, in Princeton for most of the war years and feeling increasingly isolated, with all of his colleagues going off to work for the Manhattan project, studied these theories in detail, true to his reputation as the "conscience of physics". A central aspect of this study was the question how these new propositions dealt with the infrared divergence.
Pauli had originally been quite enamored with Dirac's proposal (Letter from Pauli to Dirac, 6 May 1942), and hoped that it might give further insight into the question of the infrared divergence:

> I am particularly interested in the problem of the corrections to the Rutherford-scattering-formula due to the (small) radiation emitted by the scattering. This problem was treated by Bloch-Nordsieck, Fierz and me and by Dancoff and it was shown that it is beyond the reach of the old-form of quantum electrodynamics

[31] Dirac was too, but presented his unfinished work on the occasion of receiving the Baker Medal of the Royal Society in 1941 [20], while Heitler had moved to neutral Ireland in the same year.

(because the perturbation method fails and an infinite number of photons with small frequency is emitted). I guess the new theory will give a definite-finite result for this question.

Dirac's theory posited a "hypothetical world" in which both positive and negative energy photons existed and then gave rules for translating the transition probabilities calculated in the hypothetical world into the real world, e.g., by interpreting the emission of negative energy photon as a the absorption of a real photon. The contributions from the negative energy (or equivalently negative frequency) photons cancelled some of the self-energy divergences of quantum electrodynamics.[32] However, contrary to Pauli's hopes, they actually ruined the solution of the infrared divergence of BNPF. In order to see this, consider the equivalent of the matrix element of equation 13 in the hypothetical world, where the corresponding process is the emission of n_s positive energy photons and no negative energy photons:

$$\prod_s (0,0,\mathbf{k}|K|n_s,0,\mathbf{k}') = \prod_s \int e^{\frac{i}{\sqrt{2}}(\mathbf{k}'-\mathbf{k})\cdot\mathbf{a}_s^+ Q_s^+} H_{n_s}(Q_s^+) H_0(Q_s^+) dQ_s^+ \int e^{\frac{i}{\sqrt{2}}(\mathbf{k}'-\mathbf{k})\cdot\mathbf{a}_s^- Q_s^-} H_0(Q_s^-) H_0(Q_s^-) dQ_s^- \tag{32}$$

where $Q_s^\pm$ and $\mathbf{a}_s^\pm$ are the coordinates and polarization vectors associated with the positive and negative energy photons, respectively, and the square roots of 2 show up for normalization purposes. The decisive point is now that the negative energy (or, equivalently, negative frequency) photons have imaginary polarization vectors $\mathbf{a}_s^-$, as one can see if one replaces ω_s by $-\omega_s$ in equation 7. The exponents for the positive and the negative energy photons in the square of the matrix element (the equivalent of equation 14) then have an opposite sign and cancel, which means that the emission of a finite number of (real, positive energy) photons no longer has a vanishing probability, thus reintroducing the infrared divergence. Dirac's theory also had other, possibly even more damaging, drawbacks; in particular the infinities cancelled by the negative energy photons at order α seemed to reappear in higher approximations (Letter from Pauli to Wentzel, 27 November 1944). But it was the elementary infrared difficulty of Dirac's theory that Pauli presented in a couple of talks, during and after the war [56, 57].
A similar fate befell Heitler's theory. Heitler's general idea was to take into account only the lowest-order non-vanishing contributions to a given transition, as the higher order contributions were divergent in general, e.g., in the electron self-energy, where only the zeroth order contribution (i.e., the bare mass term) gave a finite contribution. Pauli immediately suspected that this would also reintroduce the infrared divergence, just as Dirac's theory (Letter to Wentzel, 27 November 1944): After all, Dancoff (as well as Braunbek and Weinmann) had shown that the ultraviolet divergence difficulties of QED entered the cross section through the same terms that were necessary to remove its infrared difficulties. In removing the ultraviolet divergences, Heitler and Peng were throwing the baby out with the bath water. This observation was mathematically proven in a paper written by Bethe and Oppenheimer [5] almost immediately after wrapping up the Manhattan Project. Clearly, Oppenheimer had not forgotten Dancoff's puzzling results[33] and shared Pauli's conviction that the infrared divergence

[32] Namely those that consisted of integrals over an odd power of the virtual photon frequency. Those divergences that consisted of an integral over an even power of the frequency could be removed by relativistically invariant subtraction procedures.

[33] About a year later, another Manhattan Project colleague of Oppenheimer's, Robert Serber, referred to the problem as Oppenheimer's "favorite scattering problem." Letter from Serber to Oppenheimer, 21 January 1948,

provided a good "test-problem" (Letter from Pauli to Møller, 20 May 1946) for modifications of quantum electrodynamics. He and Bethe concluded their paper with the words:

> In any case, we believe that the simple problem here considered may afford a useful test of future theories of radiation [5, 458]

Before looking at the further use this infrared test was put to, I briefly return to the linguistic question we already encountered in the context of Dancoff's work: The use of the term renormalization. In the penultimate draft, which Bethe sent to Oppenheimer on 31 May 1946 (Oppenheimer Papers), they were still using the term "renormalization" in Dancoff's sense, referring to the "renormalization of the wave function which is obtained by considering the perturbation by virtual quanta." This was changed by Oppenheimer in the final draft simply to "the perturbation by virtual quanta" (in the last paragraph of section 2). It may have been this editorial decision which prevented the term "renormalization" of establishing itself in physics in Dancoff's sense, instead freeing it for its modern usage. The use of "renormalization" in Dancoff's text remained an anomaly, one that would later, as I will show, lead to some confusion.

Leaving this question aside for the time being, there was another attempt at removing the self-energy divergence which, however, revealed other grave difficulties before being subjected to the infrared test: Already during the war, Abraham Pais had developed a theory in which a heavy scalar field (the f-field) made a contribution to the self-energy of charged particles that (almost) exactly cancelled the contribution from the electromagnetic field [52, 53], if the coupling to the f-field, given by the coupling constant f, was related to the electric charge e by

$$f^2 = 2e^2 \tag{33}$$

The resulting finite shift in the self-energy was to be regarded as the difference between the mechanical mass (i.e., the mass parameter in the Hamiltonian) and the physical mass due to the interaction with both the electromagnetic and the f-field. For electrons, f-field quanta with masses greater than about 200 MeV meant that this shift could be treated as a small perturbation to the mechanical mass, which consequently gave the dominating contribution to the physical mass. At the same time, a value for the mass of the f-field quanta at the lower end of the allowed spectrum (i.e., approximately 200 MeV) meant that for protons this shift was of the order of magnitude of the proton-neutron mass difference. Since the neutron is electrically neutral and thus also not f-charged, one could start from identical mechanical masses for proton and neutron and explain their mass difference purely through the electric charge of the proton and its corresponding f-charge, needed in order to cancel its divergent electromagnetic mass. Pauli was not too thrilled with Pais's ideas from the start (Letter from Pauli to Pais, 23 October 1945), in particular as the cancellation could only be proven to the first approximation. But it was another difficulty that led to its quick demise: Even though its large mass gave the interaction mediated by the f-field a very limited range, it became important within the nucleus. It was generally assumed in nuclear theory that the nuclear interactions treated neutrons and protons in the same way. All differences between neutrons and protons could be accounted for by the Coulomb interaction: Both for the mass

Oppenheimer Papers. Also in a letter to Kramers from 4 December, 1947, Oppenheimer refers to the problem as "the treatment of reactive effects in collision processes, about which I have always been so concerned."

differences between mirror nuclei (i.e., nuclei with their number of neutrons and protons exchanged) and for the differences between proton-proton and proton-neutron scattering the experimentally observed results were well reproduced by assuming a Coulomb interaction between two protons, which was absent when neutrons were involved. An inclusion of the f-field, which gave an additional interaction that differentiated between neutrons and protons, spoiled this agreement in both cases (as shown by Wightman for the mirror nuclei [81], and by Zilsel, Blair and Powell for scattering [82], see also letter from Rabi to Pauli, 14 February 1947), and could only be reconciled with experiment if one made the f-field quantum significantly heavier, which in turn spoiled the reproduction of the proton-neutron mass difference. In other words: By introducing a difference between neutrons and protons large enough to explain their mass difference, Pais' theory destroyed the symmetry between neutrons and protons, which had been observed both in nuclear spectroscopy and in scattering experiments.
An equivalent theory (including the explanation of the nucleon mass difference), however, had been developed in Japan by Shoicihi Sakata, also during the war. Sakata first presented his theory (which came to be known as the C-meson theory, Sakata's name for what Pais had called the f-field) at the Symposium on the Theory of Elementary Particles, held in Kyoto from 21 to 23 November, 1946 [60]. Different than in the west, the difficulties of the theory with regards to nuclear phenomenology were not immediately recognized (Wightman's article was submitted less than two months after Sakata's presentation). In fact, there is no indication of a reception of these results, before a letter from Pais to Sin-Itiro Tomonaga from 13 April 1948 in which these difficulties (and the corresponding papers) are explicitly pointed out, along with Pais' acknowledgment that he knew no way of circumventing these difficulties. In April 1947, when Sakata, together with O. Hara, gave a further elaboration of his method in a letter to the editor of the Japanese journal Progress of Theortetical Physics [61], it was still viewed in Japan as a promising way towards removing the infinities of QED - for this it did (to first approximation), the difficulties with the neutron-proton symmetry that had been discovered in the USA notwithstanding. Consequently, Bethe and Oppenheimer's prescription of applying the infrared test, was now applied in Japan, by Sin-Itiro Tomonaga to Sakata's theory.[34]
The infrared test looked somewhat different for the Sakata-Pais theory than it had for Dirac's or Heitler's theories. The introduction of the additional scalar field involved no changes in the electrodynamic interaction, it was only after calculating the electromagnetic effects in the regular way that the divergences were cancelled by the separately calculated effects of the C-meson. It was thus trivial that the cancellation of the infrared divergence would hold in Sakata's theory, just as it had for QED. The question was rather a different one: Would the C-meson field, which had been specifically designed to cancel the self-energy divergence, also cancel the infinity that Dancoff had discovered in the scattering cross section? It was this question that Tomonaga, along with Daisuke Ito and Ziro Koba, attempted to answer. By November 1947, Tomonaga had been able to reproduce Dancoff's result and to make a similar calculation for the radiative corrections to scattering due to the C-meson. He found [32] that the C-meson could in principle cancel Dancoff's divergence, if one imposed the following condition on the coupling constant:

[34] It is surprising at first sight that in late 1947, Tomonaga was aware of the paper by Bethe and Oppenheimer (October 1946 issue of the Physical Review) and, as we shall see later, of Bethe's Lamb Shift paper (August 1947, Issue 4), but not of the papers by Pais (November 1945), Wightman (April 1947, Issue 7), or Zilsel et al. (August 1947, Issue 3, i.e. not in the same issue as Bethe's paper). It should be kept in mind, however, that access to the Physical Review, although possible in postwar Japan, was initially limited to the American Civil Information and Education Section libraries, making an in-depth study of each issue very difficult [49].

$$f^2 = \frac{14}{9} e^2 \tag{34}$$

in which case, however, the self-energy divergence was no longer cancelled. Tomonaga thus concluded that

> The present formalism of the "cohesive force field" theory is not sufficient to eliminate all the difficulties of the quantum electrodynamics.

Tomonaga had repeated Dancoff's oversight of the modified Coulomb self-interaction. But for him this oversight had major ramifications: If he had included the Coulomb terms at this point, he would have found that the C-meson could indeed cancel both the self-energy divergence and the divergence in the scattering cross section at the same time, i.e., with the same condition on the coupling constant, given by equation 33. Tomonaga was thus at this point certainly wrong in the third sense given above. Tomonaga himself discovered this mistake within a couple of months, at some point between 24/25 November (when the failure of the C-meson theory was announced at a symposium of the Physical Society of Japan in Kyoto) and 30 December 1947 (when an erratum to the original letter was submitted [32]). At almost exactly the same time, Dancoff's mistake was discovered in the USA, by two students of Oppenheimer, Hal Lewis and Saul Epstein, who submitted their papers on the matter on 24 and 25 November 1947, respectively [42, 19]. In both cases the mistake was discovered in the context of the newly developing framework of renormalization theory.

3.3 Dancoff's divergence removed

Renormalization theory proper begins with Bethe's 1947 paper on the Lamb Shift, written directly after the announcement of the experimental discovery of the shift at the Shelter Island conference. This paper was received enthusiastically not only in the West, but also by Tomonaga in Japan. What was the essence of Bethe's new approach? The central problem of the divergent self-energy in QED had been that the self-energy was dependent on the state of the electron. If one thus calculated the divergent self-energy, say for a free state, and then subtracted this divergent self-energy from the self-energy of an electron in a bound state, the resulting difference was still divergent. It was Bethe's fundamental insight that one should not subtract the numerical value of the self-energy of the free electron, but instead interpret that self-energy as the expectation value of an operator and then subtract that operator from the Hamiltonian of the bound electron, the counterterm δm mentioned earlier. Non-relativistically, the self-energy of a free electron in a momentum (and energy) eigenstate with momentum $\mathbf{p}$ (and energy $\mathbf{p}^2/2m$) due to its interaction with transverse electromagnetic waves (recall that the Coulomb self-energy was generally always subtracted at the very beginning) is

$$-\frac{2e^2}{3\pi\hbar c}\frac{\mathbf{p}^2}{m^2c^2}\int_0^\infty dE \tag{35}$$

This expression was now interpreted as the expectation value of an operator, which looked exactly the same only with the eigenvalue $\mathbf{p}^2$ replaced by the squared momentum operator. This operator was then subtracted from the Hamiltonian of the bound electron. The self-energy thus corrected turned out to be only logarithmically, and no longer linearly, divergent. Bethe introduced an explicit cutoff for the photon energy integral at the rest energy of the electron mc^2, since he hoped and expected that relativistic effects would render the expression finite. The expression for the Lamb Shift obtained from this rather crude approximation turned out to be quite close to the experimentally observed value, marking the first, immediate triumph for the renormalization program.
Bethe's calculation left the search for a relativistic generalization of his procedure as the immediate next task. In a way this is very similar to the situation after the discovery of the Pauli-Fierz divergence: In both cases there was a divergence (and thus the necessity of introducing a cut-off) in a previous calculation, which was interpreted as an artifact of the non-relativistic treatment and expected to vanish in a full relativistic calculation. And in both cases a relativistic treatment was faced with considerable difficulties. The reason in the case of renormalization was the following: The self-energy of a free electron at rest had been calculated by Weisskopf in 1939, taking into account not only relativity, but also spin and Dirac's hole theory. The result was, however, a lot harder to simply turn into a self-energy operator, as it consisted of a highly non-trivial sum of three divergent integrals, identified by Weisskopf as the self-energy contributions of the electrostatic self-interaction (which, as we have already seen, was always explicitly included by Weisskopf), the electromagnetic field created by the electron's spin and the effect of the vacuum fluctuations of the electromagnetic field, respectively. Weisskopf had been able to calculate the divergent part of the self-energy, by studying only the singular ultraviolet behavior of these integrals, to be[35]

$$\frac{3}{2\pi} mc^2 \frac{e^2}{\hbar c} \int_{\epsilon}^{\infty} \frac{dp}{p} \tag{36}$$

but had not been able to give expressions for the additional finite terms. These finite terms were, however, essential to get the right correction term and this problem was pursued by several groups in the two years after Bethe's paper. Weisskopf himself, together with his student Bruce French, as well as Willis Lamb and Norman Kroll managed to obtain a full expression for δm by imposing further physical demands on the resultant renormalized Hamiltonian [23, 41]. The most elegant way of obtaining the full electromagnetic mass operator turned out to be a relativistic generalization of the BNPF procedure, which was developed independently by both Schwinger and Tomonaga. This required a total, manifestly covariant reformulation of QED. Schwinger only appears to have started working on such a reformulation in 1947, while it had been the main focus of Tomonaga's research since 1943 and of his group since right after the war, without anticipating its usefulness in the renormalization program [15].
In order to put renormalization theory to the infrared test, however, these details were unimportant. Since the electromagnetic mass was not infrared divergent, it was clear that the correction term would not reintroduce the infrared divergence, no matter what the finite terms. The question was thus the same as it had been for the C-meson theory: Would the renormal-

[35] ϵ is here some small momentum. It's precise value affects the finite part and is thus not relevant if one is only interested in the divergent part. It is included here to indicate explicitly that the self-energy is not infrared divergent.

ization of the mass also remove the divergent terms from the scattering cross section? And to answer this question it was sufficient to look at the divergent part of the electromagnetic mass correction term, which was simply taken as the numerical value for the self-energy given above multiplied by the Dirac matrix β. Note, that in this way it was not possible to actually calculate the radiative corrections to Rutherford scattering: For this one would have to know the finite parts of δm. It was only possible to check whether renormalization theory was able to get rid of Dancoff's divergence. In Japan, this calculation was performed by Tomonaga together with Ziro Koba, in the USA it was performed by Lewis and Epstein. In the course of these calculations, both groups discovered Dancoff's mistake, obtained a corrected result for the divergent parts of the cross section and found that these were indeed cancelled by the divergent parts of the correction term. They both therefore arrived at the conclusion that renormalization theory passed the infrared test with flying colors.

3.3.1 Dancoff's divergence removed in the East

Both in the USA and in Japan, Dancoff's mistake was only discovered after the reception of Bethe's renormalization method, i.e., at a time when both groups already knew what to look for: a divergent radiative correction that would be cancelled by the divergent mass correction term. Renormalization theory had already enjoyed a splendid success in explaining the Lamb Shift, thus making an in-depth revisiting of Dancoff's calculation attractive. And by relating Dancoff's divergence to the self-energy divergence, it brought the question of the Coulomb self-interaction back on the table, which, as we have seen, was habitually dropped from the outset in dynamical problems, such as the scattering problem (leading to Dancoff's mistake), but had played an important role in Weisskopf's self-energy calculations. The explicit recourse to Weisskopf's calculation is also what distinguishes renormalization theory from C-meson theory in this respect: In the latter, one did not first calculate the self-energy as an intermediate step. Rather, the dynamical contributions from the C-meson were supposed to directly cancel Dancoff's divergence. This may help to explain why Tomonaga repeated Dancoff's mistake in the calculation in C-meson theory, but discovered it in the renormalization framework. Another reason may well be that C-meson theory was not supported by a spectacular triumph, such as the Lamb Shift, and was thus more easily dismissed.
Tomonaga gave another reason for his discovery of the mistake in his Nobel lecture, where he claimed that his covariant techniques and the BNPF-inspired method of canonical transformations led him to discover Dancoff's mistake:

> This new method of calculation was to use the technique of contact transformations based on the previously mentioned formalism of the covariant field theory and was in a sense a relativistic generalization of the Pauli-Fierz method. This method had the advantage of separating the electromagnetic mass from the beginning, just as was shown in their paper.
> Our new method of calculation was not at all different in its contents from Dancoff's perturbation method, but had the advantage of making the calculation more clear. In fact, what took a few months in the Dancoff type of calculation could be done in a few weeks. And it was by this method that a mistake was discovered in Dancoff's calculation; we had also made the same mistake in the beginning.

> Owing to this new, more lucid method, we noticed that, among the various terms appearing in both Dancoff's and our previous calculation [in the context of the C-meson theory], one term had been overlooked.

But, as already indicated, the first papers in which Tomonaga presented a corrected version of Dancoff's calculation did not use these covariant techniques, but rather used the regular non-covariant perturbation techniques of the 1930s that also Dancoff had used. This set of three papers was submitted in late February/early March 1948. Here Tomonaga and Koba redid Dancoff's calculation, both in the context of the simple renormalization approach, where only the divergent mass term was subtracted [39], and (together with Daisuke Ito) in the context of C-meson theory [33, 34].
Tomonaga had already presented the general idea of his covariant renormalization techniques early on: A preliminary report on these attempts was presented by him and Takao Tati already in November 1947, at the same symposium where Tomonaga erroneously announced that the C-meson theory failed the infrared test. Tomonaga and Tati here already announced that they planned to treat radiative corrections to scattering in this new covariant framework [70]. The paper in which this was done (along with a covariant treatment of the Lamb Shift), however, was only submitted (by Tomonaga, Hiroshi Fukuda and Yonezi Miyamoto) half a year after the non-covariant calculations discussed above, in September 1948 [24]. These covariant calculations were preceded by a strong disclaimer:

> Although we have thus successfully obtained finite answers for these field reaction problems and they are of the magnitude agreeing with experimental results, we must nevertheless confess that the calculation carried out in this paper is still unsatisfactory because we have had to make a non-relativistic treatment in the evaluation of the effective energies, which have the form $\infty-\infty$. In the calculation with such improper expressions, from which we wish to draw a finite conclusion, the results are often affected by the way of calculation [...] But we think we have been able to confirm, at least, that the result converges by virtue of our subtraction procedure. A method more satisfactory from the relativistic point of view is now being investigated. [24, 48]

So, although it is quite possible that Tomonaga discovered hints that there was a problem with Dancoff's calculation during his exploratory attempts with covariant renormalization in late 1947, these techniques were not at the time sufficiently developed to reconstruct Dancoff's calculation in detail. Such a reconstruction was initially only done in the non-covariant framework also used by Dancoff himself, but now with renormalization theory as a guide. Why then, in his Nobel lecture, did Tomonaga highlight the importance of his covariant techniques for finding Dancoff's mistake, while downplaying the importance of renormalization theory? One answer is that these covariant techniques were his own, while the notion of renormalization had been taken from Bethe. There is, however, I believe, a more subtle reason, which also explains why Tomonaga became one of the two major sources for the strong version of the Dancoff story, i.e., that Dancoff was wrong in the third, strongest sense given above: Tomonaga appears to have believed that Dancoff had already in 1939 developed the notion of renormalization, and only failed to recognize its validity because of his calculational error. Now this is all the more strange, as Tomonaga was well aware that, as I have also argued, BNPF did not see their procedure as a mass renormalization, and that consequently his

(relativistic) generalization of the BNPF procedure as a renormalization method was also a reinterpretation. In his first full paper on covariant renormalization, submitted with Tati in May 1948, he wrote:

> The fundamental assumption of Bethe's theory lies in the following hypothesis: the self-energy of a free electron is, though infinite in the current formalism of the quantum electrodynamics, already included in its mass which we really observe; when, however, the electron is in a bound state, a finite deviation of the self-energy from that in the free state will appear. The deviation is then observed just as the level-shift of the bound electron in question.
> Although this hypothesis was in this manner for the first time formulated explicitly and clearly in the light of the experimental evidence, *implicitly* such an idea had been used in the earlier considerations of Bloch and Nordsieck as well as Pauli and Fierz about the self-field of an electron. (Emphasis by me). [71]

But in his Nobel lecture:

> He [Dancoff] calculated relativistically the infinities appearing in the scattering process and determined which of them could be amalgamated into the mass and which remained as infinities proper to the scattering process alone.

How does this go together? In the original research papers, Tomonaga had stressed that Pauli and Fierz had used mass renormalization only implicitly, yet here is Tomonaga claiming that Dancoff was explicitly attempting to use mass renormalization techniques to remove the infinities and failed, i.e., that Dancoff was wrong in the strongest, third sense. However, as I have attempted to demonstrate, the only sense in which Dancoff (and Bloch and Oppenheimer) went beyond Pauli and Fierz was by making their calculation relativistic. There is no evidence that they performed their calculation with an explicit concept of mass renormalization at hand, and there is certainly no hint of such a concept in Dancoff's published paper. How then did Tomonaga arrive at this statement? The answer, I believe, can be found in Tomonaga's 1966 Nishina Memorial Lecture, in which he also recounted the story of Nobel Prize-winning work, but went into somewhat more detail. This lecture is only available in Japanese and has hence escaped the attention of the western secondary literature.[36] Here, Tomonaga discusses the pre-history of the renormalization concept:

> This idea of renormalization had been around for a long time and different people had been using this concept, both explicitly and implicitly. Pauli and Fierz used it unwittingly, but Dancoff used it explicitly *and called it by the name of renormalization*. Dancoff used the word renormalization, but he made a mistake, so that there remained an infinity that could not be resolved by the renormalization of mass or charge. (Emphasis by me)[37][38]

[36] Aramaki references it, but stresses an entirely different point.

[37] I thank Dr. Yasushige Yano of the Nishina Memorial Foundation for making the text of this lecture available to me. Many thanks also to Michiyo Nakane and Hajime Inaba for helping with the identification and the translation of this decisive passage.

[38] A similar statement about Dancoff already using the word renormalization can be found in Tomonaga's Nobel lecture, but the explicit contrast with Pauli and Fierz is missing.

But, as we have seen, what Dancoff called renormalization in 1939 had nothing to do with the concept of renormalization that entered physics a decade later. Tomonaga's overestimation of Dancoff's program thus seems to stem from Dancoff's isolated and unusual use of the term renormalization and Tomonaga's later very benevolent reading of that very short and elliptic paper. The question remains then, why was this mistake of Tomonaga's not realized earlier? After all, he did meet Dancoff in Princeton in 1949. Granted, from Tomonaga's recounting they do not seem to have had a very in-depth debate on physics, but rather just briefly exchanged their respects. But they did also touch on Dancoff's mistake, both of them expressing their regret at his unfortunate oversight. And why didn't Bloch or Oppenheimer comment on this at some time, making clear that they did not yet have the concepts of renormalization at hand in 1939, despite Tomonaga's claims? To understand this, we must take a look at the second, independent discovery of Dancoff's mistake by Lewis and Epstein in the USA.

3.3.2 Dancoff's divergence removed in the West

With Lewis and Epstein there is no question that they discovered Dancoff's mistake because they working in the context of renormalization theory, and thus knew what to look for and to relate Dancoff's divergence to the self-energy. They did not yet have at their disposal the covariant techniques that Tomonaga was using in parallel to the non-covariant calculations he finished first: Such techniques were only just being developed in the West by Julian Schwinger and, with a very different approach, by Richard Feynman. Lewis and Epstein submitted their results in two separate papers in November 1948, in which they demonstrated that renormalization theory passed the infrared test [42, 19]. Regarding the possible effects of the finite terms Lewis contended towards the end of his paper:

> It should be noted that we have only shown that the divergent part of these matrix elements corresponds to a mass effect and, in fact, there are finite parts that do not, so that transitions of this type should properly be included to a finite extent. However, one is faced, in doing this, with the problem of separating a finite part of a formally infinite term, so that a specification of the way in which the infinite integral is performed is required for an unambiguous result. Schwinger has given a procedure for separation of mass terms, by means of a canonical transformation on the Hamiltonian [...] which leads to finite terms of this character. [42, 176]

But even though they they had to bracket the finite terms, the demonstration that Dancoff's divergences could be viewed as the effect of an electromagnetic mass operator required identifying the transitions from the Coulomb self-interaction that Dancoff had missed. Now, surprisingly, while this had been the central point in the reception of Dancoff's paper for Tomonaga, this point was treated rather briefly in Epstein's paper. When calculating the transitions from the divergent part of the electromagnetic mass operator, he wrote

> ...one obtains an expression which differs only in the multiplicative constant from the divergent terms that Dancoff found, suggesting quite strongly that these are identifiable as manifestations of the electromagnetic mass of the electron. In fact, the numerical difference arises from Dancoff's omission of certain electrostatic transitions, which are, of course, essential to the covariance of the scheme, and, if included, make the agreement exact. [42, 174]

In the US the problem with Dancoff's work was initially seen to be an entirely different one. We have seen, that in their initial investigation of the relativistic scattering problem, Bloch and Oppenheimer had dropped a divergent term, namely, the contribution of the electromagnetic self-energy to the density of states. I have also explained how this procedure is equivalent to what happens in the non-relativistic case, when one drops the electromagnetic mass operator at the end of the BNPF procedure. But while a relativistic generalization of the BNPF procedure (as performed by Tomonaga and Schwinger) gives a full renormalization of the mass, this is not the case for the Bloch-Oppenheimer method, even though it is placed in a relativistic framework: As I have outlined, the relativistic electromagnetic mass operator not only influences the density of states (through its diagonal elements), but also has off-diagonal elements, which influence the scattering matrix elements. Now, there is no indication that in 1939 Bloch, Oppenheimer or Dancoff had thought in terms of (and much less calculated with) an electromagnetic mass operator. They had simply dropped the higher-order corrections to the density of states and kept the higher-order corrections to the scattering matrix elements. But, of course, in hindsight this could be read as an incomplete renormalization procedure, where one had only subtracted the diagonal elements of the electromagnetic mass correction term and neglected the off-diagonal terms. This is indeed how Oppenheimer described the matter in a letter to Kramers on 4 December 1947, shortly after the submission of Lewis' paper (Oppenheimer papers):

> ...the treatment of reactive effects in collision processes, about which I have always been so concerned, is also at long last in order. In fact, the infinite terms which Dancoff found in his relativistic treatment correspond just to the circumstance that the perturbation calculations, as used, only separated off the inertial effects correctly in the approximation in which they had no non-diagonal matrix elements. In the relativistic domain such matrix elements, of β, do exist, corresponding to pair creation; by recognizing and eliminating these terms, one then obtains a perfectly sensible and finite result.

Note that there is no mention of Dancoff missing the electrostatic terms: Dancoff's mistake was solely viewed as having used an incorrect renormalization procedure. But whether or not one wants to call Bloch and Oppenheimer's procedure of dropping the divergent, first-order correction to the density of states a (insufficient) "mass renormalization," the fact remains that it was quite clear to those who discovered Dancoff's technical error (i.e., missing the electrostatic terms) in the US that there was a lot more that separated him from a full renormalized QED than simply an omitted Coulomb self-interaction: It was the realization that a removal of the ultraviolet divergences would require more than simply dropping a term in the density of states. Dancoff was missing Bethe's fundamental insight of renormalization theory that what was important was not dropping the numerical contribution of the self-energy (in this case to the density of states), but introducing a mass *operator* with all the effects that entailed. This important thought is nowhere to be found in the work of Bloch, Oppenheimer and Dancoff.
Still, Oppenheimer apparently lamented the fact that they had not discovered mass renormalization in 1939 - despite the conceptual gap just described that separated their work from the renormalization program of the late 1940s. But as to what exactly had been missing, what had put them off track, Oppenheimer was quite contradictory. In a letter to Pauli from 9 December 1947 he wrote:

> As for the scattering problem, we noted first that in the treatment that you and Fierz gave, and in the later work by Dancoff on the relativistic extension, the principle divergent terms to be expected, say in the Born approximation, because of a change in mass, were for the most part missing. [...] *You will understand the reason for this in the treatments you and* Fierz gave. In the case of Dancoff's work it was simply that he adopted the usual, but in this case incorrect, procedure of regarding the energy and the free states of the particle as unaffected by the perturbation, whereas in fact they are. (Emphasis in the original)

This is quite a convoluted logic, so convoluted in fact that my interpretation of this passage must remain tentative: It appears as if Oppenheimer here sees the reason for Dancoff's failure precisely in the use of Bloch and Oppenheimer's method of dropping the divergent corrections to the density of states (the "usual procedure"). Because of this one had failed to recognize that all the divergences were due to the electromagnetic mass. If they had left in the divergent terms, then the entire divergent contribution of the self-energy would have been there (except for the missing electrostatic terms, which Oppenheimer mentions in passing later in the letter), ready to be cancelled by an electromagnetic mass correction operator. This seems to entirely contradict his description in Kramers' letter: Rather than viewing the procedure used by him an Bloch as an approximation to renormalization that didn't go far enough, its originality is now downplayed ("usual procedure") and it is even viewed as an impediment to a possible earlier discovery of a divergence-free QED. This flip-flopping can, in my opinion, only be understood from Oppenheimer's frustration at what he perceived as a great missed opportunity.
It is no wonder given this erratic re-writing of history that, when Tomonaga arrived in Princeton a year later, his misconceptions concerning Dancoff's paper were not corrected. Rather, Oppenheimer appears to have gladly adopted Tomonaga's narrative: They (Bloch, Dancoff and he) had basically developed renormalization theory in 1939 and it was only Dancoff's mistake (which now meant missing the electrostatic terms) that had dashed their hopes. Robert Serber, who is the main primary American source for the Dancoff story, later recounted that Oppenheimer believed that he might well have gotten the Nobel Prize for renormalized QED if Dancoff had not made his mistake [13, 107], and that Bloch shared that opinion, remarking to Oppenheimer years later that if Dancoff had not made his mistake "we would have seen the point at the time" [67, 212], or in a different recounting of the story:

> If Sid had gotten the right answer, he might not have known what it meant, but we sure would have. [68, 49-50]

Serber nowhere went as far as claiming that Bloch and Oppenheimer had actually developed renormalization theory. Instead, he described their approach in the following manner:

> They had some of the right ideas, distinguishing between mass, vertex, and wave function divergences...

The mention of wave function divergences recalls Tomonaga's misreading of Dancoff's wave function "renormalization," while the mass and vertex divergences apparently refer to the divergence in the density of states (mass divergence) and to that in the cross section (vertex

divergence). This reconstruction of their approach is, however, just as confused as Oppenheimer's. After all, it was precisely the distinction between these two divergences that had initially been regarded as Dancoff's mistake in the USA: In renormalization theory these two divergences were understood as coming from the same source, the mass correction operator and its diagonal and off-diagonal elements respectively. To top it off, Serber went on to add his own little story to the now well-established narrative of Dancoff's mistake:

> Sid showed me a manuscript of his paper before he published it and I found a mistake. He didn't correct the mistake, but mentioned it in a footnote and said it didn't really affect the conclusion. [68, 49]

But, as we have seen, this footnote referred to the vacuum polarization terms, which Dancoff had treated entirely correctly. The implication in the quote above is, however, that Serber had pointed Dancoff to that mistake that later turned out be essential (and he was interpreted in this way by Crease who paraphrases Serber's recounting of the episode [13, 107]). The myth of Dancoff's paper thus became the focal point for a whole generation of field theorists, working in the 1930s, who couldn't quite believe that they hadn't discovered renormalization theory already then. This myth was consequently adopted and further simplified in the secondary literature, where we then find the claim that the self-energy divergences would have cancelled the vertex divergences without any further recourse to renormalization techniques, if only Dancoff hadn't made his mistake.

4 Conclusions

I hope I have been able to show that the story is by no means as simple as that: Of course it is not unthinkable that, if Dancoff had not made his mistake, someone would have realized that his divergence looked very much like the self-energy divergence, that both could be understood as resulting from a divergent mass operator, and consequently could have developed renormalization theory in 1939. However, any such attempt at counterfactual history would need to address the varied points addressed in this paper, which I will summarize in the following.

First of all, Dancoff's mistake was not merely a silly fumble: It has its reason in the methods of QED of the 1930s, where the Coulomb self-interaction was always (and non-covariantly) seperated (by going to the Coulomb gauge) and then dropped before any calculation was performed. Dancoff's mistake was only discovered in the late 1940s, by physicists who already had the technique of renormalization at hand, and, in the case of Tomonaga, even had covariant methods at their disposal, which were unavailable to Dancoff.

Second, although there was certainly a good deal of dropping divergences in the QED in the 1930s, these were not formalized in terms of the subtraction of a mass operator, that is in terms of renormalization theory proper, which was only suggested by Bethe in 1947. It was this conceptual advance that allowed to view the self-energy divergences (which were not solely conceptualized in terms of an electromagnetic mass in the 1930s) and the divergences in the cross-section as resulting from the same source. Once Bethe had shown that the self-energy could be expressed as the effect of a divergent electromagnetic mass operator, it was a small step to the realization that this operator could also have off-diagonal elements appearing in scattering cross sections. But as long as the self energy was merely treated as a divergent

number in energy eigenvalues, divergent scattering amplitudes seemed to be an entirely new divergence, unrelated to the self-energy.
Third, even if Dancoff's mistake had been realized in 1939 and consequently the notion of a divergent mass correction operator, which then also eliminated the self-energy divergence, had been developed, this still would not have been sufficient to usher in the triumph of renormalized QED a decade earlier. I have shown that without the covariant techniques (that were developed only in the 1940s), the only thing that could be demonstrated was the finiteness of the radiative corrections calculated by Dancoff. An actual, halfway unambiguous calculation of the exact value of these radiative corrections, was still beyond the grasp of both Oppenheimer's and Tomonaga's groups in late 1947/early 1948. It required the further maturation of the covariant techniques of Tomonaga and Schwinger (and then also Feynman and Freeman Dyson) in order to make definite quantitative predictions for these radiative correction terms.
Fourth, even if these methods had been developed in the wake of Dancoff's correct(ed) calculation, the fact remains that Dancoff's calculation was isolated from empirical results, and solely meant to address the theoretical difficulty of the infrared divergence. The acceptance of the renormalization program and the general belief that the covariant calculations of radiative corrections, dealing as they did with the subtraction of one infinite quantity from another, gave unambiguously correct predictions, certainly rested heavily on the successful calculation of experimentally observed effects, i.e., the Lamb Shift and the anomalous magnetic moment of the electron.
Finally, I hope to have shown that the way the story of Dancoff's mistake is usually presented can be traced back to two primary sources, and thus ultimately to two misinterpretations: One - Tomonaga's misreading of the use of renormalization in Dancoff's paper - arising from exaggerated benevolence, the other - the distorted accounts of Oppenheimer, Bloch and Serber - arising from frustration over what appeared in hindsight as a huge missed opportunity, along with a convenient (and soon deceased) scapegoat. In summary, Dancoff's work, rather than offering an example of how obvious the innovations of renormalized, covariant QED were in hindsight, and how easily they could have been performed earlier, Dancoff's work provides a perfect focal point for the study of QED in the 1930s, its successes and limitations, and the divide, both in concepts and in methods, that separated it from the renormalized QED of the 1940s. It also provides a perfect example of how the myth of the supposed obviousness of the advances of the late 1940s was fostered, both by the gracious victors[39] and the disappointed losers.

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