

# The Real Problem with Perturbative Quantum Field Theory

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## Abstract

The perturbative approach to quantum field theory (QFT) has long been viewed with suspicion by philosophers of science. This paper offers a diagnosis of its conceptual problems. Drawing on Norton's ([2012]) discussion of the notion of approximation I argue that perturbative QFT ought to be understood as producing approximations without specifying an underlying QFT model. This analysis leads to a reassessment of common worries about perturbative QFT. What ends up being the key issue with the approach on this picture is not mathematical rigour, or the threat of inconsistency, but the need for a physical explanation of its empirical success.

*1 Three Worries about Perturbative Quantum Field Theory*

*2 The Perturbative Formalism*

*2.1 Expanding the S-matrix*

*2.2 Perturbative renormalization*

*3 Approximations and Models*

*4 Perturbative Quantum Field Theory Produces Approximations*

*5 The Real Problem*

## 1 Three Worries about Perturbative Quantum Field Theory

On the face of it, the perturbative approach to quantum field theory (QFT) ought to be of great interest to philosophers of physics.<sup>1</sup> Perturbation theory has long

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<sup>1</sup>I use the terms 'perturbative approach to QFT' and 'perturbative QFT' here to refer to the perturbative treatment of interacting field theories that is invariable found in QFT textbooks and forms the basis of much work in high energy physics. I am not assuming that this 'approach'

played a special role in the QFT programme. The axiomatic and effective field theory approaches to QFT, which have been the locus of much philosophical attention in recent years, have their roots in the perturbative formalism pioneered by Feynman, Schwinger and Tomonaga in the 1940s. Engaging with perturbative QFT thus has the potential to shed light on these later developments.<sup>2</sup> A more flat footed reason to pay attention to this framework is that it remains the most important source of empirical predictions in high energy physics. The famously accurate prediction of the anomalous magnetic moment of the electron is just one among many perturbative results which form the backbone of particle physics phenomenology. Consequently, when it comes to the question of what we ought to believe about the world given the empirical success of the standard model the perturbative apparatus that produces much of this success will surely have to be addressed in one way or another.

Philosophers have tended to shy away from perturbation theory in their investigations of QFT however.<sup>3</sup> To some extent this reticence is understandable: the standard perturbative formalism has seemed to many, physicists and philosophers alike, to be deeply suspect from a foundational perspective. There are, I suggest, at least three distinct sorts of worry voiced in the philosophical literature, which can be roughly glossed as follows (providing a more detailed diagnosis of these concerns will be the central project of this paper):

- i) *The rigour problem.* A common complaint raised against the perturbative approach to QFT is that it is mathematically unrigorous. Among philosophers this critique is typically coupled with the methodological stricture that a certain standard of mathematical rigour is necessary (or at least strongly desired) before interpretive work can get off the ground. This attitude towards perturbative QFT is likely behind Halvorson’s comments about the lack of a “mathematically intelligible description of QFT” underlying the practice of mainstream high energy physics (Halvorson, [2006], 731). Fraser ([2009]) raises this sort of objection more explicitly in her discussion of what she calls the “infinitely renormalized” variant of QFT, which she associated with the practice of taking momentum space cutoffs to infinity during

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should be regarded as a freestanding formulation of QFT, and therefore a ‘theory’ in the sense of mainstream philosophy of science. Indeed, this question looms large in the following discussion, and is ultimately answered in the negative. Still, I take it that the term has a relatively clear referent at the level of scientific practice.

<sup>2</sup>One motivation for the present work is to lay some groundwork for debate over the significance of renormalization group methods in QFT. As I briefly discuss in section 5, the renormalization group, and the effective field theory approach it spawned, are often said to have improved the conceptual standing of renormalization theory. To substantiate this claim however we need to know what was wrong with the original, purely perturbative, conception of renormalization in the first place, which is a key concern of this paper.

<sup>3</sup>There are, of course, exceptions. Teller ([1989], [1995]) and Huggett and Weingard ([1995]) examine perturbative renormalization from a philosophical perspective. There has also been discussion of virtual particles (Weingard [1988]; Redhead [1988]) and Feynman diagrams (Meynel, 2008; Wüthrich, 2012) in the philosophical literature, which engages with QFT perturbation theory.

perturbative renormalization (a process I will describe in the next section). Fraser argues that this procedure is ill defined and complains that “the standard criticism levelled against unrigorous theories—that they are difficult to analyse and interpret—certainly applies in this case” (Fraser, 2009, 543). One putative obstacle to engaging with perturbative QFT then is that it is on too flimsy ground mathematically for foundational issues to be properly addressed.<sup>4</sup>

- ii) *The consistency problem.* An even more severe charge brought against the standard perturbative framework is that it runs foul of inconsistency. A notorious result due to Haag, Hall and Wightman—henceforth Haag’s theorem—seems to show that standard perturbative calculations rest on an inconsistent set of assumptions. While Earman and Fraser ([2006], 306) want to allay worries about the consistency of interacting QFTs in general in light of Haag’s theorem, they claim that the result does “pose problems for some of the techniques used in textbook physics for extracting physical prediction from the theory”; it is clearly perturbative scattering calculation which they have in mind here. Fraser even suggests that physicists may be tacitly employing inferential restrictions to avoid deriving contradictions when they perform perturbative calculations (Fraser, 2009, 551). The threat of inconsistency provides further reason to be wary of taking the perturbative formalism seriously from a foundational and philosophical perspective.
- iii) *The justification problem.* Even putting these concerns about the internal coherence of perturbative QFT aside however, a further puzzle remains: many of the steps involved in standard perturbative calculations appear to be problematically ad hoc. Perturbative renormalization in particular is often presented in textbooks as a mathematical trick which removes divergences in the coefficients of naive power series expansions. But why are these infinities there in the first place, and what justifies the procedure used to remove them? This concern is what Wallace is getting at when he says that the original perturbative approach to renormalization made little “physical sense” (Wallace 2011, 117) and similar complaints about the lack of a physical picture underlying the renormalization procedure can be found dotted throughout the philosophical literature.<sup>5</sup> A final reason for trepidation about philosophically engaging with the perturbative approach to QFT then is the apparent lack of justification for the various manipulations and assumptions that go into setting up the formalism.

In the face of these problems one might conclude that philosophers should sim-

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<sup>4</sup>Evaluating this challenge would seem to require delving into the broader issue of how mathematical rigour should be characterised, and what its significance is for the philosophical investigation of physical science. I do not go down this route here, as my suggestion will be that concerns about the mathematical rigour of perturbative QFT stem, at least in part, from a misinterpretation of the perturbative approach.

<sup>5</sup>McMullin ([1985], 261), for instance, cites renormalization in QFT as a paradigm case of a modification to a theory which lacks a physical justification.

ply remain silent about QFT perturbation theory, at least until physicists and mathematicians have developed a more coherent understanding of it. This paper puts forward a different response. Drawing on Norton's ([2012]) discussion of the notion of approximation I argue that the perturbative framework should be understood as a method for producing approximations without addressing the project of constructing interacting QFT models. Adopting this view of the perturbative approach leads to a reassessment of all three of the worries just identified. The rigour and inconsistency problems, in particular, lose much of their bite. Perturbation theory does not provide a structural characterisation of realistic QFTs, not because of a lapse in mathematical rigour, but because this was not its aim in the first place. Once we realise this, I suggest, the mathematical sloppiness we do find in perturbative calculations in the physics literature becomes a less pressing foundational concern. This analysis also helps to make clear why Haag's theorem is not as disastrous for the perturbative framework as it initially seems. In brief, the result does not undermine standard perturbative calculations because they do not posit the existence of a model satisfying the relevant set of inconsistent assumptions.

It does not do away with the justification problem however. What ends up being the really salient puzzle about QFT perturbation theory is why it is so successful—why, that is, the approximations it produces are often staggeringly good ones. I will suggest, however, that this is a problem which philosophers can contribute to rather than a reason to eschew discussion of perturbation theory. The plan for this paper is as follows. Section 2 introduces the perturbative formalism. Section 3 sets out the distinction between models and approximations I make use of in general terms. Sections 4 and 5 make the case for understanding QFT perturbation theory as producing approximations and discusses the ramifications of this view for the three putative problems just outlined.

## 2 The Perturbative Formalism

Perturbative QFT is a huge subject in its own right. I focus here on general aspects of the approach which are most relevant to assessing the aforementioned worries about its foundational respectability. I first describe how perturbative expansions of QFT observables are set up, stressing the role of the interaction picture and the apparent challenge posed by Haag's theorem. I then discuss the divergences which appear when this method is applied naively and the renormalization procedure which is used to remove them.

## 2.1 Expanding the S-matrix

The key object we are trying to get at in the perturbative approach to QFT, at least in the first instance, is the S-matrix.<sup>6</sup> In scattering theory, scattering events are represented as transitions from localised initial and final states at asymptotic times. The S-matrix is the operator which maps incoming states  $|\alpha\rangle_{in}$  at  $t \rightarrow -\infty$  onto outgoing states  $|\beta\rangle_{out}$  at  $t \rightarrow \infty$ :

$$S_{\beta\alpha} = {}_{out}\langle\beta|S|\alpha\rangle_{in}. \quad (1)$$

The S-matrix is of paramount importance to particle physics phenomenology because S-matrix elements, associated with particular classes of in and out states, are closely related to scattering cross sections, the quantities measured in collider experiments.<sup>7</sup>

There are two major obstacles to getting at the S-matrices of realistic QFTs, such as quantum electrodynamics (QED) and the standard model. On the one hand, there is the issue of giving concrete meaning to the S-matrix. To substantiate the above definition a precise characterisation of the in and out states, and the space in which they live, is needed. Identifying mathematical structures which are constitutive of empirically supported QFTs is a task plagued by technical and conceptual hurdles however. Axiomatic formulations of QFT give us well motivated characterisations of what a QFT system formulated on Minkowski space-time should look like, but, so far at least, interacting models of these axioms have only been constructed for toy theories in a reduced number of dimensions. We can explicitly construct cutoff formulations of empirically supported QFTs, in which degrees of freedom associated with variations on arbitrarily small length scales are explicitly removed—by putting the theory on a lattice for instance. But the foundational status of these cutoff models is controversial; this is the crux of the recent debate between Doreen Fraser (Fraser, 2011) and David Wallace (Wallace, 2011).

Besides this issue however, interacting field theories inevitably raise more practical concerns. Interaction terms lead to non-linear equations of motion, and exact solutions to empirically successful QFTs are typically out of the question. However they are characterised structurally then, there is going to be a difficulty with computing the S-matrix elements of theories like QED in practice.

Faced with these problems, the perturbative strategy is to use what we already know about free QFTs to generate expressions for the S-matrix elements of weakly interacting theories. The construction of free QFT models is much better understood than their interacting counterparts. We know how to write down continuum models in this case, and these theories can be exactly solved; we can determine the system's spectrum, get explicit Fock space representations of the field operators and obtain the S-matrix analytically (of course, the S-matrix of a free field theory

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<sup>6</sup>Other important quantities, and especially correlation functions, are also evaluated perturbatively once we have the expansion of the S-matrix up and running.

<sup>7</sup>See Peskin and Schroeder ([1995]) and Duncan ([2012]) for a more careful exposition of QFT scattering theory and its role in setting up the perturbative expansion.

is trivial). The idea then is to split the Hamiltonian of an interacting theory into a free part,  $H_0$ , and an interaction potential,  $V$ , parameterised by a ‘coupling’  $g$ :

$$H = H_0 + gV. \quad (2)$$

We then construct power series in  $g$  for the theory’s S-matrix elements whose coefficients can be computed using the explicit representations of the fields afforded by the exact solution of the free model associated with  $H_0$ . If  $g$  is sufficiently small the hope is that the first few terms of this series yield accurate predictions of experimentally observed scattering cross sections.

In order to get these expansions up and running though we need to invoke the so-called interaction picture. As is familiar from quantum mechanics there are multiple ways to implement the time evolution of a quantum system. In the Schrödinger picture the state evolves in time while the operators remain constant; in the Heisenberg picture the operators take on the time dependence and the states are constant. The idea behind the interaction picture is to use this freedom to isolate the free representations of the fields. If  $A_S$  and  $\psi_S(t)$  are operators and states in the Schrödinger picture the corresponding operators and states in the interaction picture are given by  $A_I(t) = e^{iH_0, st} A_S e^{-iH_0, st}$  and  $|\psi_I(t)\rangle = e^{iH_0, st} |\psi_S(t)\rangle$ . This means that the field operators are governed by  $H_0$  while the remaining time dependence due to the interaction potential is shifted into the states.

The time evolution operator in the interaction picture,  $|\psi_I(t)\rangle = U_I(t, t_0) |\psi_I(t_0)\rangle$ , is then used to set up perturbation series for particular S-matrix elements.  $U_I(t, t_0)$  can be expanded in a power series in  $g$  of the form:

$$U_I(t, t_0) = \sum_{n=0}^{\infty} \frac{(-ig)^n}{n!} \int_{t_0}^t dt_1 \dots \int_{t_0}^t dt_n T[V_I(t_1) \dots V_I(t_n)], \quad (3)$$

where  $T$  denotes the time order product here, which arranges operators in descending order with respect to their time arguments. This expansion can then be plugged into the definition of the S-matrix:

$$S_{\beta\alpha} = \lim_{t_i \rightarrow -\infty} \lim_{t_f \rightarrow \infty} \langle \beta | U_I(t_f, t_i) | \alpha \rangle. \quad (4)$$

Taking the in and out states to be eigenstates of  $H_0$ , on the grounds that the system is effectively isolated before and after the scattering event, we end up with a string of terms made up of Fock space operators acting on the free field vacuum state—expressions that we know how to compute, at least in principle.

What we typically need to do to work out the coefficients of the series at each order in  $g$  is evaluate a set of integrals over momentum space. Feynman diagrams bring some order to the proceedings, but the number and complexity of these integrals grows rapidly as the series proceeds and the best we can hope to do in practice is calculate the first few terms. As is now well known however, the integrals which result from naively following the prescription just described typically diverge. The next section describes the renormalization procedure that is needed to deal with these divergences.

Before we move on to discuss renormalization however there is another issue with the expansion technique as we have described it thus far which needs to be addressed: the apparent clash with Haag’s theorem. What Haag’s theorem shows is that, given assumptions common to all of the main axiomatic formulations of QFT, the interaction picture does not exist.<sup>8</sup> The root of the problem is that continuum QFT models associated with the free, and fully interacting, Hamiltonians,  $H$  and  $H_0$ , cannot be formulated in the same Hilbert space. Quantum systems with infinitely many degrees of freedom admit unitarily inequivalent Hilbert space representations, and Haag’s result flows from the fact that the vacuum state of the free and interacting theories live in unitarily inequivalent spaces. This means that there cannot be a global unitary transformation connecting the states and field operators of a free and interacting theory, which the interaction picture is clearly predicated on. Given the role that the interaction picture plays in setting up the power series expansion of the S-matrix, Haag’s theorem has seemed to some to point to a fundamental inconsistency in the perturbative method—this is the origin of the worries about consistency flagged in section 1.

## 2.2 Perturbative renormalization

To make things concrete let’s consider applying the method just described to a well understood toy model— $\phi^4$  theory (on four dimensional Minkowski space-time). The theory is defined by the Lagrangian:

$$\mathcal{L} = \frac{1}{2} [(\partial_\mu \phi)^2 - m^2 \phi^2] - \frac{1}{4!} \lambda \phi^4. \quad (5)$$

where  $\phi$  is a scalar field,  $m$  is its associated mass and  $\lambda$  is a positive constant parameterising a quartic interaction. In the absence of exact solutions, the hope is that expanding the S-matrix of the theory in powers of  $\lambda$  will at least allow us to probe the region of the parameter space in which  $\lambda$  is small and the interaction is weak. Following the prescriptions set out in the previous section however we end up having to evaluate momentum space integrals of the form,<sup>9</sup>

$$\int d^4 q \frac{1}{(\vec{q}^2 + m^2)^2}, \quad (6)$$

in order to calculate the perturbative coefficients at second order in  $\lambda$ . This integral diverges, leaving the expansion ill defined and predictively useless. The same kind of divergent integrals arise when the perturbative method is applied to realistic gauge theories such as QED.

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<sup>8</sup>The original result can be found in Haag ([1956]). It is Hall and Wightman’s ([1957]) generalisation however which is nowadays commonly referred to as Haag’s theorem. The assumptions needed to prove it are discussed in detail in Earman and Fraser ([2006]) and Miller ([2017]).

<sup>9</sup>Note that this is actually the form of a loop integral on four dimensional euclidean space. A common technique in evaluating perturbative coefficients is to work in Euclidean space and analytically continue the results back to Minkowski space-time by means of the so-called Wick rotation—see Peskin and Schroeder ([1995], 193).

The renormalization procedure used to obtain a sensible power series divides into three steps. The first is to ‘regularize’ the offending integrals—that is render them convergent. In the case of the integral above the most conceptually straightforward way of doing this is to taking the upper limit of integration to be some finite constant  $\Lambda$ , known as an ultraviolet perturbative cutoff<sup>10</sup> (this is not the same thing as constructing a cutoff QFT model however, as I will discuss in section 4). It can then be explicitly evaluated as a function of  $\Lambda$  which goes to infinity as  $\Lambda \rightarrow \infty$ . Terms of this kind are said to be ultraviolet divergent. The perturbative expansions of theories with massless particles, like QED photons, also contain infrared divergent integrals that blow up as momentum variables are integrated down to zero. In this case the integral can be regularized by taking the lower limit of integration to be some small but non-zero constant—an infrared perturbative cutoff.

Once the coefficients of the perturbation series can be manipulated as finite expressions the next step is to redefine the coupling in which we are expanding so as to remove the singular dependence on the cutoff—it is to this process that the term renormalization properly refers. In the case of  $\phi^4$  theory we can do this by reparameterising the Lagrangian as follows:

$$\mathcal{L} = \frac{1}{2}[(1 + \delta Z)(\partial_\mu \phi_r)^2 - (m_r + \delta m)^2 \phi_r^2] - \frac{1}{4!}(\lambda_r + \delta \lambda)\phi_r^4. \quad (7)$$

Where  $\phi_r = (1 + \delta Z)^{-1/2}\phi$  is the renormalized field,  $m_r$  and  $\lambda_r$  are the renormalized mass and charge, and  $\delta Z, \delta m, \delta \lambda$  parameterise so-called counterterms. Note that nothing mathematically or physically dubious is going on here. The Lagrangian has simply been rewritten in terms of different variables. The value of the counterterm parameters make no difference to the dynamics described by the Lagrangian. It does make a difference to the terms of the perturbation series at each order however: both the renormalized expansion parameter and coefficients of the series depend on the choice of counterterms. It turns out to be possible to choose these factors in such a way that the part of the series’ coefficient at second order which diverges as  $\Lambda \rightarrow \infty$  is completely removed. In fact, this can be done at each order in perturbation theory.

Theories whose ultraviolet divergences can be systematically eliminated via a redefinition of a finite number of parameters in the Lagrangian in the manner just sketched are said to be renormalizable (showing that infrared divergences can also be removed from the perturbation series is another matter which will not be discussed in detail here). The QED and standard model Lagrangians have this property, and consequently the ultraviolet divergences which appear in the

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<sup>10</sup>In practice a variety of other regularization schemes are used. One method which is often more convenient in the treatment of gauge theories like QED, and has also sometimes been attributed special foundational significance in the philosophical literature (Bain, 2013), is dimensional regularization. In this approach the integration measure is modified so as to range over a fractional number of dimensions,  $4 - \epsilon$ . For ultraviolet divergent integrals this leads to finite results for  $\epsilon > 0$ , with divergences manifesting as poles at  $\epsilon = 0$ . Infrared divergences can also be regularized in this way.

perturbation series can be completely removed at all orders. In fact, the renormalizability of these theories is no coincidence. Traditionally renormalizability was seen as a necessary condition for a viable perturbative treatment of a theory and was used as a theory selection principle during the formulation of the standard model. There is something puzzling about demanding renormalizability a priori in this way however. What licenses us to assume that the world is amenable to perturbative analysis?

Even putting aside the question of renormalizability however, there seems to be something odd about the redefinition prescription just described. How can a change of variables transform the physical respectability and predictive power of the expansion? It's worth pointing out here that the fact that a redefinition of the coupling can affect the properties of the resulting series is not surprising in itself. In general, the quality of a power series approximation can be very sensitive to the choice of expansion parameter. Suppose we want to expand  $\ln(1+x)$  as a power series, for instance. Expanding in  $x$  gives rise to a series which converges for  $|x| < 1$ , while making a change of variables and expanding in  $x' = x/(x+2)$  produces a series which converges for any positive value of  $x$ , and typically converges much more rapidly. Moreover, the practice of renormalizing the coupling parameter is often employed in the perturbative treatment of models in ordinary quantum mechanics which have no problems with divergences. In the treatment of the quantum anharmonic oscillator for instance, the most obvious choice of coupling leads to a divergent series, but redefining the coupling, in essentially the same manner just sketched for interacting QFTs, can produce a convergent series.<sup>11</sup> The redefinition procedure just described has a thoroughly ad hoc character however. No physical reason has been given for removing the infinities, or for thinking that the resulting finite expressions should provide good empirical predictions. At best, we have a purely instrumental justification after the fact, given that renormalized perturbation theory does in fact turn out to be empirically successful.

The final step of standard perturbative calculations is to remove the cutoffs. If the redefinition of the coupling has done its job this limit is well-defined and we obtain finite expressions from truncations of the series at each order. This is sometimes presented as returning us to the realms of continuum field theory after a detour through strange regulated theories. As I will shortly be arguing, this way of speaking is quite misleading—removing the regulator on integrals in the perturbation series does not amount to constructing a continuum QFT model. While there are good practical motivations for removing the cutoff—integrals over all of momentum space are, on the whole, much easier to deal with than cutoff expressions—the physical justification for removing the regulator is, once again, unclear.

Despite its ad hoc character the renormalization procedure has produced some of the most successful predictions in the history of science. As has already been mentioned, what happens in practice is that the series is truncated after the first

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<sup>11</sup>Delamotte ([2004]) and Neumaier ([2011]) give useful discussions of renormalization in this more general context—the  $\ln(1+x)$  example is borrowed from Neumaier ([2011]).

few terms and is then fed into calculations of measurable quantities like scattering cross sections. A final puzzling feature of renormalized perturbation series however is their large order behaviour. There is overwhelming evidence that, even after each term in the series has been rendered finite by renormalization, the sum of the series at all orders diverges. Dyson ([1952]) was the first to give heuristic arguments to the effect that renormalized QFT perturbation series have zero radius of convergence and this has since been borne out by studies of the large order behaviour of the perturbation series of realistic QFTs, as well as rigorous results in constructive field theory (Strocchi, 2013, 35-39). Of course, the apparent convergence of QED perturbation theory in the first few orders, and their excellent agreement with experiment, assure us that the expansion is, at least, an asymptotic series. But the fact that it diverges prevents us from directly identifying its sum with the S-matrix elements of the theory. This is perhaps one of the features of QFT perturbative expansions which contribute to their reputation for a lack of mathematical rigour.

In sum then, our survey of the perturbative approach to QFT has identified *prima facie* motivations for the rigour, consistency and justification problems identified in section 1.<sup>12</sup> In order to assess the substance of these worries however a more careful analysis of what is going on in the perturbative method is needed. The next section lays the groundwork for such an analysis by introducing some ideas drawn from the philosophical literature on scientific modelling, specifically the distinction between approximations and models.

### 3 Approximations and Models

The best way to get a handle on the notions I want to make use of here is with reference to examples.<sup>13</sup> Take a familiar classical system: a unit mass falling in a uniform gravitational field while acted upon by a linear air resistive force. The equation of motion of this system can be written in terms of the velocity:

$$dv/dt = g - kv, \tag{8}$$

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<sup>12</sup>There are also other puzzling features of the perturbative formalism which have not been discussed here. One issue is the so-called renormalization scheme dependence problem. In addition to removing the divergent part of the perturbative coefficients in the second, redefinition phase of the renormalization procedure, we can also subtract an arbitrary finite part. And while this does not make a difference to physical quantities it does make a difference to truncations of the series which are compared to experiment, apparently rendering them problematically ambiguous. I hope to examine this issue further in future work.

<sup>13</sup>Many of the examples here are drawn from Norton ([2012]). Note that Norton uses the term *idealization* to refer to a physical system which is used to represent some target system. As Norton admits, this is often what philosophers of science mean by the term *model*, and I will mostly use this later term here. Strictly speaking, when I talk about models of the Wightman axioms, I am using the term in a different (model theoretic) sense. Most of the time this ambiguity won't matter however, as the QFT systems of interest will be models in both senses of the word.

where  $g$  is the gravitational acceleration and  $k$  parameterises the frictional force. Assuming the body is initially at rest the solution of this equation is:

$$v(t) = g/k(1 - e^{-kt}). \quad (9)$$

Suppose, however, that air resistance is not very important for the aspects of the system's behaviour we are interested in describing. Perhaps  $k$  is small relative to  $g$ , and we are only interested in the system's evolution over comparatively short time periods.

There are two paths one might take in this situation. On the one hand, we could consider a new classical system, with a simplified equation of motion  $dv/dt = g$ , and treat it as a representation of our original system. This model clearly misrepresents its target in some respects; it falsely represents  $k$  as being zero and has quite different asymptotic behaviour as  $t \rightarrow \infty$ , since it lacks a terminal velocity. But it does get some of the features of the target system right—the velocity function of the idealized model will stay within some error bound of the target system's velocity over some finite time period. There is another way to proceed however. Suppose that instead of moving to a new idealised system we take the limit  $k \rightarrow 0$  of the velocity function of the original system:

$$\lim_{k \rightarrow 0} v(t) = gt \quad (10)$$

This produces a function which is, again, within some error bound of the target system's velocity over sufficiently short periods of time. It can therefore be viewed as an inexact description of this property of the system. Following Norton ([2012]), I will call the act of using a function in this way, without referring to a new physical system, an approximation.

This distinction between representing a system with an idealized model and providing an inexact description of one of its properties directly has long been recognised in the literature on scientific modelling. Its significance is less agreed upon however. Redhead ([1980]) claims that the two approaches are always interchangeable, in the sense that, for any function which approximates a quantity of a target system we can construct a system to which it is an exact solution. This certainly does seem to be the case in the above example, since the approximation obtained by taking  $k \rightarrow 0$  is identical to the velocity function of the corresponding idealized model. Following examples like these, philosophers of science have tended to see models as the fundamental unit of analysis and approximations as derivative upon them.

Norton ([2012]) has recently put forward a different view of the distinction which I want to endorse here, namely that approximation and idealization are genuinely distinct representational strategies, and that distinguishing between them can play an important role in resolving some puzzles in the philosophy of physics.<sup>14</sup> Norton presents a number of counterexamples to the idea that every approximation piggy

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<sup>14</sup>Norton's discussion of the renormalization group framework in statistical mechanics might seem to have a more direct relevance to the present project. However, while the renormalization

backs on a corresponding idealized model. Take, for instance, an ellipsoid with semi-major axis of length  $a$ . The ratio of the surface area and volume of this geometric object can be expressed as a function of  $a$ . If we take the limit  $a \rightarrow \infty$  of this function it converges to  $3\pi/4$ ; this might be an appropriate approximation if  $a$  is very large. If we take the semi-major axis of the ellipsoid itself to infinity however we get an infinite cylinder whose surface area to volume ratio is 2. In this case applying an operation to a quantity defined on a model and applying the analogous operation to the model itself gives different results and we clearly cannot say that the former is underwritten by the latter.

A second class of examples which speak against Redhead's equivalence claim are cases in which no model exists corresponding to an operation used to implement an approximation. Norton discusses the example of a sphere of radius  $r$ . The ratio of surface area to volume of a sphere is  $3/r$ , which clearly has a finite limit as  $r \rightarrow \infty$ . But there is no such thing as a sphere with infinite radius.<sup>15</sup> While it may be appropriate to treat the area to volume ratio of a large sphere as vanishing then, there is no geometric object which actually has this property. We also find cases of this sort in statistical physics. A common modelling strategy in statistical mechanics is to take the so-called thermodynamic limit, in which a system's volume is taken to infinity, with quantities like the energy density held constant. Once again, there is an ambiguity in how this practice should be understood. One could take this limit of a particular quantity, or actually construct an infinitely extended system by taking the boundary of a finite system to spatial infinity in a controlled way. The latter task often turns out to be difficult to do in practice: the existence of a limit system typically has to be established on a case by case basis, and is known not to hold for gravitating systems.<sup>16</sup> Again, it might be that the  $N \rightarrow \infty$  limit is well-defined for particular quantities despite the fact that there is no infinite volume system.

Apart from counterexamples of this kind, another motivation for taking approximations and models to be distinct theoretical outputs is that this often leads to a more natural interpretation of scientific practice. The previous examples attest to the fact that it is typically more difficult to establish the existence of a physical system than a single function. And, as Norton points out, we often find

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group is arguably very important in making sense of perturbative renormalization, as I urge in section 5, Norton's claim that the study of renormalization group fixed points should be analysed in terms of the notion of approximation, and my claim in this paper that perturbative QFT produces approximations, are independent theses.

<sup>15</sup>At least on the standard definition of a sphere in  $\mathbb{R}^3$ : the set of points a distance  $r$  away from a given (center) point. Since no points in  $\mathbb{R}^3$  are an infinite distance apart, there is no sphere with  $r = \infty$ . It may be possible to obtain a sensible "infinite sphere" by working with a different definition or modifying the background space however. In general, the question of whether a limit system exists depends on the details of how the limit in question is treated, which is often left ambiguous in less rigorous discussions: see Fraser ([2016]), section 4.3, for more on this point in the QFT context.

<sup>16</sup>Ruelle ([1969]) is a classic resource for rigorous results about the existence of the thermodynamic limit. Callender ([2011]) provides a discussion of the non-existence of the thermodynamic limit in the case of gravitational systems.

that physicists do not do this extra work; he argues, for instance, that in many applications the  $N \rightarrow \infty$  limit is taken of particular thermodynamic functions without addressing questions about the existence and properties of the corresponding infinite system. Those who hold that approximations are underwritten by the presence of a corresponding model will be forced to reproach physicists for their sloppiness in such cases. But if we give up this idea, as Norton's examples suggest we should anyway, a more sympathetic reading becomes available. On this view, producing a function which approximates some property of the target system, and producing a model which resembles it in certain respects are simply different things—the former does not rest on the latter. Consequently, there is nothing wrong with using approximate expressions for physical quantities without embedding them within a theoretical model.

It is the capacity of an emancipated notion of approximation to illuminate scientific practice which I want to draw on here. In the context of the QFT programme, the existence of QFT models is a delicate issue and consequently the construction of approximations and models naturally come apart. This, I think, is the key to a more fruitful understanding of perturbative QFT.

## 4 Perturbative Quantum Field Theory Produces Approximations

The key claim of this paper is that perturbative QFT ought to be understood as a method for producing approximations in the sense just elaborated, without picking out QFT models. The truncated power series obtained by following the prescriptions set out in section 2 can be used to approximate physical quantities like scattering cross sections, but the various steps involved in getting to these expressions should not be interpreted as an attempt to provide a structural characterisation of an interacting QFT. The central motivation for this view is that it makes sense of what physicists actually do, or perhaps more importantly do not do, when they treat QFTs perturbatively. As has already been mentioned constructing interacting QFT models is a difficult task. Looking for a solution to this problem in the standard perturbative approach leaves us disappointed. At each juncture in the perturbative method we find that the work needed to specify a physical model is missing.

Consider, for instance, the regularization of the momentum space integrals contributing to the perturbative coefficients. Imposing a high momentum cutoff on such an integral is sometimes described, in both the physics and philosophy literature, as taking us to a cutoff theory. In light of the distinction drawn in the previous section however, we can see that putting a cutoff on a single expression and constructing a quantum system which lacks high momentum degrees of freedom is not the same thing. In fact, early applications of renormalized perturbation theory made no attempt to verify the existence of such systems, or explore their

properties. The details of how to formulate QFTs on a lattice have since been worked out, and other ways of implementing a ‘non-perturbative’ cutoff have been developed in the constructive field theory programme. But this came decades after the original perturbative treatment of QED.<sup>17</sup>

Another place where the perturbative approach is apt to disappoint those looking for a class of mathematical structures to identify with realistic QFTs is the removal of the regulator. I have already suggested that it is misleading to talk about taking a continuum limit here, and we can now see why. All that is happening when an ultraviolet cutoff is taken to infinity in a typical perturbative calculation is that a limit is being taken of a particular truncated power series expression. That a great deal more is needed to verify the existence of a continuum QFT model is evident when we look at the work that goes into constructing models of the Wightman or Haag-Kastler axioms. What mathematical physicists working on this problem typically do is start with a cutoff QFT model, with non-perturbative cutoffs at high and low momentum—a theory defined on a finite volume lattice for instance—and attempt to take the continuum and infinite volume limits of this structure. The difficulties associated with taking these limits in a controlled way outstrip, and are in fact largely independent of, the problem of ridding perturbative expansions of divergences.

The continuum limit, in particular, is best understood within the renormalization group framework—see Hancox-Li ([2015a]) for a detailed discussion. The renormalization group gives us a way of studying the behaviour of a theory at different length/energy scales which is, in principle at least, entirely non-perturbative. In this case we are interested in how the couplings of a theory behave in the region of arbitrarily high energies. The existence of a non-trivial continuum limit is typically taken to be conditional on the existence of a so-called ultraviolet fixed point. What this means, in essence, is that as the lattice spacing is decreased towards zero, the couplings of the theory need to converge to zero (asymptotic freedom), or some finite value (asymptotic safety). Whether this occurs or not, however, is entirely independent of the property of perturbative renormalizability: renormalizable theories like QED and  $\phi^4$  theory are thought to lack an ultraviolet fixed point, while non-renormalizable theories, such as naive quantizations of general relativity, may have one.<sup>18</sup> Moreover, the treatment of infrared divergences in the perturbation series of gauge theories like quantum chromodynamics does not

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<sup>17</sup>The lack of any attempt to construct a quantum system corresponding to a regularized integral is even more evident in the case of dimensional regularization mentioned in footnote 7. This method works by analytically continuing the integration measure to a non-integer number of dimensions. Again, one sometimes finds talk of quantum theories with a fractional number of dimensions in this context. Yet (to my knowledge) no attempt has been made to actually construct QFT models of this kind, and dimensional regularization techniques do not play any role in extant work in the constructive field theory. Rivasseau ([2014], 7) comments that dimensional regularization “cannot be used up to now in a constructive non-perturbative program”.

<sup>18</sup>There is a whole programme in the quantum gravity literature exploring the possibility that quantizations of general relativity have an ultraviolet fixed point despite being non-renormalizable—see Niedermaier and Reuter ([2006]) for a review.

solve the problems associated with bringing its infrared behaviour under mathematical control in the non-perturbative context. In sum then, removing the cutoffs on momentum space integrals in the perturbative renormalization procedure has very little to do with the project of constructing continuum QFT models.

A final blow for those searching for explicit constructions of QFT models in the perturbative framework is the divergence of the expansion. If renormalized perturbation series converged we could use their sums to define S-matrix elements and construct QFT models from there. Alas, they do not. This may not rule out the possibility of extracting a structural characterisation of interacting QFTs from renormalized perturbation series entirely—there are, after all, a number of ways of defining sums for divergent series.<sup>19</sup> But it does, at least, show that it is a non-trivial undertaking. And again, is not a project which we find high energy physicists working with the perturbative formalism engaging in. In most practical calculations particle physics phenomenologists simply truncate the perturbation series after the first few terms and do not concern themselves with the fate of the sum to all orders. Once again, no attention is being given to the existence of interacting QFT models.

This all makes sense if we take the intended output of the perturbative approach to be approximations rather than physical models. On this reading the strategy underlying the perturbative treatment of interacting QFTs is to dodge rather than solve the problem of how to characterise the theory in terms of mathematical structures. While philosophers of a realist persuasion in particular will find this troubling—a point I will come back to shortly—there is nothing manifestly incoherent about it if we accept the conclusions of the previous section. I argued that it is a mistake to think that approximations must be embedded within a model in order to be meaningful. We should not berate particle physics phenomenologists then for failing to tell us what an interacting QFT is. Obtaining approximate expressions for scattering cross sections and constructing QFT models are different objectives; in principle the former can be pursued without addressing the latter.

A great advantage of this reading is that it opens the way for more constructive philosophical engagement with perturbative QFT. The analysis I have developed here sheds new light on the three putative problems with the perturbative approach I distinguished in section 1 and ultimately leads to a less pessimistic assessment of the conceptual respectability of the perturbative framework.

One of the complaints about perturbative QFT, which I called the rigour problem, was that its lack of mathematical rigour makes it impossible to engage with from a foundational perspective. This objection is usually left relatively vague;

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<sup>19</sup>It turns out that the perturbation series of interacting theories which have been constructed as models of the Wightman axioms in a reduced number of space-time dimensions have a unique Borel sum which is identical to the exact values of the corresponding S-matrix elements—see Rivasseau ([2014]) for a discussion of these results. There are good reasons to think that this behaviour does not generalise to the case of realistic QFTs however, for a review of this issue see Duncan ([2012]) chapter 11. Thanks to Michael Miller for discussions about this issue.

we rarely find philosophers pointing to specific aspects of the perturbative method which they find problematically unrigorous. I suspect, however, that what is often driving this sort of worry is the absence of an explicit specification of a class of mathematical structures underlying the approach. As I discussed in the previous section, philosophers of science have often viewed approximations as derived from, and underwritten by, physical models. Given what has been said already about perturbative QFT it is easy to see how a follower of this doctrine might perceive it as hopelessly sloppy, and attribute this to an unrigorous treatment of the relevant mathematics. As I mentioned in section 1, Fraser identifies an “infinitely renormalized” variant of QFT which is associated with the process of imposing and removing a cutoff in the perturbative renormalization procedure. According to her this amounts to adding infinite counterterms to the Lagrangian and leads to a mathematically ill defined system.

Rather than reading the perturbative approach as a botched attempt at constructing QFT models however, I have been arguing that it is much more natural to interpret it as never taking up this project in the first place. Understood as a method for producing approximations, the perturbative approach is more difficult to dismiss as mathematically unsound. This is not to say that standard perturbative computations are paragons of mathematical precision. The treatment of the convergence of momentum space integrals in the physics literature, for instance, often falls short of the standards of rigour upheld by mathematicians. But this is the kind of imprecision that is ubiquitous in applied mathematics—if we reject the perturbative approach on these grounds we will also be jettisoning much of physical science.<sup>20</sup> The perturbative approach has been seen as mathematically problematic in a more radical sense, I think, because of the lack of attention paid to the existence of QFT models. On the view I have been developing here however this has nothing to do with mathematical rigour and instead reflects the more modest objective of the perturbative method. Whether Fraser’s “infinitely renormalized” QFTs make sense as constructive mathematical objects or not, they are not deployed in any way in conventional perturbative calculations.

The apparent inconsistency problem posed by Haag’s theorem also turns out to be less threatening than it initially appeared. The crucial point here is that Haag’s result pertains to the properties of QFT models. Roughly speaking, it tells us that the time evolution of models of the Wightman and Haag-Kastler axioms cannot be carved up in the manner prescribed by the interaction picture. Why doesn’t this undermine perturbative evaluations of scattering cross sections? The short answer is that since the perturbative method is not in the business of providing a structural characterisation of QFT there cannot possibly be a conflict here. Perturbative evaluations of S-matrix elements do not posit the existence of models satisfying the assumptions shown to be inconsistent by Haag’s theorem

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<sup>20</sup>Making sense of the prevalence of unrigorous mathematics in the physical sciences is a philosophical project in its own right, and there may be scope for fruitful engagement with perturbative QFT in this context. My claim here is not that there is nothing more to discuss about the applicability of mathematics here but that one source of worries about more radical lapses of rigour in the perturbative approach is unfounded.

because they do not pick out any physical models at all.

This fits well, I think, with Miller's ([2017]) recent discussion of Haag's theorem, which helps to clarify how perturbative calculations avoid inconsistency. As Miller points out, the methods used to regularize ultraviolet and infrared divergences in the perturbative expansion invariably cut against the assumptions needed to prove Haag's theorem. Consider, in particular, the imposition of high and low momentum cutoffs on the relevant integrals. As I argued above this is not the same thing as constructing a cutoff model, but we now know how to write down such systems. If we put a QFT on a finite volume lattice, for instance, the resulting model is not touched by Haag's theorem because it violates the assumption needed to prove it. In fact, since the number of degrees of freedom is finite, such a system does not admit unitarily inequivalent Hilbert space representations at all. This means that the interaction picture exists and the steps involved with setting up the perturbative expansion of the S-matrix can be concretely implemented. Crucially though, when we remove the cutoffs at the end of the calculation this should be understood as taking a limit of a particular function. Just as taking the radius to infinity of particular quantities defined on a sphere does not commit one to the existence of an infinite sphere, in Norton's example, removing the cutoffs does not rest on the assumption that the interaction picture can be implemented in the continuum and infinite volume limit. The perturbative method simply does not assert the set of claims shown to be inconsistent by Haag's theorem.<sup>21</sup>

## 5 The Real Problem

The rigour and inconsistency problems turn out to be red herrings on the reading of the perturbative approach I have developed then. Worries about the internal coherence of the QFT perturbation theory have, I suggest, largely sprung from a misunderstanding of its aims. Still, there clearly is something puzzling about the perturbative approach, as I have characterised it. It is the justification problem which ends up being the really fundamental conceptual issue with perturbative QFT. The various manipulations described in section 2 may provide a sound method for generating well-defined functions but there remains a mystery about why they provide such good approximations to observables measured in collider experiments. The renormalization procedure, in particular, seems to be flagrantly ad hoc. The process of regularizing integrals, redefining the coupling and removing the regulator in the standard renormalization procedure all seem to proceed in the absence of any physical argument underlying each step.

While my analysis of the perturbative approach does not solve this problem it does help us identify the root cause. The success of the perturbative approach is

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<sup>21</sup>This is not, of course, to say that Haag's theorem has no foundational import at all. In fact, I briefly suggest in the next section that it can be reinterpreted as contributing to the severity of the justification problem for perturbative QFT.

mysterious, I suggest, precisely because it dodges the question of what an interacting QFT really is. Many of the seemingly peculiar features of QFT perturbation series are actually found in many applications of perturbative methods elsewhere in physics. As I mentioned in section 2.2, the process of redefining the expansion parameter in the renormalization procedure is often employed in perturbative calculations in classical and quantum theory even when there is no problem with divergences. Furthermore, divergent perturbative expansions are commonplace in many branches of physics. In most of these cases however, we typically know what the physical systems whose properties we are trying to approximate look like. Although we cannot solve the standard quantum mechanical models of the Helium atom, for instance, we know how to write down its Hamiltonian and Hilbert space. What sets the original perturbative treatment of QED apart from these cases is the absence of any non-perturbative characterisation of the system of interest. While I have argued that this does not render perturbative QFT incoherent it undercuts the possibility of telling a physical story which could explain its success. Haag's theorem can be reinterpreted as contributing to the severity of this situation since it shows that a flat footed attempt to translate perturbative scattering theory into a non-perturbative characterisation of QFT is doomed to failure.<sup>22</sup>

This appears to be bad news for the scientific realist, who wants to say that scientific predictions succeed because they are derived from theories which accurately represent the way the world is. On the other hand, my analysis of the perturbative approach does not fit neatly with the most popular forms of anti-realism. The constructive empiricist states their epistemic commitments with respect to models, taking them to be empirically adequate rather than representationally faithful. If I am right then perturbative QFT cannot be read in these terms either: it does not provide us with physical models at all, empirically adequate or not. Prima facie then, the sort of anti-realism motivated by the discussion so far would have to be a fairly radical form of instrumentalism, which takes the perturbative apparatus to be an algorithm for producing empirically successful predictions. Of course, instrumentalism has well known problems of its own.

Besides these connections with the realism debate, the ad hoc character of perturbative renormalization can also be viewed as a local problem within the QFT programme, and was perceived in this way by physicists in the aftermath of its initial success. The emergence of the axiomatic and effective field theory approaches to QFT in the second half of the 20th century can both be understood as responding to limitations with the original perturbative formalism, the need for an improved physical understanding of the renormalization procedure being an important motivation for these developments. We should not move too quickly to dismiss the perturbative approach to QFT on the basis of the justification problem then. The analysis put forward here suggests that the perturbative formalism lacks the resources to answer this challenge on its own, but we now have non-perturbative approaches to QFT on the table which may be able to fill this

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<sup>22</sup>Miller ([2017], 15) advocates a similar reading of Haag's theorem as pointing to a tension between perturbative and non-perturbative approaches to QFT.

explanatory lacuna.

The claim that the advent of the renormalization group has improved the conceptual standing of renormalization methods, made by some physicists and philosophers (Lepage, 2005; Wallace, 2011) can be understood in these terms. The renormalization group framework arguably provides a non-perturbative justification for many of the steps involved in renormalized perturbation theory. One issue which has been discussed in the recent philosophical literature is how the renormalization group explains the prevalence of renormalizable theories in high energy physics, and thus improves on the traditional view of renormalizability as a mysterious a priori principle (Butterfield and Bouatta, 2015; Hancox-Li, 2015b). But there is also, I think, a more detailed story to tell about how the renormalization group illuminates the perturbative renormalization procedure itself. The imposition of a perturbative cutoff, originally understood as a purely pragmatic step, is re-interpreted in this setting as a matter of throwing away information about unknown high energy physics. The process of redefining the expansion parameter so as to remove divergences in the perturbative coefficients also acquires a physical, and not merely instrumental, reading in the renormalization group context. General results in the renormalization group framework tell us that the S-matrix elements of a QFT model at low energies are very weakly dependent on the value of the ultraviolet cutoff. The hypersensitivity to the cutoff manifest in ultraviolet divergent integrals can thus justifiably be identified as artefacts of the way the perturbative approximation scheme is set up. Removing them can simply be understood as a matter of ensuring that truncations of the series have the scaling properties of the non-perturbative quantities they are supposed to be approximating.<sup>23</sup>

Developing this story in detail is a substantive project in its own right, and there remains a great deal to be said about how the explanatory contribution of the renormalization group should to be understood. Still, these inchoate remarks are enough to show that the justification problem need not be a reason to eschew philosophical engagement with perturbative QFT. Rather, there is much work to be done in examining the extent to which non-perturbative approaches to QFT can account for its success. They also suggest that scientific realism may not be in such bad shape after all.

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<sup>23</sup>Wallace ([2006], [2011]) claims that, in the wake of the success of the renormalization group, we now have a non-perturbative structural characterisation of empirically successful QFTs, namely cutoff formulations of these theories. If this is the case, can we view applications of the perturbative formalism as a calculation tool within this cutoff version of QFT in the modern context, and therefore more analogous to perturbation methods in, say, atomic physics? Perhaps, but developing this view of perturbative QFT requires meta-scientific engagement—it is not simply obvious from looking at contemporary practice in high energy physics. This would be one route to addressing the justification problem.

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