MOVING BEYOND "THEORY T": THE CASE OF QUANTUM FIELD THEORY

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ABSTRACT

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A standard approach towards interpreting physical theories proceeds by first identifying the theory with a set of mathematical objects, where such objects are defined according to mathematicians' standards of rigor. In making this identification, philosophers rule out the relevance of many inferential methods that physicists use, as these often do not meet mathematicians' standards of rigor. Philosophers thus *sanitize* physical theories of all mathematically messy or ambiguous parts before interpreting them.

My dissertation argues against this sanitized approach towards interpreting theories using the example of quantum field theory (QFT). When we look at the details of QFT, we find that the mathematical objects it requires differ according to the specific systems the theory is being applied to in ways that advocates of the sanitized approach do not anticipate. Furthermore, the mathematical objects required for successful application are still being developed in some applicational contexts, so it would be unwise to determine in advance which objects constitute the theory. During this ongoing developmental process, physicists interpret the mathematics using strategies that violate the standards of pure mathematics. In contrast to the sanitized approach, these strategies are more sensitive to the ways in which the mathematics required for the relevant contexts is still under development. I argue that these strategies are not merely instrumental. They suggest alternative approaches to interpretation that philosophers should take into account.

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PREFACE

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1.0 INTRODUCTION

A significant part of philosophy of science is engaged in figuring out what our scientific theories say the world is like. This is the process of interpreting scientific theories. Interpretations of theories are germane to many debates about scientific explanation, metaphysics, and theory acceptance. For theories in physics, the predominant approach towards interpretation proceeds by first identifying the theory with a set of mathematical objects, where such objects are defined according to mathematicians' standards of rigor. I call this the *sanitized* approach towards interpretation, as it ignores the messiness inherent in applying mathematics to the physical world and identifies the theory with a cleaned-up mathematical structure that is not what physicists use in their day-to-day work.

Hans Halvorson has articulated what I call the sanitized approach as follows:

> In philosophy of science in the analytic tradition, studying the foundations of a theory T has been thought to presuppose some minimal level of clarity about the referent of T... There remains an implicit working assumption among many philosophers that studying the foundations of a theory requires that the theory has a mathematical description ... In any case, whether or not having a mathematical description is mandatory, having such a description greatly facilitates our ability to draw inferences securely and efficiently.
> So, philosophers of physics have taken their object of study to be theories, where theories correspond to mathematical objects (Halvorson & Müger, 2006)

As Halvorson suggests, the position he articulates is widespread in "philosophy of science in the analytic tradition". In keeping with Halvorson's language, which is typical of philosophers of physics, Mark Wilson (2008) has labelled this position "Theory T syndrome". Victims of this syndrome may, for example, take "classical mechanics" to refer to some mathematical object, then figure out whether the theory so defined is deterministic (or not), supports some ontology, explains the relevant phenomena, and so on (Earman, 1986; Belot, 1998; Allori, 2013).

Laura Ruetsche (2011) is another philosopher who has articulated what she calls a "standard account" of how to interpret theories that exhibits the symptoms of Theory T syndrome. In the standard account, to interpret a theory, we first assign a mathematical structure to the theory. The physical instantiation of that mathematical structure is an interpretation of the theory. The standard account thus shares with Halvorson's view that one should assign a well-defined mathematical description to a physical theory as a prerequisite of any interpretation.

This dissertation argues for a more unsanitized approach towards interpreting theories, using quantum field theory (QFT) as a case study. I argue that a close look at quantum field theory suggests that we ought not to identify QFT with a set of mathematical objects. The sanitized approach fails to explain certain dynamics of reasoning that physicists have found success with when using mathematics that is still under development. It also tends to turn a blind eye to the mathematics that is used in contexts of application, because such mathematics tends to be too multifarious to be neatly encompassed in a set of mathematical objects. The advantages of my approach, therefore, are that it is able to offer a *rational reconstruction* of physicists' reasoning methods and of the successful application of QFT.

This kind of rational reconstruction, however, differs from that of the logical positivists. The logical positivists took "rational reconstruction" to be the enterprise of putting a physical theory into a logically impeccable framework. Halvorson, as quoted above, is an heir to this tradition. The only modification that he wants is to put a theory into a *mathematically* rather than logically rigorous framework, where the definition of mathematical rigor is defined by the community of professional mathematicians.[1] However, rational reconstruction in Halvorson's sense fails to explain why physicists can successfully reason when the mathematics they use is not rigorous by mathematicians' standards. The contrast between the mathematics used by physicists and the mathematics that many philosophers identify as the referent of the theory is particularly stark in the case of QFT. The disadvantage that Halvorson's form of rational

[1] In the same paper quoted above, Halvorson writes: "In the early twentieth century, it was thought that the referent of [a theory] T must be a set of axioms of some formal, preferably first-order, language. It was quickly realized that not many interesting physical theories can be formalized in this way. But in any case, we are no longer in the grip of axiomania, as Feyerabend called it. So, the standards were loosened somewhat—but only to the extent that the standards were simultaneously loosened within the community of professional mathematicians."

reconstruction has is therefore magnified when it comes to QFT—philosophers' version of QFT is particularly lacking in resources to explain how physicists' practice is rational and successful.

Rational reconstruction in my sense, however, is the interpretation of physicists' practices as strategies for constructing and improving their theories.[2] Rational reconstruction in my sense has the advantage of shedding light on one of the central questions of philosophy of science—the question of why we take the science we have here and now to be a particularly rational way of learning about the world. The basic point that I make in the rest of this dissertation, using both historical and contemporary examples of scientific practice, is that rational reconstruction (in my sense) of scientific practice requires that we adopt the unsanitized approach towards interpreting theories.

Before the philosophical work begins, however, a primer on the theoretical landscape of QFT is necessary. In Chapter 2, I summarize the variety of mathematical frameworks that are used in QFT, highlighting those that are primarily used by physicists and those that are primarily used by mathematicians and philosophers.

The unsanitized approach towards interpretation that I advocate pays closer attention to the mathematics of specific applicational contexts. In many cases this mathematics will still be under development and thus not yet rigorous. Advocates of the sanitized approach have taken this to mean that this part of theoretical practice is merely instrumental and irrelevant to interpretation (Fraser, 2009; Kuhlmann, 2010). In Chapter 3, I use examples from the history of QFT to show that physicists can and did apply alternative strategies to extract information about the world from apparently mathematically senseless manipulations. As they did this, they reworked the mathematical and physical meaning of the original formalism and laid the basis for a new understanding of the theory's content, namely that given by the renormalization group (RG). Thus, their efforts were not merely instrumental and were germane to what the theory says about the world. The success of their strategies suggests that generally speaking, it may be worth paying attention to yet-to-be-rigorized mathematical methods for the purpose of interpretation.

In Chapter 4 I show why the renormalization group should be regarded as providing

[2] I owe the idea of this second sense of "rational reconstruction" to Wimsatt (1976).

interpretively relevant information about the world, even though it has been regarded by many philosophers as merely instrumental and thus not part of a "Theory T" characterization of QFT. One reason is that the renormalization group explains why the formerly unrigorous procedures of subtracting infinities in perturbative renormalization were successful. The RG is not merely instrumental in the way that perturbative renormalization is, because it provides more of a mathematical and physical explanation of the physics that makes perturbative renormalization successful.

In the same chapter, I address readers who are skeptical of the mathematical rigor of the renormalization group. I point out that one can apply a rigorous version of the renormalization group, and that this is crucial to figuring out the microphysics associated with a particular Lagrangian model of QFT. Thus, one cannot object to the significance of the RG for interpretation on the basis of its lack of rigor. Furthermore, the fact that the RG is essential in many cases for microphysical information suggests that such information is not automatically given by the mathematical structures that philosophers often take to constitute the content of QFT. These mathematical structures are typically purged of methods that philosophers regard as merely instrumental, such as the RG.

The upshot of Chapters 3-4 is that we ought not to prematurely dismiss inferential methods that appear to lack mathematical rigor as irrelevant to a theory's content. We see in QFT that these methods later turn out to be of tremendous physical significance and help us to a new understanding of our original mathematical formalism. In Chapter 5 I suggest that this developmental pattern is a common one in mathematics generally. I show how it has recurred in the development of the operational calculus and in the development of the theory of divergent series, and suggest that it fits Mark Wilson's account of how the interaction of syntax and semantics can drive mathematical change (Wilson, 2008, Chapter 8). The recurrence of this pattern is a reason for us to distrust our initial mathematical picture of a physical theory and to pay more attention to "heuristic" factors such as mysteriously successful methods of application. This is contrary to the sanitized approach, in which one starts out with an already rigorous mathematical structure that is the content of the theory.

In Chapter 6, I offer an additional reason for why the sanitized approach may miss out on information that is relevant to interpretation. I argue that the mathematics used in specific applicational contexts is interpretively relevant because it is the solutions we get in these contexts, not just the equations to be solved, that best correspond to what philosophers would call the physical systems compatible with the theory. In both QFT and continuum mechanics, the mathematical nature of these solutions fragments across applicational contexts and, in many cases, is still being determined. That is, the mathematics required to describe physical systems covered by these theories is open-ended and multifarious. Thus, if part of what our theory says the world is like is what systems it says could exist, then it would be unwise to begin answering this question by restricting ourselves to a set of mathematical objects.

2.0 VARIETIES OF QUANTUM FIELD THEORY: A PRIMER

This primer is necessary partly because of the gulf between the methods that physicists take to constitute QFT and the mathematical objects that philosophers of QFT have often taken to constitute QFT. Most of the methods described in a standard QFT textbook for physicists involve calculations in *perturbative* QFT. In perturbative QFT, one takes as a baseline an exactly solvable model of QFT in which there are no interactions. This is represented by a Lagrangian that has no interaction terms. Having no interactions, this model is not of direct physical interest. But to solve models in which there are interactions, one can consider the interactions as small perturbations on the exactly solvable non-interacting model. This allows one to apply the apparatus of perturbation theory to obtain approximate solutions for the interacting model. The problem with all this is that the application of perturbation theory is strictly valid only under certain conditions, and it is hard to verify if these conditions apply in the case of QFT. Furthermore, perturbative QFT uses mathematical tools known as functional integrals. While the exact definition of a functional integral is still in flux, physicists have devised ways to compute these functional integrals without adhering to mathematical standards of rigor.

In truth, the physicists' toolkit includes more than just perturbative QFT, and one can question the rigor of the other parts of their toolkit as well. In addition, there exist programs to rigorously analyse perturbative QFT (Steinmann, 1971), although these form a very small proportion of the work on perturbative QFT. However, because the most common approach among physicists is perturbative QFT, and because it is also the most empirically successful approach, I will use perturbative QFT as the main contrast to rigorous variants of QFT. Thus, when I mention perturbative QFT from hereon, I am referring to the kind used by most physicists and which takes up the bulk of any introductory QFT textbook, not the more rigorous kind that is being developed by a very small number of mathematical physicists.

In their attempts to make the mathematical character of QFT clearer, mathematical physicists developed various more rigorous, non-perturbative formulations of QFT. These typically contain some axiomatic component. The Wightman axioms, Nelson axioms, Haag-Kastler axioms, and Haag-Araki axioms are all examples of axiomatic formulations of QFT. However, these axiomatic formulations lack any specification of the dynamics of specific systems in QFT. As we will see, the task of specifying the dynamics and constructing systems that have those dynamics falls to constructive field theory (CQFT).

Algebraic QFT (AQFT) is the variant of QFT that receives the most attention from philosophers, due to its mathematical rigor. It is based on the Haag-Kastler or Haag-Araki axioms. Nonetheless, AQFT is not the only strain of QFT that philosophers regard as sufficiently rigorous. Constructive QFT[1] is another (Fraser, 2011).

In constructive QFT, one tries to construct interacting models satisfying the Osterwalder-Schrader (OS) axioms, which specify the properties that a theory's Schwinger functions, respectively, must satisfy to define a QFT.[2] Such models, if they exist, automatically satisfy the Wightman axioms, according to the Osterwalder-Schrader reconstruction theorem (Rivasseau, 1991). The Wightman axioms, in turn, are widely accepted as delineating the conditions that all models of QFT must satisfy. CQFT takes its models of interest to be those characterized by Lagrangians that physicists use. One of the aims of CQFT is to find out if these Lagrangians correspond to non-trivial QFTs in the ultraviolet (UV) and infrared (IR) limits.

Unfortunately, neither constructive nor algebraic QFT has so far been able to provide solutions that describe systems with four spacetime dimensions, which are the kind of systems we expect in our world. CQFT contains the best attempts so far to rigorously construct solutions to Lagrangian models, having done this successfully for several systems with dimensions other than four.

The renormalization group (RG) is a collection of methods that investigates problems of scaling in QFT and statistical mechanics. It was first developed in an unrigorous manner

[1]Also known as constructive field theory.

[2]The Schwinger and Wightman functions are important because any observable can be computed from them.

within perturbative QFT. As mentioned earlier, physicists have generally regarded the RG to be foundationally and interpretively significant. Part of the cleavage between philosophers and physicists lies in how the former regard the RG. For example, Doreen Fraser writes that

> RG methods make a significant contribution to the articulation of the empirical content of QFT and to clarifying the nature of the relationship between the empirical and the theoretical content. However, RG methods do not shed light on the theoretical content of QFT. For this reason, appeal to RG methods does not decide the question of which set of theoretical principles are appropriate for QFT... The reason that constructive field theorists are able to exploit RG methods—even though they reject elements of the theoretical content of LQFT—is that RG methods concern the empirical structure of the theory rather than the theoretical content (Fraser, 2011).

In a similar vein, Kuhlmann, Lyre, and Wayne (2002) characterize the RG as providing "a deductive link between fundamental QFT and experimental predictions". This echoes the thought, latent in Fraser's writings, that there is some "fundamental QFT" given prior to using the RG, presumably by some axiomatic form of QFT, and that all the RG does is link this fundamental theory to experimental predictions. This pattern of reasoning is common in the philosophy of physics: for foundational or interpretive purposes, we should focus on only the "fundamental principles" of a theory, given by its axioms, because these constitute the entire theoretical content of the theory. Methods to extract predictions from these principles add no new theoretical content, only pragmatic filigree.

A large part of what I do in this dissertation is to show that what is added by physicists' apparently unrigorous methods is not just pragmatic filigree. The dichotomy that philosophers make between what is rigorous and fundamental and what is unrigorous and instrumental is unhelpful. As we will see in the next chapter, unrigorous reasonings can be non-instrumental and foundationally significant.

3.0 INTERPRETIVE STRATEGIES FOR DEDUCTIVELY INSECURE THEORIES

3.1 INTRODUCTION

The sanitized approach towards interpretation demands that we interpret only theories that are known to be mathematically rigorous. In this chapter I demonstrate the utility of unrigorous theories in foundational inquiries in physics. I focus on the development of quantum electrodynamics (QED) in the 1930s and 1940s. I apply William Wimsatt's account of the use of what he calls "false models" (Wimsatt, 2007) to make sense of how physicists successfully got from the deductively insecure models that were available to them to what we now recognise as modern QED. In particular, the problematic models physicists had available were still useful in helping them to diagnose the physical reasons behind the models' problems. They were also useful in helping them to figure out which particular aspects of the models were responsible for the problems. With these diagnostic strategies, the physicists were then able to extract information about the world that was robust against expected uncertainties in their knowledge.

While renormalization in the history of quantum field theory (QFT) has sometimes been characterized as a process of simply trying to get predictions that agree with experiments (Fraser, 2011), my reconstruction of scientists' reasoning in this period will show that they found their way to the "correct" calculational method not just by having a purely instrumentalist attitude. They interpreted the mathematically suspect theories they had as providing physical information about the world, not just as mere tools for making predictions. The fact that the theories they used were empirically successful does not imply that these theories were *only* "empirical tools" that have no relation to physical reality besides empirical success. Instead, the strategies I describe show that, interpreted appropriately, these theories did provide correct information about the world. This information was important for the development of a better theory.

In short, my argument takes the following form:

- Reasoning in early QED, including the use of renormalization, was not merely instrumental or *ad hoc*. It did not merely aim to extract empirically adequate predictions. Instead, physicists took seriously the idea that the theories provided information about the world beyond mere predictions, and this idea was crucial to the development of a better formulation of QED.
- The strategies used by these physicists suggest alternative ways of deriving information about the world from our theories. These alternative ways go beyond the usual recipe of rigorously deducing the consequences of an axiomatized theory—the so-called "received view" (Cartwright, 1999, p. 179). Furthermore, philosophers such as Wimsatt have argued that these strategies may in some circumstances be more reliable than the approach recommended by the received view.
- Since these strategies exist, can work, and may have some advantages over the usual recipe, it is plausible that they can be successfully applied to contemporary quantum field theories as well.

The plan for this chapter is as follows. In the next section, I explain what I mean by deductively insecure theories or models [1] and outline a few suggestions for how to reason with them. I also explain some advantages these reasoning strategies might have over more familiar deductive reasoning. In Section 3.3, I look at examples of theorising in early QED and argue that they show how applying the strategies described in Section 3.2 to fundamental physics can help us to obtain more than empirical information from false theories. In Section 3.4, I consider the implications of this historical case study for some widely held assumptions about how theories in physics should be interpreted. In Section 3.5, I address some possible objections to my take on the significance of these strategies.

[1]By "models" I merely mean what scientists mean when they use the term "model", not in the technical sense that some philosophers import to the term. For some of my examples, philosophers (or physicists, too) might prefer to use the term "theory" for what I call a "model", or vice versa. This does not matter for my argument since the reasoning strategies I focus on do not depend on these distinctions.

3.2 EXTRACTING PHYSICAL INFORMATION FROM DEDUCTIVELY INSECURE MODELS

Before I discuss the reasoning strategies which I characterize as "deductively insecure", I will clarify what I mean by "rigorous" and "deductively secure", two terms that I will use interchangeably in this chapter.

Although philosophers of physics often use the term "rigorous" as though it has a clear and well-known meaning (Kuhlmann, 2010; Fraser, 2009), it is unclear if there is any consensus either in mathematics or in philosophy of mathematics about whether there exists an absolute standard of mathematical rigor. The debate between (Jaffe & Quinn, 1993) and their respondents (Thurston, 1994; Atiyah et al., 1994) about which proofs in mathematics are rigorous indicates significant disagreement about the definition of rigor in mathematics. In the philosophy of mathematics, (Kitcher, 1981) has considered several candidates for a definition of rigor and opted for a relativized notion of rigor, a conclusion that some historians of mathematics have also come to (Kleiner, 1991).

Despite this lack of consensus in mathematics about the definition of rigor, philosophers of physics tend to use the word "rigor" in what (Kitcher, 1981) calls the "deductivist" sense. The idea is that a rigorous inferential system consists of some axiomatically organized theoretical framework in which the axioms are the "first principles" which we have confidence in, and theorems are deduced from the axioms. This seems to be the notion of rigor that is in operation when axiomatic variants of QFT are viewed as more rigorous than other variants (Halvorson & Müger, 2006; Fraser, 2009). A similar notion of rigor seems to operate in the literature on the foundations of quantum field theory (Dosch & Müller, 2011). Under the deductivist view of rigor, inferences that are deduced from first principles are reliable because we have confidence in the first principles and the theorems we deduce inherit the confidence we have in the first principles.

Another view relating to rigor that seems operative in the QFT literature is the view that perturbative renormalization, the subtraction of infinities in perturbation series or the extraction of information from divergent series, is unrigorous (Iagolnitzer, 1993; Fraser, 2009). Thus, at the very least, a rigorous treatment of QFT must avoid perturbative renormalization. This association of lack of rigor with perturbative renormalization can be attributed to the deductivist view of rigor. The idea is that the inferential move of subtracting infinities is not rigorous because it is not deductively licensed by the axioms of QFT.

For convenience's sake, I will hew to prevailing trends in philosophy of physics by using "rigor" in the deductivist sense of rigor. Thus, when I speak of a theory or an inference as being "rigorous" or "deductively secure", I mean that it is formulated in an axiomatic fashion and that the inferential moves made in the theory are licensed by deductions from the axioms of the theory. Many scientific theories involve deductively *in*secure inferences in their application. These deductively insecure inferences are often made because a straightforward application of the mathematics leads to consequences that have to be dealt with using inferential methods that are not licensed by the original theoretical framework of the fundamental laws or axioms. For example, in early QED, attempts to calculate certain empirical quantities, such as scattering cross-sections, almost immediately led to infinite mathematical quantities. To reason around these infinite quantities, physicists adopted inferential rules that are not countenanced within a strictly deductive framework. There are also examples in fluid dynamics and classical electrodynamics in which the attempt to calculate quantities known to be finite produces infinite results known as divergences (Wilson, 2008; Rohrlich, 2002). In order to dodge these divergences and obtain empirically acceptable finite results, an appeal to inferential techniques beyond what one would consider the "standard axiomatic formulation" of the theory is required. In the case of classical electrodynamics, one may go beyond Maxwell's laws by adding additional assumptions such as an internal force within electrons that holds its charge together (Frisch, 2005, p. 56). In the case of fluid dynamics, the solution to a partial differential equation might include shock waves, the propagation of which often requires inferential resources that are not provided by the original partial differential equation (Wilson, 2008).

In all these cases there is some descriptive gap in the axiomatic formulation of the theory—in QED, we cannot calculate cross-sections in the "usual" way based on the "fundamental equations"; in fluid dynamics, we cannot, in some situations, calculate energy or density in the "usual" way based on the "fundamental equations". Thus, all these models or theories have descriptive gaps in what would traditionally be considered their "deductive" formulation. As an abbreviation, I will describe theories like these as "deductively insecure".

3.2.1 Strategies

I take what I have above described as deductively insecure theories to fall under Wimsatt's possibly broader category of "false models" (Wimsatt, 2007). In doing so, I don't mean to claim that what I call deductively insecure theories are *false*. None of my arguments depend on the truth values of these theories. I merely intend to use the *methodological* lessons Wimsatt draws from his analysis of "false models". Therefore, for the purposes of this subsection, I will use Wimsatt's language of "false models" without committing myself to the view that the models of early QED were indeed false.

Wimsatt describes several ways in which scientists may use false models to obtain information about the world. He argues that it is not just the similarities models have with the world that make models useful. We can, in addition, learn about the world by studying the *patterns* in which false models *fail* to accurately describe the world. The descriptive failures of false models are particularly useful in helping us to localize the "parts, aspects, assumptions, or subcomponents" of the model that are responsible for its failures (Wimsatt, 2007, p. 103).

Wimsatt offers the following positive strategies for localizing the errors and successes of models to parts of the models:

1. A comparison of multiple false models can determine which features of the various models are particularly relevant, and which are irrelevant, to the phenomenon of interest. For example, if multiple models share an assumption but differ in their other assumptions, we can then take that common assumption to be a reason behind the commonalities shared by the results of the multiple models. In Section 3.3, we will see that the agreement of multiple methods of ignoring high-frequency photons in QED led to a diagnosis that the failures of QED were due to a breakdown of the theory in the high-frequency domain.
2. Since all our theories begin with some unreliable assumptions, when we use them to articulate our physical knowledge, we ought to look for information we can get from these theories that is relatively insensitive to the reliability of some of our assumptions.

One way of doing so is to look for a result that is robust across multiple false models. One would then have reason to think that this result is independent of the differences between the assumptions made in the various models. Wimsatt argues that these are the aspects of our false models that we have more reason to accept as true (Wimsatt, 2007, p. 105). If the robustness holds across variations in modelling assumptions that one might reasonably expect to occur in future theory change, then this form of robustness can be thought to also be a kind of robustness over time. Sections 3.3.3 and 3.3.5 offer examples in early QED of finding robustness through agreement across multiple models.

3.2.2 Not Instrumentalism

While there undoubtedly are false models that have only instrumental value, this need not be the case for *all* false models. Wimsatt argues that insofar as the strategies described above for using false models are effective, it is *because* the false models involved partially capture some aspect of reality while, at the same time, deviating from it in certain systematic ways. For example, in early QED, it was widely expected that the available models would encounter descriptive failures whenever phenomena occuring on sufficiently small length scales were involved. These failures, however, were not taken to be merely failures of prediction. The manner in which they failed and the specific phenomena for which they failed were interpreted as telling us something about what the world is like. It is by adopting an attitude of what Wimsatt calls "local realism" towards some false models that one can exploit them using the strategies he describes (Wimsatt, 2007, p. 95). A pure instrumentalist would have no reason to pursue these strategies, because these strategies extract information by exploiting the *reasons* for why the models reflect or deviate from reality (Wimsatt, 2007, p. 101, 392). False models can be more than mere instruments for prediction and explanation. They can also be sources of physical knowledge, provided that we apply the appropriate inferential strategies to them. In the next section, we will see how these strategies succeeded in early QED.

3.3 REASONING WITH FALSE THEORIES IN EARLY QUANTUM ELECTRODYNAMICS

3.3.1 Background

Quantum Electrodynamics (QED) is the attempt to give a quantum mechanical description of electrodynamics. While classical electrodynamics obeys the principles of special relativity, quantum mechanics before QED did not incorporate these principles. Furthermore, quantum mechanics had yet to be formulated in a completely field theoretic framework. Traditionally, non-relativistic quantum mechanics modelled electrons and other constituents of matter as particles and not as fields. In trying to combine the principles of quantum mechanics with field theory, theorists in QED quickly ran into problems, largely surrounding the appearance of divergences in many key quantities.

The divergences appeared in calculations of the following quantities: the interaction energy of the electron with its own field, otherwise known as "self-energy", and the polarizability of the vacuum. The latter arises when an external field is applied to any system. In QED, the vacuum is not strictly empty but can contain, depending on one's model, either "virtual charges" or an infinite number of electrons. The application of an external field changes the distribution of those charges and gives the vacuum a polarizability that has to be taken into account when modelling systems in QED. Similarly, the self-energy of the electron appears in all calculations involving electrons. The divergences were thus a serious impediment to making calculations about empirically observable phenomena.

In time, physicists arrived at procedures by which divergences essentially could be subtracted from both sides of an equation to yield finite observable results. This was known as renormalization. It was applied to various quantities, including the charge and mass of the electron. In the 1930s and 1940s, the period currently under discussion, the mathematical justification of renormalization was unknown.[2] For this reason, renormalization has often

[2] Among practising physicists, it is widely believed that the renormalization group and effective field theory today provide a mathematical and physical justification for perturbative renormalization. See for example Chapter 16 of (Duncan, 2012). Although philosophers like (Fraser, 2011) have claimed that the renormalization group is merely instrumental, in my view they have conflated *perturbative* renormalization with the renormalization group, which can be given a *non-perturbative* formulation. Their claims of lack of rigor may apply to perturbative renormalization, but there exist rigorous non-perturbative methods.

been characterized as a physically and mathematically unmotivated move, or as one that was made solely for the sake of producing predictions (Fraser, 2009).

Against this instrumentalist view of how the divergences were overcome, I will show that physicists made crucial use of information about the world—information going beyond mere empirical predictions—that they extracted from their deductively suspect inferential frameworks. In doing so, they used the strategies described above: identifying robust aspects of their theories and diagnosing the factors relevant to their difficulties by comparing different models in QED.

3.3.2 Background on the Available Models

Before I begin describing how multiple deductively insecure models were used to obtain theoretical information, I will give a brief outline of some of the models that were used. All of these models were acknowledged to be problematic in some way. In particular, they all encountered problems with divergences in key physical quantities.

3.3.2.1 Quantum Theory of Wave Fields One of the earliest attempts to render quantum mechanics compatible with special relativity was Heisenberg and Pauli's quantum theory of wave fields (Heisenberg & Pauli, 1929). Heisenberg and Pauli begin by considering classical field systems in the Lagrangian formulation. For a field ψ, one writes down the Lagrangian density L of the system: $\mathsf{L} = \mathsf{L}(\psi_\alpha, \nabla\psi_\alpha, \dot{\psi}_\alpha)$, where ψ_α are the components of the field. One next defines the canonical momentum π_α to each ψ_α by $\pi_\alpha = \frac{\partial \mathsf{L}}{\partial \dot{\psi}_\alpha}$. The Hamiltonian density is defined by $\mathsf{H}(\pi_\alpha, \psi_\alpha) = \sum_\alpha \pi_\alpha \dot{\psi}_\alpha - \mathsf{L}$. Finally, this system of classical field equations is quantized by requiring that $[\pi_\alpha, \psi_\alpha] = \hbar/i$. This is the field analogue of the canonical commutation relations in non-relativistic quantum mechanics.

In pursuing this approach towards QED, Heisenberg and Pauli ran into problems involving divergences in the self-energy of charged particles.

3.3.2.2 Positron Theory While the quantum theory of wave fields started with classical field theory and then proceeded to quantize the classical fields, Dirac's positron theory started from a particle-based point of view. Before he formulated the positron theory, Dirac had found the Dirac equation, which was meant to describe the free motion of a relativistic electron. However, the Dirac equation implied the existence of negative energy states for the electron. The positron theory was an attempt to give those states a physical interpretation. According to the positron theory, even in a vacuum, there are an infinite number of electrons in negative energy states. An unoccupied negative energy state is manifested as a positron, a particle of the same mass as the electron, but with an opposite charge. Pair creation, which is when a photon turns into an electron and a positron, corresponds in this theory to the movement of an electron from one of the negative energy states to a higher, positive energy state. The "hole" it leaves behind in the negative energy state from which it moves corresponds to the positron. The positive energy state it moves to corresponds to the electron that is created together with the positron. The external electromagnetic field is treated as a field while the infinite sea of electrons is treated like a collection of particles. In this manner, positron theory models the interaction of light quanta with the vacuum. As with the quantum theory of wave fields, the positron theory encounters problems with infinities. The infinite number of electrons in the vacuum with negative energy leads to various divergences when one tries to calculate the effects of these vacuum electrons.

3.3.2.3 Pauli-Weisskopf Theory Pauli and Weisskopf (1934) formulated a model of quantum electrodynamical phenomena starting with the dynamics of a free spinless particle described by the Klein-Gordon equation:

$$\left(\Box - \frac{m^2c^2}{\hbar^2}\right)\psi = 0$$

Following Heisenberg and Pauli's method of quantizing in the quantum theory of wave fields, Pauli and Weisskopf quantized the Klein-Gordon equation. Unlike the Dirac equation, the Pauli-Weisskopf theory implied no negative energy states. From it, one could infer the existence of positrons and of pair production processes. However, unlike the positron theory, it could not incorporate spin in a relativistically invariant manner.

Like the positron theory, the Pauli-Weisskopf theory encountered divergences when incorporating the effects of the vacuum, this time because of the infinite field-dependent polarizability of the vacuum.

3.3.3 Robustness Across Different Models

Having described some of the deductively insecure models on offer in early QED, I will now go on to describe some interpretive strategies that were applied to them. One such strategy was to find features that are robust across multiple models, and then infer something about the world from such features.

While none of the models described above served as a satisfactory theory of QED, all of them were nonetheless fruitfully used to confirm some of the physical hypotheses upon which they were based. This was done partly by cross-checking the models against one another to ensure that they agreed on quantities involving the physical processes that were common to them. Such agreement was taken to be a positive sign even if the models being compared contained divergences and thus produced apparently physically meaningless predictions. Indeed, as we shall see, at times even the divergent terms in different models were cross-checked against one another to look for agreements that would confirm certain physical hypotheses the modellers were considering. Such cross-checking is an example of the strategy outlined in Section 3.2 of finding results that are robust across multiple models even if each of the models is acknowledged to be inadequate on its own.

One example of comparing models that contain divergences occurs in Heisenberg and Euler (1936). They use Dirac's positron theory to calculate quantities associated with light-light scattering. They then compare their results with those obtained from the quantum theory of wave fields:

> Regardless of the question of whether it is physically acceptable to neglect higher order terms, each expansion term in the result of the last section agrees with a direct calculation of the corresponding scattering process in the quantum theory of wave fields if the perturbation calculation is only performed to the lowest order that yields a contribution to the corresponding process. In both calculations, the contributions of the terms which correspond to the formation and disappearance of the light quantum and a pair are neglected. [The agreement of the terms of fourth order with the terms obtained by the direct calculation of light-light scattering is therefore a test of the correctness of the calculation.]

(Heisenberg & Euler, 1936, p. 731, my translation. The brackets appear in the original text.)

Note that this exercise of comparing two models is not simply one of confirming their instrumental usefulness for making empirical predictions. If that were so, it would be pointless to ensure that the two models being compared both contain terms that correspond to the same physical processes, and similarly that they both leave out terms that correspond to the same physical processes. Furthermore, since Heisenberg and Euler (1936) do not actually compare the quantities they calculate with empirical results, they are clearly not interested solely in the usefulness of their model as a predictive instrument. Rather, they are interested in showing how those two models could provide non-predictive information about the same physical process.

Another example of the use of multiple deductively insecure models to confirm a hypothesis about what they tell us about the world occurs in Weisskopf (1936). Before Weisskopf, Dirac (1934) had used his positron theory to formulate a method to subtract divergences in energy and charge- and current-densities that were due to the effects of the infinite number of vacuum electrons in his theory. This method was not completely *ad hoc*, being partly justified by the following facts:

1. The substracted portion of the calculation, which also happened to contain all the divergences, is fixed for any choice of external field. Thus, it would not be measurable by experiments.
2. The portion that is *not* subtracted is finite, relativistically invariant, gauge invariant, and Hermitian—all properties that one would expect of a "physically real" entity.
3. The electric and current densities corresponding to the portion that is not subtracted satisfy the appropriate charge-current conservation law.

Weisskopf set out to provide further justification of Dirac's subtraction method. In particular, he suggested taking the following properties of the vacuum electrons to be physically meaningless (Weisskopf, 1936, p. 6, my translation):

1. the energy of the vacuum electrons in a space without external fields;

2. the charge- and current-densities of the vacuum electrons in a space without external fields;
3. any component of the electric and magnetic polarizability of the vacuum which is field-independent and constant in space and time.

Weisskopf goes on to show that the assumption that these properties are physically meaningless in the Dirac positron theory allows us to eliminate exactly those divergent terms that Dirac had subtracted in *his* method. Weisskopf found it significant that this assumption worked not just for Dirac's subtraction method. If one applied the same assumption about which quantities were physically meaningless to the Pauli-Weisskopf theory, then one obtained results that were similar to those in Dirac's positron theory (Weisskopf, 1936, p. 8). In effect, he cross-checked the effectiveness of the assumption in the two models.

Like Heisenberg and Euler (1936), Weisskopf also checked terms in one model against those in another. The divergences associated with the vacuum in the Pauli-Weisskopf theory are not due to the infinite number of negative energy electrons, which do not exist in the Pauli-Weisskopf theory. Instead, they are due to an infinite field-dependent polarizability of the vacuum. However, they are similar to those in Dirac's positron theory in the sense that they also exist even in a vacuum, and they are composed of terms with mathematically similar forms. Specifically, Weisskopf points out that in *both* the Dirac positron theory and the Pauli-Weisskopf theory, a calculation of the energy density of an electromagnetic field produces the following divergent terms: one that is independent of the field strength and thus represents the energy density of the field-free vacuum, and another that is a quadratic function of the field strength (Weisskopf, 1936, pp. 25-26). The implication of this partial "agreement" is that both these theories are reflecting similar physical situations. In addition, the hypothesis Weisskopf makes above about which quantities are physically meaningless serves to remove divergences of these types in both the Pauli-Weisskopf theory and the Dirac positron theory. By checking that the two false theories produce similar results and that a key hypothesis leads to similar consequences in both of them, Weisskopf ensures that the fruitfulness of the hypothesis he is proposing is not simply due to the quirks of one particular deductively insecure model. Success across a range of different models suggests that his hypothesis is getting at something more general about QED.

Heisenberg, Euler and Weisskopf did not conclude from the divergences in their respective calculations *merely* that the theories they were using were inadequate and to be discarded. They drew richer inferences than that. They used the patterns in which the divergences occurred as diagnostic tools. These patterns were used to isolate particular aspects of the various theories that they regarded as physically significant or insignificant. Instead of rejecting one theory or another wholesale, they compared theories in order to determine which *parts* of the theories are to be taken seriously as contributing towards our physical knowledge.

3.3.4 Comparing Models to Find Reasons for Divergences

Another way in which the comparison of multiple problematic models can provide physical knowledge is in diagnosing the physical reasons for why the models fail in the ways they do. Although physicists in the 1930s and 1940s were unaware of any mathematically rigorous justification of renormalization, they justified it by noticing that in different situations and different models, the divergent terms were associated with the electromagnetic mass, and that the same divergent terms appeared in the calculations for different physical systems, thus suggesting a common reason for the divergences.

One example of this strategy of comparing models to determine the physical significance of the divergences occurs in the work of H. W. Lewis (1948). One of the earliest quantities that QED was used to calculate was the Lamb shift—the small difference in the $^2S_{1/2}$ and $^2P_{1/2}$ energy levels of the hydrogen atom that is due to the interaction between the electron and the vacuum. Because this interaction is predicted by QED but not in non-relativistic quantum mechanics, the calculation of the Lamb shift was a chance to verify QED. Hans (Bethe, 1947) gave a calculation of the Lamb shift that, so long as one ignored certain divergent terms, matched experimental measurements.

Noticing that all the divergent effects in Bethe's calculation were due to the electromagnetic mass, Lewis studied radiative effects for electron scattering to see if divergences in that calculation would also be due to the electromagnetic mass. Like many other physicists working in QED at that time, Lewis was circumspect about the generality of QED, so he operated on the assumption that "the electromagnetic mass of the electron is a small effect and . . . its apparent divergence arises from a failure of present day quantum electrodynamics above certain frequencies" (Lewis, 1948, p. 173). Lewis' motivation was to "re-examine *some other areas* in which the electrodynamics has failed, to see whether these considerations [about the electromagnetic mass] affect the conclusions that have been drawn" (Lewis, 1948, p. 173, emphasis mine). Lewis' phrase "other areas" suggests that he considered both the Lamb shift and the radiative effects of electron scattering to be areas in which quantum electrodynamics had *failed*, presumably because of the need in both cases to renormalize the mass. Thus we can see that here Lewis is explicitly employing a strategy of investigating the failures of QED in order to confirm a hypothesis about which physical factors are relevant to the phenomena he is trying to model. By comparing the theories that exhibited these divergences, he also found support for a hypothesis about whether QED applies to high-frequency phenomena.

In addition, Lewis found that in his non-relativistic calculation of radiative corrections to electron scattering, the only divergent terms in the results were also due to the electromagnetic mass—just like in the calculation of the Lamb shift. This finding that the electromagnetic mass was responsible for divergences in two distinct physical situations supported the idea that the failures of QED were indeed due to its lack of validity at high frequencies. The later discovery that the discrepancy in the hyperfine structure of hydrogen could also be explained by the same renormalization procedures further supported this idea (Schweber, 1994, p. 317).

While it is undoubtedly true that obtaining empirical predictions for phenomena like the Lamb shift was a key motivation for renormalization, the reasoning in Lewis' paper shows that considerations besides mere instrumental value were also at work. Lewis was concerned not just about making predictions, but about determining which physical factors out there in the world were relevant to the phenomena that QED is able to account for.

Lewis' conclusion that failure in the high energy regime accounted for the divergences was echoed by Richard Feynman, who sought to support Julian Schwinger's diagnosis of "which terms are to be identified in a future correct theory with rest mass, and hence should be omitted from a calculation which does not renormalize the mass" (Feynman, 1948, p. 1430). The way Feynman did this was to work with a model in which there was a "cutoff" at a high frequency which allowed all terms in the model to converge without a need for renormalization. The cutoff essentially eliminates from one's calculations all the terms corresponding to interactions involving high-frequency photons. Feynman found that the cutoff did in fact lead to the omission of exactly those terms that Schwinger's mass renormalization procedure eliminated. While Feynman admitted that the cutoff was an "arbitrary rule", he nonetheless maintained that his model "confirmed" Schwinger's ideas (Feynman, 1948, p. 1430).

Here, Feynman is using a model he explicitly admits to be false to "confirm" an inferential method—Schwinger's technique of renormalization—for which, at that time, there was no known rigorous mathematical justification. Schwinger (1948) had justified renormalization by arguing that QED fails for high frequency phenomena, and that renormalization was a way to get results that were independent of those phenomena, but neither of these arguments were based on rigorous mathematical proof. Feynman's approach fits my description in Section 3.2 of how scientists use multiple deductively insecure models to discern which features of the models are relevant to the phenomena of interest. Feynman created a model that explicitly ignored the influence of high frequency phenomena but accurately accounted for lower-frequency phenomena. He then used it to confirm Schwinger's approach, the latter being partly justified by its own failure at high frequencies. Seen as a comparison of models within a limited domain in which both claimed to valid, Feynman's reasoning appears less arbitrary.

Furthermore, Feynman's motivation here is clearly not just instrumental. From an instrumentalist point of view, there would have been little need to reproduce Schwinger's results using another approach. Rather, the main import of Feynman's paper was to confirm hypotheses such as which quantities corresponded to rest mass and the relevance of high frequency phenomena to QED.

3.3.5 Independence from Assumptions About Epistemically Inaccessible Regions

One common research strategy for physicists working in early QED was to look for results and components of theories that were independent of what the world would be like at arbitrarily small length scales. By finding those aspects of our theories that are independent of what the world is like in currently epistemically inaccessible realms, we can take into account the imperfections and incompleteness of our theories in figuring out what they tell us about the world. Those aspects of our theories that are robust across variations in assumptions about what happens in epistemically inaccessible realms of the world are less likely to be a consequence of mistaken assumptions about what the world is like in those realms. Therefore, it is better to include as part of the content of our physical knowledge only those aspects of current theories that are independent of what goes on in those realms.

As explained in Section 3.2, one way of reducing the dependency of one's conclusions on possibly false assumptions is to look for conclusions that are supported by a variety of models that differ in their assumptions. In particular, if variations in a particular assumption are shown to be irrelevant to the model's conclusions, then one can conclude that the incorrectness of that assumption is irrelevant to the reliability of the model's conclusions. This strategy was used in early QED, when the dependence of results from models of QED with high-frequency cutoffs on the choice of cutoff frequency was considered. It turns out that if one considers a range of models in which a cutoff frequency can be varied, then the physically significant quantities of the models are "nearly independent" of one's choice of cutoff frequency if the cutoff frequency is of order $137mc^2/\hbar$ or higher (Feynman, 1948, p. 1431).[3] In other words, the multiple possible cutoff models, taken together, tell us that the dominant dynamics of QED is largely unaffected by electromagnetic interactions involving high frequency photons.

In summary, these examples of theorising from early QED show how physicists used the strategies described in Section 3.2 to extract physical information from their existing,

[3]The significance of $137mc^2/\hbar$ is that it is the frequency that corresponds to the length scale at which nuclear forces become important. Since QED does not account for nuclear forces, it is expected to be invalid as one moves to length scales smaller than $137mc^2/\hbar$.

deductively gappy inferential frameworks in order to guide themselves to better frameworks.

The case of early QED also demonstrates how robustness against unreliable assumptions was an important desideratum of information to be extracted from theories. This robustness was obtained by formulating inferential recipes that were independent of the details of what the universe is like at very small length scales.

3.4 IMPLICATIONS FOR CONTEMPORARY PRACTICES OF INTERPRETING THEORIES

Some philosophers engaged in interpreting contemporary quantum field theories have argued that contemporary modelling practices of physicists, using what these philosophers call "heuristic" quantum field theory, are irrelevant to interpretation. Their argument hinges on their belief that firstly, "heuristic" quantum field theory is not mathematically rigorous, and secondly, that only mathematically rigorous theories should be interpreted (Fraser, 2009; Halvorson & Müger, 2006). While I suspect that the first part of their belief is incorrect for at least some quantum field theories, that is an argument for another paper.[4] The second part of their belief stems from the abovementioned "received view" of theories. Under this view, non-rigorous theories cannot really tell us about the world—all they do is serve as instruments for prediction.

As we saw above, the case of early QED is an example of how deductively insecure models or theories were used not *merely* as instruments of predictions, but also as ways of figuring out what the world was like beyond mere experimental results. The various strategies of doing this with deductively insecure models were also described above. These strategies do not merely involve looking at the deductive consequences or models of a theory. Instead, they go beyond the received view in the following ways:

1. They provide a way for us to discriminate between the parts of the theory that are more reliable and the parts that are less reliable. In this way, we can accept "part" of what a

[4] See footnote 2.

theory says the world is like, without accepting the whole theory, even if in some sense the whole theoretical apparatus is required for practical goals like prediction.

2. They allow us to delineate the domains in which the theory is expected to fail. Since most of our theories fail to adequately describe some part of the world, we should look for interpretive strategies that tell us what a theory says about restricted domains of the world without also having to swallow the theory's *global* account of what there is in the world.

3. The received view does not accommodate information we derive from studying the patterns in which descriptive gaps occur in our theories. In contrast, in the strategies described above, an erroneous prediction is not *merely* a descriptive gap in a theory, but can provide physical information about the world. This is important given that even among our contemporary theories, most encounter descriptive gaps in some domain. For example, Wilson has argued that even classical mechanics contains descriptive gaps within what we would normally consider as the "possible worlds" of classical mechanics (Wilson, 2013). (Rohrlich, 2002) has similarly argued that classical electrodynamics encounters mathematical blowups unless we restrict its applicability to a certain range of relative length and time scales. In quantum field theory, proponents of the effective field theory point of view accept that all quantum field theories fail at a small enough length scale (Lepage, 2005; Zee, 2010; Duncan, 2012).

4. The above strategies offer ways in which we can distinguish the aspects of our models that are relevant to the phenomena of interest from those that are not. By comparing multiple models rather than looking at the deductive consequences of a single set of axioms, we may be able to better figure out which aspects of the our models are more important for the phenomena of interest.

5. Comparing the results of multiple deductively insecure models can be a way to derive information that is more insensitive to the details about what the world is like in those realms of nature that are epistemically inaccessible to humans. In this way we can also derive information that is less dependent on the truth of some less reliable theoretical assumptions. This can be done by looking for patterns that are robust across models that differ on their "less reliable" components. For example, we saw in Section 3.3.5

that using the above strategies, physicists derived information about QED that did not depend on assumptions about what goes on at arbitrarily small length scales.

6. By understanding how we can derive information about the world from deductively insecure models, we can better explicate the success of some reasoning strategies that are commonly used by scientists. These strategies would otherwise be dismissed as *ad hoc* moves without physical or mathematical motivation. This is especially the case for reasoning strategies used by physicists who have yet to make the inferential framework they are working with "rigorous" by the standards of their time, but who are nonetheless able to use that framework not just for predictions, but also to learn something about what the world is like.

While in this chapter I have shown how these strategies were applied in early QED, they may also be used to interpret contemporary theories in physics. In quantum field theory (QFT), Stephan Hartmann has suggested that phenomenological models are sometimes used to determine which aspects of a model were relevant for a particular phenomenon (Hartmann, 1999). Similarly, I have suggested here that physicists working with deductively insecure models in early QED were able to isolate the features of their models that were responsible for the limited empirical successes they had. These features then could be taken to provide genuine physical information about the world—the lesson taken from the deductively insecure models was more than just their successful empirical predictions. The interpretation of physical theories is often taken to be the figuring out of what our theories tell us the world is like (van Fraassen, 1991; Rickles, 2008). Since I have argued that the deductively insecure models of early QED do tell us what the world is like, just not necessarily in the usual way as described by the received view, it is plausible that the reasoning strategies I have described are relevant to the interpretation of contemporary theories. This is made more plausible by the fact that contemporary theories are often more deductively insecure than is popularly assumed.

Another reason to expect these strategies to be of general applicability even in contemporary QFT is that the basic philosophy behind renormalization that already existed in the 1930s and 1940s has not changed as much as one might expect. We saw above that as early as the 1930s, the manipulation of deductively insecure models had led physicists to suspect that the divergences of QFT arise due to extensions of QFT into arbitrarily small length scales, and that these divergences could be avoided if we understood the theory to fail for small enough length scales. This "effective field theory" philosophy is still a prevalent view among contemporary quantum field theorists (Zee, 2010; Duncan, 2012).

If we take this view seriously, then we should also consider applying the interpretive strategies described above to contemporary QFT. As we saw, the strategies physicists used to interpret early QED were partly based on the assumption that the theory didn't necessarily apply in all domains. They succeeded because they used the theory's *descriptive gaps*, such as the occurrence of divergences, to inform their interpretations. We can do the same with contemporary QFTs, and indeed, physicists already do this. For example, they use perturbative expansions to determine where a given QFT fails to be applicable. A given point of failure may then be interpreted as the length scale at which the dynamics of the systems of interest shift from one QFT to another. Arguably, this change in dynamics is a real feature of the world and not just "instrumental information".

In contrast to the QFTs widely used by physicists, the axiomatic frameworks that many philosophers of QFT have been working with make assumptions about what goes on at arbitrarily small length scales.[5] Given our inability to experimentally probe phenomena at arbitrarily small length scales, relying on these assumptions, even within a mathematically rigorous framework, is a risky endeavour. In such circumstances, applying the above strategies to the physicists' "heuristic QFT" may not be any *less* reliable an approach than interpreting axiomatic QFT via the received view. This is one way in which the strategies I have described may be *secure* in some sense without being *deductively* secure in the sense defined above. Another way is through Wimsatt's concept of robustness, described above in Section 3.2: if multiple less than fully secure inferential methods lead to the same conclusion, then the reliability of that conclusion may be greater than the reliability of any single one of those inferential methods. Thus, a conclusion may still be reasonably reliable even if it is endorsed by multiple *deductively* insecure inferential methods.

[5]For example, in the Wightman axioms, Poincare covariance and microcausality are assumed to hold at all length scales (Streater & Wightman, 1964).

3.5 OBJECTIONS

3.5.1 Philosophers Ought to Interpret Only Mathematically Rigorous Theories

It may be objected that although I have described successful reasoning strategies that physicists use, I have not shown that it was *good* for them to use those strategies. Perhaps it would have been even better if they had used only mathematically rigorous reasoning. One reason for such an objection may be that the deductions one makes starting from rigorous theories are more *reliable* than the knowledge that one may get through the inferential strategies outlined above. Deductions made in a mathematically rigorous inferential framework are truth-preserving. If the assumptions one starts with are true, then one can count on the conclusions being true.

One problem with this reason has been given by (Kitcher, 1981), who points out that it is often not the case that the statements we end up accepting as axioms have the epistemological certainty attributed to first principles—our knowledge of the axioms is often less certain than our knowledge of some of their deductive consequences.

Another problem with the reason proferred above is that it is not clear if determining content only from theories that have been cast in a mathematically rigorous form is a more reliable practice in real-world conditions, where *most* and perhaps all theories are deductively insecure in the sense defined above. We do not at present have any theory that perfectly describes the world. As mentioned before, many contemporary physicists believe that even quantum field theories, supposedly the "most fundamental" theories, fail to describe the world accurately at some small enough length scale. If, as physicists like (Duncan, 2012) argue, we *know* that some of the assumptions of quantum field theory must be erroneous because they do not take into account gravitational effects, then the disadvantage the received view has of being more sensitive to false assumptions is non-trivial. In such conditions, it is unclear if extracting deductions from the mathematically rigorous system is more reliable than an alternative method in which one uses multiple less-than-rigorous inferential frameworks, each starting from different assumptions, and accepts the conclusions which they all agree on. As Wimsatt (2007, p. 49) has argued, inferences made from a single rigorous inferential system may be too sensitively dependent on errors in the assumptions one begins with.

In contrast, if multiple less-than-rigorous inferential systems, each starting from different assumptions, agree on some result, that result may be more reliable than those given by single rigorous system that starts with some false assumptions (Wimsatt, 2007, p. 49). As we saw in some of the QED examples, the divergences that appeared in physicists' calculations stemmed from assumptions about what the world is like on arbitrarily small length scales. Since we have no experimental access to very small length scales, these assumptions are quite possibly false. Yet, axiomatic quantum field theory includes such assumptions in its axioms. In contrast, the strategies I delineated in Section 3.2 do not require us to either have epistemic access to all length scales in the universe or to make unwarranted assumptions about what epistemically inaccessible realms of the universe are like.

3.5.2 "Heuristics" Are Acceptable Only for Scientific Discovery

Another possible objection is that however much the strategies described in this chapter may be useful to scientists, philosophers should adopt the received view instead. The thought behind this may be that scientists have different goals than philosophers. While scientists aim to discover theories, philosophers aim to clarify or justify theories. The strategies suggested in this chapter may work well for the purpose of scientific discovery, but not for the purposes of philosophers.

I have a few responses to this objection. Firstly, part of the point of the historical example was to show that sometimes figuring out what *contemporary*[6] theories say the world is like, even if these theories are deductively insecure, is an important part of discovering a better theory. The process of discovery is not just one of *ad hoc* curve-fitting or serendipity, but also involves using pre-existing theoretical scaffolding. In the same way, one might think that the discovery of quantum gravity will involve figuring out what contemporary QFTs tell us about the world. And since one can construe contemporary QFTs as consisting in part of deductively insecure models, that means that the current task of figuring out what

[6]That is, contemporary in the context of physicists working in the 1930s and 40s.

QFT tells us about the world can include such strategies as I have described.

Secondly, the objection ignores some key aims of philosophy of science. Among other things, one of the purposes of philosophy of science as a normative discipline is to provide norms for how science should be practised. If, as I have argued, the reasoning strategies described above are helpful norms for scientists to follow when trying to figure out what their theories say the world is like, then it is legitimate for philosophy of science to articulate these norms.

Furthermore, many justificatory questions in philosophy of science depend on methodological issues. If we adopt an approach towards philosophy of science that ignores the methodology of science, then we are ignoring a major part of what justifies science as a way of obtaining knowledge. If we have an account of theory interpretation that explains how the methodology of practising scientists is an effective way of getting at what the world is like, that does in fact shed light on the justifactory issue of why science is a reliable route to knowledge.

As for the claim that philosophers aim to clarify theories and the fuzzier notion of what our theories tell us about the world outlined in this chapter undermines that aim, I can only respond that insofar as clarificatory projects in philosophy of physics proceed by leaving out theoretically important but deductively murky aspects of physical theories and clarifying only the bits left over that are amenable to clarification, then such projects ought to be viewed as illuminating a part of our theories, not as characterising theories in general. Much of this chapter has been devoted to showing that certain methods of inference dismissed by philosophers of physics as purely instrumental or *ad hoc* do in fact contribute to our physical knowledge.

3.6 CONCLUSION

I have argued in this chapter in favour of the importance, even in foundational contexts, of using certain reasoning strategies to figure out what deductively insecure models tell us about the world. From these models, physicists can extract information about the world that goes beyond mere empirical predictions. I have described how they did this in the case of early QED. They confirmed hypotheses about which parts of their theories were somewhat representative of physical processes by looking for common results across multiple models. They were also circumspect about the range of applicability of contemporary theories, taking care that these theories did not rely on assumptions about phenomena at arbitrarily small length scales or arbitrarily high energy scales. Finally, they exploited the descriptive gaps of their models to figure out what kinds of physical events were relevant to the phenomena they were trying to model.

I have argued that these strategies are not necessarily any less reliable than the axiom-based received view of how to figure out what theories tell us about the world. In a certain sense, these strategies are reliable and therefore justified ways of interpreting deductively insecure theories. Furthermore, accepting that these theories give us genuine physical information allows us to make rational sense of physicists' reasoning in early QED, thus serving one of the purposes of philosophy of science. Extrapolating the lessons learned from this historical case to modern times, it is plausible that paying attention to physicists' "heuristic reasoning" with models in contemporary QFT can similarly illuminate foundational issues in QFT. After all, many models in contemporary QFT are descriptively adequate only up to some very small but non-zero length scale. Similarly to what happened in early QED, one can nevertheless extract some information about the world from the points of descriptive failure of these theories. Thus, contrary to what some philosophers have recently argued, contemporary Lagrangian-based QFTs may be worthy of interpretive attention—but perhaps not via interpretive strategies of the kind assumed by the received view.

In the following chapters, I offer reasons drawing more directly on the specifics of contemporary Lagrangian-based QFTs for why they should be interpreted.

4.0 THE INTERPRETIVE RELEVANCE OF THE RENORMALIZATION GROUP IN QUANTUM FIELD THEORY

4.1 INTRODUCTION

With some exceptions (Huggett & Weingard, 1995; Cao & Schweber, 1993; Wallace, 2011), philosophers of quantum field theory have not contributed much towards explicating the significance of the renormalization group for the interpretation of quantum field theory. In some cases this may be because they think the renormalization group is just a calculational method that gets us from the theoretical axioms of QFT to empirical predictions, as explained in Chapter 2. In this chapter I argue that philosophers engaged in interpreting QFT should pay attention to the renormalization group (RG), even if it is hard to characterize what mathematical objects it corresponds to. The interpretive relevance of the RG exposes the shortcomings of the sanitized approach. It is unclear how to include mathematical *techniques* like the RG, as opposed to mathematical *objects*, under the sanitized approach.[1]

In the philosophy of QFT, the split between those who prefer intepreting QFT solely based on axiomatic frameworks and those who pay attention to the more loosely organised inferential methods used by physicists has occurred largely because the methods used by physicists have been perceived as lacking in rigor (Fraser, 2009). More specifically, much of the physics literature on QFT uses perturbative QFT, in which the case where quantum fields do not interact at all is taken as a "base case" and interactions are introduced as small perturbations to the base case. However, many of the series expansions used in perturbative QFT are only formal expansions of apparently questionable mathematical validity. Further-

[1] Kaiser (2005) has made a similar point—that conventional views of what a physical theory consists in fail to take into account the significance of many theoretical *tools* used by physicists.

more, to avoid troublesome infinities that occur in these expansions, mathematical operations of apparently questionable mathematical validity are applied to these formal expansions.[2] The apparent lack of mathematical justification for perturbative QFT is one reason it has been dismissed as a candidate for interpretation.

A related approach to QFT that is widely adopted in the physics community is that of the effective field theory (EFT) framework. Effective field theories are theories that apply to only a restricted range of length and energy scales. In particular, these theories lose their validity at a high enough energy scale, or equivalently, a low enough length scale. In the EFT framework one assumes that all QFTs are EFTs: that they all have some length scale beyond which they are invalid. This approach has been characterized by, among others, Michael Redhead, as being "less intellectually exciting" than the pursuit of the "regulative ideal of an ultimate theory of everything" (Redhead, 2004, p. 40). Steven Weinberg countered that "this is analogous to saying that to balance your checkbook is to give up dreams of wealth and have a life that is intrinsically less exciting" (Weinberg, 2004, p. 250). Per Redhead, other philosophers have characterized EFTs as merely instrumental tools for prediction, while insisting that QFTs that are worthy of interpretation apply to all length scales, including arbitrarily small ones that are inaccessible to our experiments (Fraser, 2011).

In their flight from allegedly instrumentalist versions of QFT, many philosophers of QFT have focused on variants of axiomatic QFT as fodder for interpretation. They take the mathematical rigor of axiomatic QFT to be a reason to prefer it for the purposes of interpretation (Halvorson & Müger, 2006; Fraser, 2011). As a result, theoretical approaches to QFT that are not explicitly axiomatically formulated, such as the EFT framework, perturbative QFT, and the renormalization group, have received relatively less interpretive attention. This chapter tries to fill that deficit by arguing for the interpretive relevance of the renormalization group.

I suggest that the RG is relevant to interpreting QFT in the following ways:

1. The physical and mathematical picture painted by the RG explains the empirical success of some perturbative approaches to QFT. These perturbative approaches would otherwise

[2] I use the term "apparently" here because, as we shall see, one of the main payoffs of the RG is that it explains the inferential success of many instances of perturbative QFT.

appear incoherent from a mathematical point of view.[3]

2. The RG can tell us which QFTs lose applicability at small enough length scales and thus *must* be considered EFTs at most, and which QFTs are candidates for being genuine "continuum" QFTs that are potentially valid down to arbitrarily small length scales. It can also tell us when a perturbative expansion being used is valid in the regime of usage, and when the expansion is invalid and merely a "formal" symbolic manipulation that bears no relation to the function being approximated. This latter question is important for interpretation because it enables us to identify perturbative approaches that are inferentially sound and thus possible candidates for interpretation, while discarding perturbative expansions that are merely formal.

3. The RG suggests that EFT-based interpretations of QFT are *reliable* in a sense that is different from the *logic-based reliability* of the sanitized approach. Our inferences are reliable in this sense if their propensity to lead us to the truth is insensitive to unavoidable uncertainties in our knowledge. In the QFT context, these uncertainties are about the structure of the world at arbitrarily small length scales. Because our experiments can probe the structure of the world only down to some non-zero length scale, we necessarily have uncertainties about what the world is like beyond this experimental limit. In the physics community the consensus is that quantum gravity effects become important at the Planck scale, at which point the assumption of axiomatic QFT that spacetime has a Minkowski structure fails (Doplicher, Fredenhagen, & Roberts, 1995; Gibbs, 1996; Rivasseau, 1991). These are reasons to distrust what QFT says about phenomena at very small length scales. EFT-based interpretations are reliable in the sense described above because they are insensitive, in their claims about the ontology and dynamics of QFT, to assumptions about what the world is like at arbitrarily small length scales. This reliability makes EFT-based interpretations desirable, and it is the RG that gives us a

[3] Some readers might think this is just a restatement of the no-miracles argument—that I am arguing from the empirical success of a theory to that theory's truth. However, there is an important difference between the line I am taking here and the no-miracles argument. The "miracle" in question is not just that a scientific theory of some generic type is empirically successful, but that inferences based on symbolic manipulations with no apparent grounding in rigorous mathematics are empirically successful. In some sense this "miracle" is a *bigger* miracle than the usual one presented in the no-miracles argument. That is, the EFT framework does not address *only* the "miracle" of empirical success, it also provides a *mathematical justification* for perturbative QFT.

mathematical account of this reliability.

Some philosophers will deny the significance of some of the RG-based methods presented here on the grounds that they are not sufficiently mathematically rigorous. For these philosophers, I devote Section 4.6 to explaining how these methods are made rigorous in constructive QFT, a tradition of QFT that is accepted as rigorous by philosophers (Fraser, 2011). I explain how the RG remains interpretively significant in the construction of models in constructive QFT.

The plan for this chapter is as follows. In Section 4.2 I explain the concepts behind perturbative renormalization and the RG. I then explain in Section 4.3 how EFT is related to the RG. In Section 4.4 I show how the RG, far from being just a calculational instrument of no interpretive import, explains why our inferences in perturbative QFT have been so successful. I also explain how the RG helps us to delimit the domains in which perturbative QFT is valid, and to figure out which perturbative QFTs must be considered as EFTs and which could potentially apply to all length scales. In Section 4.5 I argue that the RG allows for reliable inferences about QFT that do not make unwarranted speculations about high energy phenomena that cannot be experimentally confirmed. In contrast, I argue, axiomatic approaches that do not utilise the RG make unwarranted speculations about high energy phenomena that then undermine the reliability of what they say about low energy phenomena. In Section 4.6 I sketch the "rigorous" version of the RG and show how it is also a route to microscopic physics in constructive QFT.

4.2 RENORMALIZATION IN PERTURBATIVE QFT

I now present the formalism of renormalization in perturbative QFT. First, I sketch the Lagrangian formalism commonly used in perturbative QFT and describe how that typically leads to infinities in one's calculations. Next, I explain how perturbative renormalization in QFT removes those infinities. Following that, I explain the approach in EFT of using finite cutoffs to deal with non-renormalizable interactions. Section 4.3 then explains how the renormalization group and EFT justify the apparently unrigorous mathematical manipulations of perturbative QFT.

4.2.1 Dynamical Framework and Quantities of Empirical Interest

The Lagrangian formalism of QFT is one of the most common dynamical treatments of QFT. For various reasons, this approach is the best suited for explaining effective field theory. However, one could express much of the key mathematics in QFT, including the partition functions and Green's functions, in the language of the canonical or Hamiltonian formalism. The renormalization group analysis is also valid in the canonical formalism.

In the Lagrangian approach, the dynamics of the theory are derived from a quantity known as the action:

$$S[\phi] = \int d^4x \mathcal{L}\left(\phi\left(x\right), \partial\phi\left(x\right)/\partial x^{\mu}\right),$$

where $\mathcal{L}[\phi]$ is the Lagrangian density of the quantum field ϕ. The form of the Lagrangian density is generally based on considerations of the kind of interactions we expect in the system of interest, and on the symmetries we expect the system to obey. While it is common, as a starting point, to rely on analogies with the form of the classical Lagrangian density, the quantum Lagrangian density will typically not be exactly the same as its classical cousin. For example, in both quantum electrodynamics (QED) and classical electrodynamics, the term $F_{\mu\nu}F^{\mu\nu}$ makes an appearance in the Lagrangian density. However, the QED Lagrangian density has additional terms containing terms relating to spin, which is a concept alien to classical electrodynamics. The QFT Lagrangian density typically also contains terms describing self-interactions which are missing from the classical Lagrangian density.

It is common in QFT to simply call the Lagrangian density the Lagrangian, and I will sometimes slip into this way of speaking. We will see some specific examples of Lagrangian densities later in this chapter.

Another quantity of central importance in QFT is the partition function, which is defined in terms of the action as follows:

$$Z = \int \mathcal{D}\phi e^{S[\phi]} \tag{4.1}$$

The "$\mathcal{D}$" indicates that this integral is a functional integral, sometimes called a Feynman path integral. Intuitively, the integration ranges over the space of "possible functions" ϕ, for some value of “possible”.[4] Path integrals also feature in expressions for the Green’s functions, which are closely related to experimental measurements.

Because QFT is confirmed largely through scattering experiments, the Lagrangian must relate to scattering amplitudes in some way. The results of scattering experiments are encoded in an entity known as the S matrix. The S matrix indicates the amplitudes of the various transitions of particles from one state to another during the scattering experiment, and the probability of a given transition is just the square of the amplitude associated with that transition. Via the Lehmann-Symanzik-Zimmermann reduction formula, the S matrix can be constructed out of correlation functions, also known as Green’s functions in the QFT context. The Green’s functions tell us the correlations of the quantum field between different points of spacetime. For example, the 2-point Green’s function tells us the correlation of the quantum field between two points in spacetime, (x_1, t_1) and (x_2, t_2). Mathematically, it is expressed as follows:

$$G_2\left(x_1, t_1; x_2; y_2\right) = \left\langle 0_{int} \,\middle|\, \mathcal{T}\psi\left(x_1, t_1\right)\psi^{\dagger}\left(x_2, t_2\right) \,\middle|\, 0_{int} \right\rangle ,$$

where $\mathcal{T}$ is a time-ordering operator that rearranges its arguments in chronological order and $|0_{int}\rangle$ indicates the ground state of the interacting quantum field.[5]

This correlation between the quantum field at different points of spacetime is typically interpreted as an instance of particle propagation or scattering. The 2-point Green’s function in particular is typically interpreted as describing a particle travelling between the two points (x_1, t_1) and (x_2, t_2). Scattering processes correspond to Green’s functions involving more spacetime points. Thus a 4-point Green’s function may describe a process of two particles scattering off each other into two end products.

Because of their close relationship with the S matrix and the fact that they can be interpreted as describing scattering processes, it is common to use the Green’s functions of

[4]See Section 4.6.1 for attempts to define a measure for the integral.

[5]To be precise, $\mathcal{T}$ acts on the $\psi\left(x_i, t_i\right)$ to its right such that the field at the smallest value of t_i is placed to the extreme right, and the remaining fields are arranged in ascending order of their t_i variable from right to left. So $\mathcal{T}\left(\psi\left(x_1, t_1\right)\psi^{\dagger}\left(x_2, t_2\right)\right) = \psi\left(x_1, t_1\right)\psi^{\dagger}\left(x_2, t_2\right)$ if $t_1 \geq t_2$, and $\psi^{\dagger}\left(x_2, t_2\right)\psi\left(x_1, t_1\right)$ otherwise. One can easily see how this rearrangement according to the values of the t_i can be extended to cases of more than two arguments.

a QFT as a proxy for the "physics" described by a QFT. Physicists, for example, speak of keeping the Green's functions invariant as a shorthand for keeping the physics invariant.

Green's functions are derived from the Lagrangian of a system by functional integrals. For instance, the 2-point Green's function of a scalar quantum field ψ can be derived from its Lagrangian as follows:

$$G_2\left(x_1, t_1; x_2.t_2\right) = \frac{\int \psi\left(x_1, t_1\right) \psi^\dagger\left(x_2, t_2\right) e^{iS[\psi,\psi^\dagger]/h} \mathcal{D}\psi \mathcal{D}\psi^\dagger}{\int e^{iS[\psi,\psi^\dagger]/h} \mathcal{D}\psi \mathcal{D}\psi^\dagger}$$

Since S matrix elements can be derived from Green's functions, the above equation provides the link between the dynamics of a QFT, as encoded in its Lagrangian, and the results of scattering experiments.

4.2.2 Regularization and Perturbative Renormalization

The path integrals mentioned above can be given a straightforward finite, analytic expression when the action involved is that of a free scalar field with no interactions, also known as a "Gaussian" field. In this case, $\mathcal{L} = \frac{1}{2}\left(\left(\partial\phi^2\right)^2 - m^2\phi^2\right)$. For interacting fields, physicists typically use perturbation theory to evaluate the path integrals. Since the path integral for the free field has a known analytic expression, the perturbations are applied using the free field case as a reference—we consider the interaction as a small perturbation to the free field Lagrangian. The following example illustrates how this is done in a simple case.

Suppose a small interaction $-\frac{\lambda}{4!}\phi^4$ is added to the free field Lagrangian, so that $\mathcal{L} = \frac{1}{2}\left(\left(\partial\phi^2\right)^2 - m^2\phi^2\right) - \frac{\lambda}{4!}\phi^4$. This is the Lagrangian of the so-called ϕ^4 theory, which describes a self-interacting scalar field. The partition function is

$$Z = \int \mathcal{D}\phi e^{\int d^4x\left(\left(\left(\partial\phi^2\right)^2 - m^2\phi^2\right) - \frac{\lambda}{4!}\phi^4\right)}.$$

Assuming λ to be small, we then convert the $e^{-\frac{\lambda}{4!}\phi^4}$ factor into a Taylor series in λ:

$$Z = \int \mathcal{D}\phi \left(1 - \frac{\lambda}{4!}\int_{x_1} \phi^2\left(x_1\right) dx_1 + \frac{1}{2}\left(\frac{\lambda}{4!}\right)^2 \int_{x_1,x_2} \phi^4\left(x_1\right)\phi^4\left(x_2\right) dx_1 dx_2 + \cdots\right) e^{\int d^4x\left(\left(\partial\phi^2\right)^2 - m^2\phi^2\right)}$$

where I have included only the first two terms of the Taylor series to illustrate the general rule.

Unlike in the free field case, when evaluating path integrals such as the above, infinities known as *divergences* often arise in one's calculations. These infinities make it difficult to directly compute experimental quantities known to be finite, such as scattering cross-sections, from the path integral. To deal with these infinities in the perturbative context, physicists carry out the operations of regularization, subtraction of counterterms, and renormalization. I will now explain what each of these involves.

4.2.2.1 Counterterms, Regularization and Renormalization

The perturbation expansion may be represented graphically by Feynman diagrams, which also depict the possible space-time processes contributing to the cross-section. Feynman diagrams are a useful tool for evaluating the perturbation expansion because one can correlate individual diagrams with individual terms in the expansion. In QED, for example, each graph may be represented by an integral of the form

$$\int \frac{d^{4N}k}{\left(k^2\right)^{P_i} k^{E_i}}, \tag{4.2}$$

where P_i is the number of internal photon lines in the graph, E_i the number of internal electron lines in the graph, and N the number of independent internal 4-momenta in the graph. k denotes momentum, so the integral is over the space of momenta. If no cutoff on momentum is imposed, which is to say that processes involving unrestricted values of momentum are taken into account, then we take the integral from 0 to infinity. Without a cutoff, this integral is generally divergent.

Divergences present a problem because they occur in the calculation of quantities, such as the electron's self-energy, that we expect, for both theoretical and experimental reasons, to be finite. To deal with divergences, regularization methods are used. Some regularization methods involve an explicit cutoff, in which momenta above a certain cutoff momentum are, in some way or other, ignored. In Equation 4.2, an explicit cutoff could be implemented by taking the integral over momenta from 0 to a finite momentum Λ. Alternatively, one could "weight" the contributions from higher momenta in a way that smoothly decreases as momentum increases, without setting them to zero (Duncan, 2012, p. 548). In other methods, such as dimensional regularization, there is no explicit cutoff, but contributions from different scales are distorted, leading to an *effective* cutoff.[6] In non-perturbative formulations of QFT, lattice regularization, in which the theory operates over a discrete lattice of points in spacetime rather than a continuum, is used. However, all these regularization methods are thought to lead to the same results modulo some local terms. This means that the differences in results from different regularization methods can be absorbed into redefinitions of the coupling constants—the non-trivial structure of the Feynman diagrams is preserved across all regularization methods (Collins, 1986, p. 13). I use dimensional regularization in the example below only because it offers a particularly simple illustration of how regularization works.

Regularization involves subtracting terms known as regularization terms from the divergent integrals. In order to compensate for the subtraction of these terms, counterterms are added to the Lagrangian density. Before the development of rigorous methods for renormalization, regularization was thought to lack mathematical rigor. The subtraction of infinities was thought to be ill-defined and susceptible to ambiguities. However, it has been proven rigorously that the subtracting of regularization terms is equivalent to formally adding counterterms to the Lagrangian density (Manoukian, 1983). Thus, in the following I will use the "formal" approach of adding counterterms to compensate for subtracting regularization terms, with the understanding that this formal treatment can be given a rigorous justification.

A theory is said to be *perturbatively renormalizable* if after regularization and the adding of counterterms, the number of coupling parameters in the theory is finite and constant at each order in perturbation theory. Another way of putting this is that renormalization occurs when the effects of regularization and the adding of counterterms are absorbed entirely by changing only the values of a finite number of coupling parameters. For example, in the so-called ϕ^4 theory, the Lagrangian we start with *before* regularization and renormalization

[6] Dimensional regularization works by considering the theory in dimensions $4 - \epsilon$ instead of 4. The calculation of intermediate quantities is done in dimension $4 - \epsilon$. To get results that apply in four dimensions, one takes the limit $\epsilon \to 0$ at the end of one's calculations. In changing the theory from dimension 4 to dimension $4 - \epsilon$, dimensional counting considerations lead to a new scale μ being introduced into the Lagrangian, so that the coupling constants become implicitly dependent on μ. This is the sense in which dimensional regularization also causes a change in the scaling behaviour of the theory which, at the low energies experimentally accessible to us, is empirically equivalent to the changes in scaling caused by cutoff regularization.

is $\mathcal{L} = \frac{1}{2}(\partial\phi)^2 - \frac{1}{2}m^2\phi^2 - \frac{\lambda}{4!}\phi^4$. In carrying out dimensional regularization, we first go from 4 dimensions to $4 - \epsilon$ dimensions. This introduces a new scale μ and an ϵ exponent into the Lagrangian, so that $\mathcal{L} = \frac{1}{2}(\partial\phi)^2 - \frac{1}{2}m^2\phi^2 - \mu^{2\epsilon}\frac{\lambda}{4!}\phi^4$ after regularization. After both dimensional regularization and renormalization, we obtain a renormalized Lagrangian $\mathcal{L}_{ren}$ with counterterms $\mathcal{L}_{ct}$:

$$\mathcal{L}_{ren} = \mathcal{L} + \mathcal{L}_{ct},$$

where

$$\mathcal{L}_{ren} = \frac{1}{2}(\partial\phi_0)^2 - \frac{1}{2}m_0^2\phi_0^2 - \frac{\lambda_0}{4!}\phi_0^4,$$
$$\mathcal{L} = \frac{1}{2}(\partial\phi)^2 - \frac{1}{2}m^2\phi^2 - \mu^{2\epsilon}\frac{\lambda}{4!}\phi^4,$$

and

$$\mathcal{L}_{ct} = \frac{1}{2}A(\partial\phi)^2 - \frac{1}{2}m^2B\phi^2 - \mu^{2\epsilon}\frac{\lambda}{4!}\phi^4.$$

The coupling parameters ϕ, m and λ have been renormalized as follows:

$$\phi_0 = (1+A)^{1/2}\phi,$$

$$m_0^2 = m^2\frac{1+B}{1+A},$$
$$\lambda_0 = \lambda\mu^{2\epsilon}\frac{1+C}{(1+A)^2}.$$

A, B and C are constants that determine how the coupling parameters are renormalized. As one can see, the counterterms that are added preserve the form of the interactions in the original Lagrangian, so that one has to change only the existing finitely many coupling parameters in $\mathcal{L}$ in order to obtain $\mathcal{L}_{ren}$. This is why the ϕ^4 theory is said to be perturbatively renormalizable.

Not every theory can be perturbatively renormalized. *Perturbatively non-renormalizable* theories are those for which regularization forces one to add counterterms which change the Lagrangian in a way that cannot be accounted for by merely changing the existing finitely many coupling parameters. Instead, either an infinite number of coupling constants must be changed, or new interactions with new coupling parameters must be added to the Lagrangian. Perturbative non-renormalizability also means that perturbatively non-renormalizable theories do not yield finite measurable quantities in the continuum limit. The

Fermi theory of the weak nuclear force is an example of a perturbatively non-renormalizable theory.

4.2.2.2 Perturbative Renormalization and Cutoffs The process of adding counterterms to obtain a renormalized Lagrangian outlined above can also be understood in terms of changing the cutoff used in regularization. Recall that one can regularize integrals of the form of Equation 4.2 by not integrating over all momentum space, but by integrating over momenta only up to a cutoff Λ. In physical terms, this means that processes involving momenta above that cutoff are neglected in one's calculations. When using cutoff regularization, one typically chooses a cutoff that represents the scale at which one expects the QFT one is working with to lose its validity. The coupling parameters, which are the renormalized quantities, become a function of the cutoff. Renormalization then becomes a process of changing the cutoff, and thus changing the coupling parameters. In QED, for example, cutoff regularization and renormalization amount to merely changing the coupling parameters e_Λ, m_Λ in the Lagrangian $\mathcal{L} = \overline{\psi}(i\gamma \cdot \partial - e_\Lambda \gamma \cdot A - m_\Lambda)\psi - \frac{1}{2}F_{\mu\nu}F^{\mu\nu}$.

When working with perturbatively renormalizable theories, it is common to take the continuum limit after regularizing, renormalizing, and finding mathematical expressions for the empirically measurable quantities. If the method of regularization used was a cutoff, then taking the continuum limit means taking the cutoff to infinity. One interpretation of this practice is that by taking the cutoff to infinity, one is implementing QFT as a true "field theory" that treats spacetime as a continuum describing phenomena at arbitrarily small length scales (Fraser, 2011). Another reason that the continuum limit is often taken is that one may not know of any physical reasons, such as the failure of the theory to apply beyond a particular scale, to expect the cutoff to be any particular finite value. Taking the cutoff to infinity is then a way of sidestepping the arbitrariness that would accompany any finite cutoff. In many cases, this practice of taking the cutoff to infinity yields good empirical predictions.[7] However, for a large enough cutoff, keeping the cutoff finite and taking it to

[7]One might be puzzled as to why taking the cutoff to infinity does not lead to divergent predictions. If we keep the cutoff finite when expanding the perturbation series, we find that the terms in the perturbation series cancel in such a way that the only terms that do not cancel are the ones that are finite as the cutoff goes to infinity. However, if we take the cutoff to be infinite from the start, then we will encounter divergent terms in the series, and these do not "cancel" in any mathematically rigorous sense.

infinity both lead to the same empirical predictions, within experimental error.

Described merely in terms of subtracting counterterms to obtain finite predictions, it may seem that perturbative renormalizability and non-renormalizability are merely statements about the calculational effectiveness of perturbation theory. It may also seem that regularization is simply a means to obtaining the correct "continuum" predictions, since the cutoff is often taken to infinity. These techniques may appear to be merely instruments for getting from the fundamental laws to empirical predictions. However, the EFT approach suggests a different perspective on perturbative renormalization and cutoffs. We saw that the subtracting of counterterms is equivalent to changing the cutoff in the theory. In the EFT framework, we will see that these cutoffs can often be interpreted as delineating the scales at which QFTs lose their applicability.

There are other ways in which the EFT framework makes sense of perturbative renormalization as more than a calculational instrument. It also sheds light on why perturbative renormalization is a reliable way of making inferences in QFT. Furthermore, the EFT approach is not restricted to perturbatively renormalizable theories. The EFT perspective suggests that non-renormalizable theories, despite not yielding finite results for experimentally measurable quantities in the continuum limit, do have a role to play in our interpretation of QFT. In what follows, we will see how the EFT framework relates to the processes of renormalization described earlier.

4.3 THE EFFECTIVE FIELD THEORY FRAMEWORK AND THE RENORMALIZATION GROUP

Since the late 1980s, physicists' understanding of QFT has increasingly been shaped by the perspective of effective field theory (EFT) and the renormalization group (RG). This perspective rationalizes the apparently mysterious subtraction of infinities in perturbative renormalization. Crucially, renormalization group techniques can be formulated *non-perturbatively*, thus avoiding the charges of lack of rigor that accompany the subtraction of infinities in perturbative renormalization.[8] In this section, I will explain as much of the mathematics of the EFT and RG as is needed to explain why they are thought to rationalize perturbative renormalization. Then, I will explain how the RG tells us that the low-energy dynamics we experimentally measure display *universality*—they are insensitive to the exact form of the unknown high-energy Lagrangian. This suggests that inferences based on speculations about the form of the high-energy Lagrangian is necessarily will be unreliable.

Although there are a few regularization methods available, for the purposes of illustrating the important concepts behind the renormalization group and EFT, it is easiest to use the method that involves imposing a sharp momentum cutoff at some momentum Λ. Λ separates the high momentum terms ϕ_H in the Lagrangian from the low momentum terms ϕ_L.

In Wilson's momentum-space account of renormalization, one integrates out the ϕ_H in order to obtain a "Wilsonian effective action" $S_\Lambda[\phi_L]$ from the full action $S[\phi_H, \phi_L]$:

$$\int \mathcal{D}\phi_L \int \mathcal{D}\phi_H e^{iS[\phi_H,\phi_L]} = \int \mathcal{D}\phi_L e^{iS_\Lambda[\phi_L]}$$

Here, the notation $\mathcal{D}\phi$ indicates that the integral concerned is a *functional integral*: an integral over all smooth functions ϕ. $S_\Lambda[\phi_L]$ is known as the effective action because it "acts like the full action" but involves fewer degrees of freedom.[9] It is that thing that behaves like the full action when we want to describe our system with a reduced set of variables, that is, with only ϕ_L instead of $\phi_L + \phi_H$. The coupling parameters in the action are *re-scaled* when we go from $S[\phi_H, \phi_L]$ to $S[\phi_L]$. Figure 1 depicts how the integrating out of the high-momentum or fast modes and re-scaling works on modes in momentum space. The rescaled fields ϕ_L are indicated by (c) in the diagram: the fast modes have been explicitly removed as degrees of freedom but their effects are encoded in the new action $S[\phi_L]$.

Just like in the case of regularization in perturbative renormalization, the integrating out of high energy degrees of freedom in EFTs changes the coupling parameters in the original Lagrangian. The combined process of cutting off high-momentum modes, rescaling momentum, and changing the coupling parameters is known as an *RG transformation*. The

[8] However, as I explain in Section 4.6, non-perturbative methods may still involve uncontrollable approximations.

[9] Strictly speaking, as we will see below, it behaves like the full action up to a finite accuracy—the integrating out of the higher energy degrees of freedom is compensated for only up to a finite degree of accuracy.

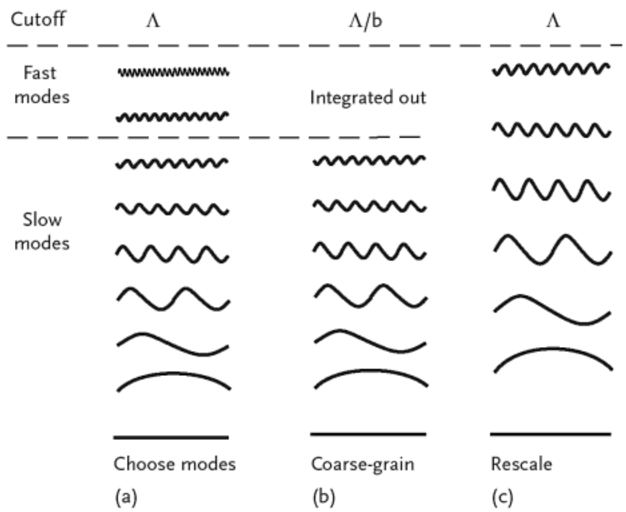

Figure 1: Integrating out the fast momentum modes and rescaling the remaining modes. Figure taken from Huang (1998).

RG transformation is often said to keep the physics the same because the Green's functions are invariant under the RG transformation.

Repeated applications of the RG transformation change the coupling parameters in the effective action in ways that are described by equations known as the *renormalization group equations*. These describe how the coupling parameters vary with the cutoff Λ. We can see an example of this in quantum electrodynamics (QED). In QED, the Lagrangian is

$$\mathcal{L} = \overline{\psi}(i\gamma^\mu \partial_\mu - e_\Lambda \gamma^\mu A_\mu - m_\Lambda)\psi - \frac{1}{2}F_{\mu\nu}F^{\mu\nu},$$

where γ^μ are the Dirac gamma matrices, A_μ is the vector potential, and $F_{\mu\nu}$ the electromagnetic field tensor.[10]

[10] The ingredients that go into this Lagrangian are as follows. There is a Lagrangian corresponding to a Dirac field, $\mathcal{L}_{Dirac} = \overline{\psi}\,(i\gamma^\mu \partial_\mu - m)\,\psi$. This represents the dynamics of the electron field and is related to the Dirac equation for an electron, $(i\gamma^\mu \partial_\mu - m)\,\psi\,(x) = 0$. The external electromagnetic field is represented by the Lagrangian $\mathcal{L}_{em} = -\frac{1}{4}F^{\mu\nu}F_{\mu\nu}$, as in the classical electromagnetic Lagrangian. The interaction between the external electromagnetic field and the Dirac field is given by $\mathcal{L}_{int} = e\overline{\psi}\gamma^\mu A_\mu \psi$. $\mathcal{L}_{Dirac} + \mathcal{L}_{em} + \mathcal{L}_{int}$ gives us the QED Lagrangian above.

The coupling parameters that vary with Λ are the effective electron charge and mass, e_Λ and m_Λ. They vary with Λ according to two evolution equations:

$$\Lambda \frac{de_\Lambda}{d\Lambda} = \beta(e_\Lambda)$$

$$\Lambda \frac{dm_\Lambda}{d\Lambda} = m_\Lambda \gamma_m(e_\Lambda)$$

These are the renormalization group equations.

Since each combination of coupling parameters (e_Λ, m_Λ) characterises a particular action, the evolution equations describe a flow in the *space of actions* or *space of coupling parameters* that represents the possible actions the theory takes on as the cutoff is lowered. Each point on the flow represents the *effective dynamics* at scales lower than the cutoff.

A *fixed point* of an RG flow is defined as the point at which the flow maps the point onto itself: where the coupling parameters stop changing with Λ. In the example above, this would correspond to $\frac{de_\Lambda}{d\Lambda} = \frac{dm_\Lambda}{d\Lambda} = 0$. An *ultraviolet RG trajectory* is one where lowering the cutoff takes one further away from the fixed point. The fixed point is then called an ultraviolet (UV) fixed point. An *infrared RG trajectory* is one where lowering the cutoff takes one closer to the fixed point, which is then known as an infrared (IR) fixed point. As we will see later, the nature of the flow and fixed point is important for questions of the "triviality" of theories and of whether perturbative renormalization is valid.

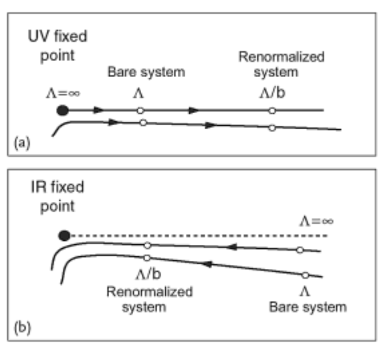

Figure 2: (a) represents an ultraviolet RG trajectory. The cutoff is infinite at the fixed point and decreases as one moves away from the fixed point. (b) represents an infrared RG trajectory. Figures taken from (Huang, 1998).

If the RG flow is an ultraviolet trajectory, then the fixed point is the point at which the predictions and parameters of the theory converge to finite values when we take $\Lambda \to \infty$. Because the cutoff is infinite at this point, the action at the fixed point is independent of the cutoff and gives us predictions that are independent of the cutoff. Furthermore, the convergence also implies that for a high cutoff close to the fixed point, the precise value of the cutoff has negligibly small effects—close to the fixed point, the theory is *nearly* independent of the value of the cutoff. These properties will turn out to have important philosophical implications.

In the above example of QED, we had just two coupling parameters. In general, one might have a theory with N coupling parameters g_i, $i = 1, \cdots, N$. For each coupling parameter g_i one has an RG equation

$$\mu \frac{dg_i}{d\mu} = \beta_i(g_1, \cdots, g_N).$$

Using the reparametrization $t = \log(\mu/\mu_0)$, μ_0 constant, one can rewrite the RG equations as follows:

$$\frac{dg_i}{dt} = \beta_i(g_1, \cdots, g_N).$$

The fixed point is the point at which $\beta_i = 0$ for all i—the flow converges here since $dg_i/dt = 0$ at this point. By studying the behaviour of the system near the fixed point, we can learn about the behaviour of the system when we include an arbitrarily high momentum cutoff. We can also learn about *universality*: the phenomenon of multiple theories differing in their high-energy behaviour but manifesting the same (or nearly the same) low-energy behaviour.

4.3.1 The RG and Perturbative Renormalization

Using the RG, we can also better understand the concepts of perturbative renormalizability and non-renormalizability, relating them to the RG flows associated with a theory. My account below follows that of Polchinski (1999). The effective action S_Λ described above can generally be expanded in a series of local operators that are combinations of the derivatives of the local fields involved (and powers thereof) and powers of the local fields involved. We include all local operators that are compatible with the known symmetries of the theory. The series is generally infinite, with the powers of the local fields becoming arbitrarily high.

Perturbative renormalizability and non-renormalizability can be understood in terms of the identification of three kinds of terms in the effective action: marginal, relevant, and irrelevant. Expanding the action $S_\Lambda[\phi_L]$ in terms of local operators $\mathcal{O}_i$, we get:

$$S_\Lambda = \int d^D x \sum_i a_i \mathcal{O}_i, \tag{4.3}$$

where each a_i is a coupling parameter associated with the corresponding local operator $\mathcal{O}_i$. D denotes the number of dimensions of spacetime. The local operators $\mathcal{O}_i$ include all possible interactions consistent with the symmetries of the theory. For a scalar field theory with scalar field ϕ, for example, each O_i corresponds to all powers of ϕ and its derivatives, excluding only those terms inconsistent with known symmetries.

Let E be the energy scale at which our low-energy experimental observations are conducted. Let Λ be the characteristic scale of the interactions. Λ will typically be much larger than E. Define δ_i such that $\mathcal{O}_i$ has units E^{δ_i}. Dimensional considerations suggest that the ith term in this expansion is of order

$$g_i \left(\frac{E}{\Lambda}\right)^{\delta_i - D}, \tag{4.4}$$

where the dimensionless couplings g_i are defined by $g_i = \Lambda^{\delta_i - D} a_i$. From Equation 4.4 we can categorise the terms in the local operator expansion of S_Λ into three categories:

1. *Relevant* terms are those for which $\delta_i < D$. These terms become larger and hence more important at lower experimental energies, where E/Λ is small.
2. *Marginal* terms are those for which $\delta_k = D$. These terms do not change as E decreases.
3. *Irrelevant* terms are those for which $\delta_i > D$. These decrease as E decreases.

The above distinctions are important in understanding how the renormalization group in the effective field theory program relates to the process of perturbative renormalization described in the previous section. Recall that a theory is *perturbatively renormalizable* if the process of renormalization keeps the number of coupling constants at each order in perturbation theory constant. In the EFT framework, perturbative renormalizability amounts to the absence of irrelevant terms in the local operator expansion of S_Λ. One can show via a graphical argument that if there are any irrelevant terms at all, then an infinite number of coupling constants are required to describe the physics correctly at each order in perturbation theory (Duncan, 2012, p. 644).

On the flip side, theories with irrelevant terms are those that are perturbatively non-renormalizable. These theories are also *literally* non-renormalizable in the following sense. If we take "renormalization" to mean *only* the avoidance of divergences by adding counterterms to the Lagrangian, then the process of renormalization described in Section 4.2.2 does not work for theories with irrelevant terms, since those terms become larger as $\Lambda \to \infty$. However, as we shall see, the EFT framework allows us to not only extract finitely valued predictions from non-renormalizable theories, but also to attach interpretations to them. Furthermore, there is no *a priori* reason to expect that all theories that accurately describe the world are perturbatively renormalizable. If we are willing to consider perturbatively non-renormalizable theories as possible descriptions of the world, then the EFT framework is not just convenient but essential for interpreting QFT.

4.3.2 Non-renormalizable theories

Taking the cutoff to infinity is problematic for theories that are not perturbatively renormalizable, because these theories contain irrelevant terms that grow indefinitely as the cutoff increases. This creates problems when we try to imitate the procedure of perturbative renormalization by taking the cutoff to infinity. In the language of perturbative renormalization, when one tries to regularize a non-renormalizable theory, one finds that one has to add an infinite number of terms to the new Lagrangian, instead of simply changing the coupling parameters in the original Lagrangian. An infinite number of terms means an infinite number of coupling parameters that have to be fixed by experiment in order to have a theory that can produce numerical predictions. Since it is not possible for experimentalists to fix an infinite number of coupling parameters, non-renormalizable theories are thought to be problematic for perturbative renormalization. Yet, there is no reason why our Lagrangians should have only finitely many terms—the symmetries of our theories generally allow for infinitely many terms.

To address these issues, the strategy with non-renormalizable theories is to work with a finite cutoff and require only a finite accuracy from the theory. The finite cutoff is understood to be a scale beyond which the theory is inapplicable. Working with a finite cutoff removes the need for regularization, since the integrals needed for the calculation of observable quantities no longer diverge when the cutoff is finite. The exact value of the cutoff depends on the QFT we are interested in. One may, for example, take the cutoff to be the Planck scale, beyond which considerations from general relativity render the applicability of Minkowski spacetime-based field theory dubious.

To deal with the problem that as the cutoff is lowered towards experimentally feasible energies, arbitrarily many terms have to be added to the Lagrangian to retain the same physics, we require only a finite accuracy from our theory. With a finite cutoff Λ, one can show that so long as one is interested in an accuracy of $(p/\Lambda)^n$, where p are the external momenta of the scattering process of interest, then only a finite number of new interaction terms representing local interactions need to be added to the low-energy Lagrangian to account for interactions at higher momenta. The infinitely many other interaction terms are suppressed by factors smaller than $(p/\Lambda)^n$ and need not be accounted for.

In addition, the cutoff Λ is informative in other ways that an infinite cutoff is not. It tells us when the cutoff theory will fail to accurately describe high energy processes. For example, when $p \sim \Lambda$, then the errors incurred by discarding parameters with factors $(p/\Lambda)^n$ become large. At this momentum scale, we can expect to see new physics not accounted for by the low-energy theory. In this way, EFTs with finite cutoffs are able to give us pointers as to where to expect new physics to be found.

The informativeness of non-renormalizable theories, once considered in an EFT framework, suggests that the cutoff can be more than a mere instrument for obtaining finite predictions. In the EFT framework, a cutoff can be assigned a realistic interpretation. Indeed, far from the cutoff serving merely as a tool to implement the technique of perturbative renormalization, only to be taken to infinity at the end of our calculations, it is the existence of a realistically interpreted cutoff in EFT that *explains* the empirical success of perturbative renormalization. It explains why we can achieve empirical success with only finitely many interaction terms in our Lagrangian, when the symmetries of the theory allow for an infinite number of interaction terms.

4.3.3 Universality

I will now elaborate further on how the RG explains perturbative renormalization. I will explain the assertion made above that a realistically interpreted cutoff in EFT explains the empirical success of perturbative renormalization. I will also argue that the RG explains why EFT can be so empirically successful despite its assumption that we do not have access to the details of phenomena at high enough energy scales. This is explained by *universality*: the fact that multiple, wildly differing accounts of what goes on at high energies can nevertheless have the same effective low energy description.

These points will be made by adopting the perspective of the RG: considering a space of coupling parameters in which individual theories exist. Each theory is identified by the distinctive combination of coupling parameters in its Lagrangian. As explained above, an RG flow is a curve in this space of coupling parameters, telling us how coupling parameters, and their corresponding theories, change with scale.

In this subsection I will adopt the analysis offered by Duncan (2012, pp. 653-7). We assume that there exists a high energy cutoff Λ_{UV} at which quantum gravity effects are expected to become important and the assumption of Minkowski spacetime becomes inadequate. In the literature this cutoff is often known as the "bare scale". We then consider RG flows from this high energy cutoff to a lower energy scale μ which represents the scale at which we can make experimental observations. We assume that the perturbation theory used in the QFT of interest involves weak couplings.[11] We assume that there are N relevant and marginal operators at the scale Λ_{UV}.[12] The number of irrelevant operators is left completely open, and is generally infinite.

With these assumptions, Duncan derives the following important result: Up to an accuracy of inverse powers of Λ_{UV}, the RG flow maps an arbitrary initial hypersurface in the space of high-energy coupling constants to a low-energy N-dimensional hypersurface defined by the N marginal and relevant operators. Note that the initial hypersurface in the space of high-energy coupling constants at the scale of Λ_{UV} is defined not just by the marginal

[11] Since QCD has strong couplings, perturbation theory is not valid for QCD and different techniques, like lattice field theory, are required.

[12] By dimensional arguments, one can show that there are only finitely many relevant and marginal operators (Duncan, 2012, p. 575).

and relevant operators, but also the irrelevant operators. Thus, the initial hypersurface encompasses both perturbatively renormalizable and non-renormalizable high-energy theories. What this result shows is that the effective low-energy theory that has only N marginal and relevant operators, and which is thus perturbatively renormalizable, is consistent with a much wider class of both perturbatively renormalizable and non-renormalizable theories at higher energy scales beyond the reach of our experiments. After giving a sketch of how Duncan derives the result, I will explain its philosophical import.

Recall the local operator expansion described in Section 4.3.1 (Equation 4.3). Consider the dimensionless couplings $g_n(\Lambda)$ in the local operator expansion (Equation 4.4). Suppose we can fix the couplings at the scale Λ_{UV}:

$$\overline{g_n} = g_n(\Lambda_{UV})$$

The $\overline{g_n}$ will serve as the "initial conditions" for the RG flow to the low energy scale μ. The RG flow is determined by the RG equations $\mu\frac{\partial}{\partial\mu}g_N(\mu) = \beta_n(g_n(\mu))$, where μ is any scale lower than Λ_{UV}. To study universality, we conduct a linear stability analysis on the couplings g_n. We consider what happens when we change the couplings by a small amount $\delta g_n(\mu)$. To first order, we get the following:

$$\mu\frac{\partial}{\partial\mu}\delta g_n(\mu) = \frac{\partial\beta_n}{\partial g_m}\delta g_m(\mu),$$

using the Einstein summation convention so that the right hand side involves a sum over the indices m. We define a matrix G_{nm} denoting how the low-energy couplings g_n vary with the initial couplings $\overline{g}_n$:

$$G_{nm}(\mu) = \frac{\partial g_n}{\partial\overline{g}_m}$$

From these, Duncan derives the central result:

$$\delta g_\alpha(\mu) \sim G_{\alpha a}G^{-1}_{ab}\delta g_b(\mu) + O\left((\mu/\Lambda_{UV})^{|d_\alpha|}\right), \tag{4.5}$$

where the index α denotes the indices of the irrelevant couplings only and the index b denotes the indices of the marginal and relevant couplings only. d_α denotes the mass dimension of the couplings g_n in the local operator expansion we saw in Equation 4.3.

The key lesson to take home from Equation 4.5 is as follows: Up to powers of μ/Λ_{UV}, variations in the irrelevant couplings g_α can be compensated for by variations in the marginal and relevant couplings g_b. Since $\mu << \Lambda_{UV}$, this means that variations in the irrelevant couplings are compensated to a high degree of accuracy by variations in the marginal and relevant couplings.

It may be helpful to think of this graphically. The initial couplings $\bar{g}_n$ lie on a hypersurface at the high energy scale Λ_{UV} (see Figure 3). At Λ_{UV} the full Lagrangian has N marginal

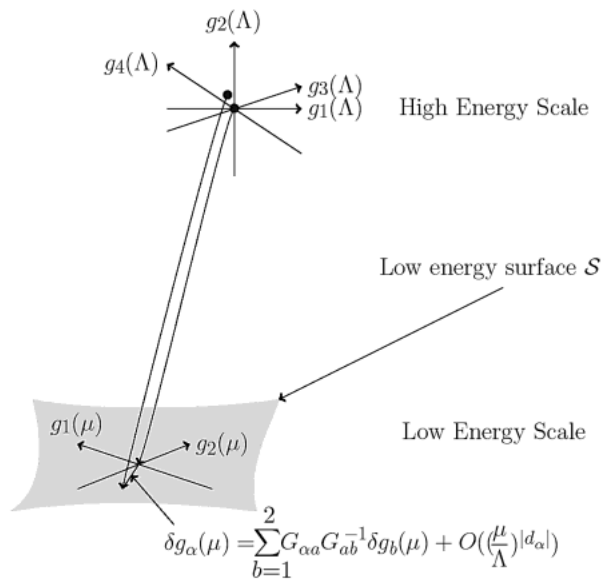

Figure 3: Multiple high energy theories in the full coupling parameter space are attracted to a lower-dimensional manifold at a lower energy scale. Figure taken from Duncan (2012, p. 657).

and relevant operators and possibly infinitely many irrelevant operators. The RG flow starts on this high energy hypersurface. However, even though the initial couplings may contain irrelevant operators, the RG flow, when evolved down to the lower scale μ, terminates on an N-dimensional hypersurface delineated by only the N marginal and relevant operators.[13] This convergence onto a hypersurface of lower dimension at low energies, despite the possible existence of arbitrarily many irrelevant operators at high energies, is known as *universality*.

[13]Up to powers of μ/Λ_{UV}, a qualification that I will drop from hereon for brevity's sake.

The huge variety of theories available at the energy scale Λ_{UV}, most of them with infinitely many coupling parameters, all turn out to be effectively described by the same N number of parameters at the energy scale μ.

The universality demonstrated by the above analysis explains the success of perturbative renormalization as follows. Recall that in perturbative renormalization, no irrelevant operators appear at all. The EFT framework, however, allows for an arbitrary number of irrelevant operators. It includes cases where there are no irrelevant operators. So one can consider the limited number of operators in perturbative renormalization to be a particular choice of couplings in the EFT framework. Within the EFT framework, perturbative renormalization consists of fixing the couplings of the irrelevant operators to be zero at Λ_{UV}:

$$\overline{g_\alpha}(\Lambda_{UV}) = 0$$

Meanwhile, the couplings of the relevant and marginal operators at Λ_{UV} are chosen so that the RG flow from Λ_{UV} to μ will produce coupling parameters at μ that match the empricially determined coupling parameters of the low-energy theory. These choices of initial coupling parameters at Λ_{UV} are the so-called "renormalization conditions". In the case of QED, the coupling parameters fixed by experiment at low energies are e_Λ and m_Λ, the effective electron charge and effective electron mass. Thus, there are two renormalization conditions for QED: two choices of $\overline{g}_a(\Lambda_{UV})$ that will suffice to determine, via the RG flow from Λ_{UV} to μ, the coupling parameters for the low-energy theory.

The perturbatively renormalizable Lagrangian lies in the same universality class as the "perfect action" which accurately describes nature even at Λ_{UV} and likely contains irrelevant operators. This universality explains why our perturbatively renormalizable theories can still be successful at describing phenomena at scales much smaller than Λ_{UV}. Thus, it is universality as displayed by the RG analysis that explains why our technique of perturbative renormalization, including its blithe assumption that there are no irrelevant operators, can be so empirically successful.

The RG explains the success of perturbative renormalization in other respects. For example, consider the typical move in perturbative renormalization of taking the cutoff to infinity after one has obtained renormalized expressions of empirically observable quantities.

Why does this "continuum limit" succeed after renormalization, especially since taking the cutoff to infinity *before* renormalization would merely have produced divergent results? In the EFT framework, the success of the continuum limit is explained in terms of the success of high-cutoff effective theories combined with the fixed point structure of the RG flow. On an ultraviolet RG flow near its fixed point, the empirical differences between a theory that takes $\Lambda \to \infty$ (a theory at the fixed point) and a theory with large but finite Λ on the flow near the fixed point become negligible. Thus, instead of taking $\Lambda \to \infty$ as is often done in perturbative renormalization, we could always stick with large but finite Λ. However, this practical equivalence of $\Lambda \to \infty$ with Λ large but finite holds only near an ultraviolet RG flow away from a fixed point. In general, one cannot expect that taking Λ to infinity should produce good empirical predictions at the low energy levels that characterise our particle physics experiments. However, as explained above, the RG explains how the low energy details that we observe are insensitive to the high energy details of our EFT. Thus, we can work with a perturbatively renormalizable theory that has a high cutoff and still expect, if we apply the correct intial parameters for the RG flow, that it reproduces our low energy observations. We can do this even if we cannot verify that the theory is correct in its high energy details. If, furthermore, this high-cutoff theory is near the fixed point of an ultraviolet RG flow, then it produces, for all practical purposes, the same predictions as a procedure in which the cutoff is taken to infinity.

4.4 THE RENORMALIZATION GROUP TELLS US WHEN PERTURBATIVE QFT IS VALID

In this section I argue for the interpretive relevance of the renormalization group to QFT by describing how the RG can tell us whether or not a given perturbative expansion describing a QFT counts as a mathematically valid expression, and in which domains of the world this expansion can be expected to be valid. Since the mathematical validity of a theoretical framework influences how seriously we take it as an object of interpretation, the RG has interpretive relevance in this sense. By telling us which QFTs have well-defined continuum limits and which are EFTs that are valid up to a finite energy scale, the RG also has interpretive relevance—it tells us the length scales on which we should take the claims of QFTs to be valid.

The key advantage that RG analyses have over perturbative QFT is that one can conduct RG analyses non-perturbatively—through lattice field theory, for example.[14] Thus RG analyses can serve as a way of checking which perturbation expansions are well-defined and which are merely formal. The validity of perturbation expansions also has to do with the locations of QFTs with respect to fixed points. The location and nature of fixed points can be investigated non-perturbatively by the RG and thus serve as an independent check on the validity of perturbation expansions.

4.4.1 Validity of Perturbative Renormalization

As explained above, one of the differences between a perturbatively renormalizable theory and a perturbatively non-renormalizable theory is that in the latter, the cutoff must be kept finite to derive sensible empirical results. In the former, the cutoff can be taken to infinity to derive a "continuum limit". This difference has interpretive significance, since we would want to interpret non-renormalizable theories as effective only up to a finite energy scale, while renormalizable theories may be considered to be potentially applicable to arbitrarily high energy scales.[15] However, it is the RG that underwrites the validity of a perturbative expansion. Perturbative renormalization is typically valid only near a fixed point, so a perturbative expansion that is not done near a known fixed point may turn out to be invalid. In addition, if we conduct a perturbative analysis around the wrong fixed point, a renormalizable theory can appear to be non-renormalizable, and this would affect our interpretation of the theory.

Perturbative expansions are valid only near a fixed point because the linear stability analysis described in Section 4.3.3 that proves universality is valid only near the fixed point.

[14] Lattice field theory involves assigning quantum fields to a lattice of spacetime points, instead of a continuum of spacetime points. Using a lattice instead of a continuum means that the divergences of the continuum theory no longer appear. Thus, perturbative renormalization is not required in lattice field theory.

[15] They may not *in fact* be applicable in the actual world if said world deviates from Minkowski spacetime structure at small enough length scales, but they are at least candidates for being applicable at those length scales.

Far away from a fixed point, the assumption of linearity is no longer reliable. Furthermore, perturbation expansions are valid only when the perturbation is small. In the case of QFT, they are valid only when the coupling constants, which measure the strength of interactions, are small. This is the case only when the system is near a Gaussian fixed point, since the coupling constants are zero at a Gaussian fixed point.

One way in which a perturbative analysis can go wrong is when it is carried out around the "wrong" fixed point. It is possible that an RG trajectory emanating from a non-trivial fixed point passes close to a Gaussian fixed point (Figure 4). If we then conduct a perturbative analysis of the theory around the Gaussian fixed point, we might conclude that the theory is non-renormalizable because it does not lie on a trajectory emanating from the Gaussian fixed point. This would be an erroneous conclusion if the theory does indeed lie on an RG trajectory that emanates from a non-trivial fixed point that our perturbative analysis does not have access to (Rosten, 2012).

4.4.2 Nature of Fixed Point and Triviality

The nature of the fixed point about which the perturbation expansion is carried out also answers questions about the "triviality" of a theory. Both the ϕ^4 theory and QED are thought to be trivial—that is, non-interacting—in the continuum limit.[16] This means that as we take the cutoff to infinity, their coupling constants are thought to vanish, forming a theory with no interactions. A perturbative analysis *suggests* that this is the case because of the appearance of divergences known as "renormalons" or Landau poles in the continuum limit. But we can get more reliable information from a non-perturbative RG analysis. According to the RG, triviality occurs when the perturbation expansion occurs near a "Gaussian" fixed point for which the only relevant directions[17] are non-interacting. This is depicted by the left trajectory in Figure 5. The question of whether QED and ϕ^4 are trivial then comes down to the nature of the fixed points in the vicinity of those theories. We know that the

[16] This is also called an ultraviolet (UV) limit.

[17] Recall that the space of coupling parameters has axes corresponding to the various coupling parameters. Near a fixed point, consider the coupling parameters that are associated with relevant operators. Call these a_r. The relevant directions near the fixed point are those that are parallel to the axes corresponding to the a_r.

Gaussian fixed points for QED and ϕ^4 have no interacting relevant directions. In addition, there is some evidence from non-perturbative RG analyses that for QED and ϕ^4, there are no non-Gaussian fixed points in the neighborhood (Gies & Jaeckel, 2004; Rosten, 2009). This suggests that QED and ϕ^4 are either based on an invalid perturbation expansion (if there is no fixed point at all to expand around) or that they are trivial in the continuum limit. In either case, it would then be more appropriate to interpret them as effective field theories that are valid up to only finite energy scales. We would then treat inferences based on perturbative QED and ϕ^4 theory as sound up to that scale, and unsound beyond that.

4.5 THE RENORMALIZATION GROUP AS PROVIDING RELIABLE INTERPRETATION

The account given above of how the RG deployed in the EFT framework relates to perturbatively renormalized QFT indicates a few ways in which the use of the RG provides us with reliable interpretations of QFT. By *reliable interpretations*, I mean interpretations that are insensitive to details about high-energy phenomena that we cannot experimentally verify. This insensitivity allows us to make inferences about what happens at low energies without said inferences being vulnerable to possibly mistaken assumptions about what happens at high energies. It also includes the phenomenon I described above as universality: the fact that multiple theories may differ on what they say about high energy phenomena but agree on the low energy phenomena that we can observe.

4.5.1 Reliability of Interpretations: the Form of High-Energy Dynamics

There is a tendency in the philosophy of physics literature to regard effective field theories as merely "phenomenological" and non-indicative of the true nature of reality. Some people have claimed that fundamental theories are those that apply to all length scales. In a similar vein, theories in which the continuum limit is ill-defined have been said to "not exist" as QFTs (Bouatta & Butterfield, 2012).

I argue in this section that for the purposes of discerning what the nature of the world is, theories that apply to arbitrarily high energy scales are actually *more unreliable* than effective field theories. Thus, EFTs serve as more reliable fodder for interpretation than theories that are commonly regarded as "more fundamental" than EFTs.

The phenomenon of universality mentioned above is key to why EFTs are more reliable. We saw that the low energy dynamics which are experimentally accessible to us are a form of universal behaviour that is manifested by multiple theories which differ in their high energy dynamics. As long as our experiments can access only a limited range of energy scales, we cannot expect to determine through our experiments which of those multiple theories correctly describes the high energy phenomena of the world. Universality implies that any theory that correctly describes low energy dynamics but *specifies* the high energy dynamics will not be able to have the latter confirmed by humanly achievable experiments. In other words, we cannot take seriously any interpretations that make claims about the specific form of the high energy dynamics.

EFTs, by including from the start an energy scale beyond which they admit to losing applicability, avoid the probable mistake of specifying the wrong high energy dynamics. But theories that apply to all length scales necessarily must specify the form of their high energy dynamics, and we would have no way of confirming that specific form. In this way, interpretations of such theories are unreliable, relying on a speculative hope that the specified high energy dynamics are the correct ones even if we have no way of confirming that. In contrast, interpretations of EFTs are more reliable since EFTs do not incorporate such speculative aspects.

One might object that even if a theory that applies to all length scales must be speculating about its high energy dynamics, that we should still prefer this to an EFT because the former is more ambitious and more intellectually exciting. After all, the former kind of theory also contains information about low-energy phenomena that we can use just as well as we use EFTs to tell us about low-energy phenomena. However, in the following subsection we shall see that the former kind of theory carries with it inferential risks even about low-energy phenomena.

4.5.2 The Lesson of Haag's Theorem

Apart from the speculative aspect of guessing the form of unconfirmable high energy dynamics and probably getting that form wrong, there is also the danger that such guesses may, if taken seriously, lead to further mistaken inferences that affect some very general conclusions that are germane to even low-energy phenomena. I argue here that Haag's theorem is an example of such a mistaken inference.

As we saw above, a successful application of the RG to QFT shows that the low-energy dynamics of that QFT are insensitive to the details of high-energy phenomena. In contrast, in axiomatic QFT without the RG, assumptions about high-energy phenomena can drastically affect inferences concerning the low-energy dynamics. The much-discussed Haag's theorem is an example of this sensitivity to assumptions about high-energy phenomena. Haag's theorem states that the interaction picture of QFT, which is the basis for perturbative QFT, cannot exist except in the case of free fields. In other words, it is inconsistent to use the interaction picture to treat interacting fields. The theorem makes a statement that is clearly relevant to both high-energy and low-energy phenomena, since it implies that a certain theoretical approach to both kinds of phenomena is inconsistent.

The trick to avoiding the consequences of Haag's theorem is to discard the assumption that the axioms are true on all length scales. Haag's theorem is derived on the basis of axioms that make assumptions about what happens at arbitrarily small length scales. For example, one of the Wightman axioms states that Poincare covariance holds down to arbitrarily small length scales (Streater & Wightman, 1964, p. 99). This axiom is used in the derivation of Haag's theorem but is violated in conventional QFT in the intermediate steps of its calculations. Specifically, in the process of perturbative renormalization described in Section 4.2.2, regularization means that strict Poincare covariance no longer holds down to arbitrarily small length scales. This violation of strict Poincare covariance at very small length scales is most intuitively seen in the cases of cutoff methods of regularization and lattice regularization. A Lorentz transformation typically alters lengths. Introducing a "basic length" such as a lattice spacing or a cutoff that demarcates two very different regions of physics with an absolute length thus violates Poincare covariance. Perturbative QFT works by operating with a regularized QFT when using the interaction picture, deriving the Green's functions in this way, then obtaining Poincare-covariant quantities at the end of the calculational process by taking the cutoff or lattice spacing to zero in the expressions for the Green's functions. Thus, perturbative QFT can produce Poincare-covariant observable quantities that cohere with our experiments, while at the same time using methods like regularization and renormalization to insulate its conclusions from unreliable assumptions about phenomena at arbitrarily small length scales.

Haag's theorem is an example of how taking an axiom about what happens at arbitrarily high energy scales too seriously can lead to a general conclusion that undermines our ability to make predictions about phenomena at low energy scales. In other words, committing to an unreliable assumption can mean that even the "more correct" parts of the theoretical framework do not get a chance to show how they are correct—the lack of reliability is in a sense *contagious*.

In contrast, with the RG, we have seen that the dynamics of an EFT are robust against the details of high-energy phenomena. Multiple different possible high-energy theories all, according to the RG equations, evolve onto the same hypersurface at low energies, so that we know that our low-energy theory is constrained to be on that hypersurface. This offers us a reliable interpretation of QFT, because we know that whatever probably mistaken assumptions we may make about what happens at high energies, our interpretation of low-energy structures is still for the most part correct. As I've explained above, this kind of reliability is lacking from the theoretical framework that leads to Haag's theorem—in that framework, the axioms contain experimentally unconfirmable assumptions about the high-energy behaviour of the theory, and these assumptions lead to unreliable statements about the non-existence of models that seem to undermine our inferences about low-energy phenomena as well.

4.6 A MORE RIGOROUS RENORMALIZATION GROUP

We saw in the Section 4.4 how the RG as expressed in conventional QFT sheds light on whether specific Lagrangian QFTs have continuum (UV) limits. However, some philosophers may regard the material presented there as irrelevant because it is not based on sufficiently rigorous mathematics. Thus, I will devote a section to sketching the rigorous renormalization group as used in constructive field theory. Constructive field theory distinguishes itself from other means of finding a UV limit by its greater rigor. This rigor consists in:

1. Making sure that the relevant functional integrals are well-defined;
2. In computing the functional integrals, making sure that the approximations and expansions used are well-controlled.

I illustrate point 1 in Section 4.6.1 and point 2 in Section 4.6.2.

4.6.1 Functional Integrals in Constructive Field Theory

I now sketch the constructive field theory approach to defining functional integrals. For simplicity, I consider the ϕ^4 theory (with dimension unspecified for now). Constructive field theorists like to operate with Euclidean functional integrals because this allows them to use apparatus from the theory of Gaussian integrals. Much of the work in defining (4.1) draws from this probability theory basis. In Euclidean field theory, we can regard the real-valued fields $\phi(x)$ as random variables on the d-dimensional Euclidean space $\mathbb{R}^d$. These random variables are associated with a Gaussian measure that is perturbed by an interaction term. The Gaussian measure is associated with the properties of free particles, and the interaction term with interactions between particles.

The Gaussian random field $\phi(x)$ has a mean given by $\int \phi(x) d\mu_C(\phi) = 0$ and a covariance given by $\int \phi(x)\phi(y) d\mu_C(\phi) = (-\Delta + m^2)^{-1}(x, y) \equiv C(x, y)$. We can formally write $C(x, y) = \int_{\mathbb{R}^d} \frac{e^{ip(x-y)}}{p^2+m^2} dp$, which will help us understand ultraviolet regularization later. The Schwinger functions $\langle F(\phi) \rangle$ can be formally written as

$$\langle F(\phi) \rangle = \frac{1}{Z} \int F(\phi) e^{-V(\phi)} d\mu_C(\phi), \tag{4.6}$$

where $Z = \int e^{-V(\phi)} d\mu_C(\phi)$. In the case of ϕ^4 theory, $V(\phi) = \lambda \int_{\mathbb{R}^d} \phi(x)^4 dx$, where λ is a coupling parameter.

The first task of constructive field theory is to modify the above expression for $\langle F(\phi)\rangle$ so that it is well-defined. The measure $d\mu_C(\phi)$ is generally not well-defined before the following steps: ultraviolet regularization, infrared regularization, and, in four dimensions, the addition of counterterms.[18] Ultraviolet regularization is required to ensure that the product of distributions $\phi(x)^4$ is well-defined. This is usually done through a momentum cutoff or lattice regularization. For brevity's sake, I outline only the momentum cutoff method. The momentum cutoff is imposed by altering $C(x,y)$ to $C_\epsilon(x,y) = \int_{\mathbb{R}^d} \frac{e^{ip(x-y)}}{p^2+m^2} e^{-\epsilon|p|^2} dp$, $\epsilon > 0$. Infrared regularization imposes a finite volume Λ over which the integral for $V(\phi)$ is to be carried out. So $V(\phi)$ becomes $V_\Lambda(\phi) = \lambda \int_\Lambda \phi(x)^4 dx$. Finally, if $d = 4$, we have to add a counterterm $\delta V_{\Lambda,\epsilon}$ to V_Λ, so we have $V_{\Lambda,\epsilon} = V_\Lambda + \delta V_{\Lambda,\epsilon}$ in the exponent instead.[19]

The upshot of all this is that the formal expression (4.6) is turned into a well-defined expression:

$$\langle F(\phi)\rangle_{\Lambda,\epsilon} = \frac{1}{Z_{\Lambda,\epsilon}} \int F(\phi) e^{-V_{\Lambda,\epsilon}(\phi)} d\mu_{C_\epsilon}(\phi). \tag{4.7}$$

The task of constructive field theory is to show that this expression has a well-defined limit as $\epsilon \to 0$ and $\Lambda \to \infty$. If this limit exists, then the model being investigated has a UV limit. Multiscale methods allow one to evaluate the integral by decomposing it into momentum scale-indexed parts. This decomposition allows for each scale-indexed part to be evaluated using certain kinds of expansions, without running into problems with the expansions failing when they try to cover too large a momentum range.

4.6.2 Applying the Renormalization Group in Constructive Field Theory

In Section 4.3 we saw a sketch of the physical ideas behind the RG. Constructive field theorists implement the same ideas using more rigorous mathematics. As with more mathematically cavalier implementations of the RG, the existence of a UV limit in constructive field theory is linked to the existence of fixed points of RG transformations. However, many RG methods used in conventional QFT fail to account for the large field problem. Some non-perturbative approaches to the RG use non-perturbative approximations that we do not know how to place error bounds on.

[18] In two or three dimensions, the ϕ^4 model is superrenormalizable and no counterterms are needed.

[19] I leave out the details of the form of $\delta V_{\Lambda,\epsilon}$ for brevity. See Watanabe (2000) for details.

Constructive field theory tries to find the UV limit using approximations that are better controlled than those of conventional QFT. One way to do this is via the exact renormalization group (ERG).[20] The term "exact" in this context indicates that the RG is implemented non-perturbatively and that the approximations involved are well-controlled. Benfatto et al. (1980), Gawędzki and Kupiainen (1983), Gawędzki and Kupiainen (1985), Brydges, Dimock, and Hurd (1995), and Abdesselam (2007) are examples of how the ERG is used in constructive field theory. I now sketch an RG analysis based on integrating out fluctuations over slices of momentum space, showing how one may determine whether a given Lagrangian has a UV limit in this way.[21]

As mentioned in Section 4.3, the basic idea of the RG is to integrate the functional integral over momentum slices. This avoids the failures of various kinds of expansions when one integrates over a large range of momenta in one step. In the constructive field theory framework this integration can take place by dividing the covariance C_ϵ into parts that correspond to momentum slices. Notating C_ϵ as D for convenience, we have

$$D = \sum_{k=0}^{N} D_k,$$

with independent Gaussian variables $\phi_k(x)$ that each have mean 0 and covariance D_k. Each ϕ_k corresponds to a fluctuation field of momentum scale L^k. The slices of measure D_k are defined as follows:

$$D_k(x,y) = \int_{\mathbb{R}^d} \frac{e^{ip(x-y)}}{p^2+m^2}(\chi(L^{-k}) - \chi(L^{-(k-1)}p))dp, \quad k = 1, 2, \ldots, N,$$

$$D_0(x,y) = \int_{\mathbb{R}^d} \frac{e^{ip(x-y)}}{p^2+m^2}\chi(p)dp,$$

where $\chi(p) = e^{-p^2}$ serves as a cutoff function. The D_k serve the purpose of scale decomposition because each D_k effectively isolates the range of momenta between L^{k-1} and L^k.

[20] Note of caution: some who work in the tradition of the functional renormalization group take themselves to be using the "exact" renormalization group, which they take to a term referring to Wilson's non-perturbative understanding of RG flows (Rosten, 2012; Bagnuls & Bervillier, 2001). However, the lack of precise error bounds on their approximations sets them apart from the constructive field theory tradition, as Gurau, Rivasseau, and Sfondrini (2014) point out.

[21] Besides momentum slice integration, another way of implementing the RG in constructive field theory is the block spin transformation, where one treats the quantum field in a lattice setting.

Defining $H(\phi) \equiv H_N(\phi) = e^{-V_{\Lambda,\epsilon}(\phi)}$, $\phi_{k,0} = \sum_{j=0}^{k} \phi_j$, and $D_{k,0} = \sum_{j=0}^{k} D_j$, $k = 0, 1, \ldots, N$, we can define the operation of scaling out higher momenta as follows:

$$H_{k-1}(\phi_{k-1,0}) = \int d\mu_{D_k}(\phi_k) H_k(\phi_k + \phi_{k-1,0}), \quad k = N, N-1, \ldots, 1. \tag{4.8}$$

H_{k-1} is simply the coarse-grained version of H_k, with the higher momenta integrated out. In an RG analysis, we would want to iterate this operation of integrating out higher momenta. Before iterating it, however, we rescale the field $\phi_{k-1,0}$ so that it has a wavelength comparable to ϕ_k's. The rescaled field is defined as $\tilde{\phi}_k(x) = L^{-k(d-2)/2}\phi_k(L^{-k}x)$. We also rescale the covariance D_k, the details of which I omit for brevity.[22] Then we define the rescaled H_k by

$$\tilde{H}_k(\tilde{\phi}_{k,0}) = H_k(\phi_{k,o}).$$

This gives us the RG transformation

$$\tilde{H}_{k-1}(\tilde{\phi}_{k-1,0}) = \int d\mu_{\tilde{D}_k}(\tilde{\phi}_k)\tilde{H}_k(\tilde{\phi}_k(\cdot) + L^{-(d-2)/2}\tilde{\phi}_{k-1,0}(L^{-1}\cdot)).$$

While we have been using the notation $H(\phi) = e^{-V_{\Lambda,\epsilon}(\phi)}$ for convenience, we can think of the RG transformation as acting on the action V. Each transformation consists of the following steps:

1. Rescaling of the fields;
2. Integrating over a momentum slice;
3. Taking the logarithm of $\tilde{H}_{k-1}$ to get the V needed for the next transformation.

The problem of finding a well-defined Lagrangian in the ultraviolet limit then reduces to seeing if V converges in the limit of infinitely many RG transformations: in the limit of $k \to \infty$. The convergence of V in this way corresponds to the existence of the fixed point we are looking for, as explained in Section 4.3.

Constructive field theory differs from other ways of implementing the RG in how well it controls the approximations that are involved. For bosonic interactions, the step of taking the logarithm of $\tilde{H}_{k-1}$ is not well-defined for certain values of ϕ. This is known as the "large field problem." Constructive field theory deals with this by carrying out the transformation

[22] See Watanabe (2000) for details.

only for small fields. The steps of integrating out fluctuations in a momentum slice and taking the logarithm are carried out only for small fields. This means that we can use a cluster expansion for the former step and a Mayer expansion for the latter step. Both these expansions would not be well-controlled in the large field region. There are various methods for controlling the large field region. Because of their complexity, I can only list them here without going into the details: the domination procedure (Feldman, Magnen, Rivasseau, & Sénéor, 1987), polymer systems (Pordt, 1994), and using the fact that "large fields" occur with a relatively small probability (Balaban, Imbrie, & Jaffe, 1984).

4.7 CONCLUSION

I have made the case that the renormalization group is interpretively relevant in QFT. When we pay attention to the RG, we realise that scaling considerations implemented by the RG enable our QFTs to tell us about what the world is like at low energies without requiring us to provide accurate information about what the world is like at arbitrarily high energies. This insensitivity to the details of high-energy phenomena is not yet available in axiomatic treatments of QFT that leave out the renormalization group.[23] Yet it is this insensitivity that assures us that our inferences in conventional QFT are reliable and independent of whatever guesses we may make about what occurs at higher energies. In contrast, axiomatic QFT without the renormalization group does not possess this insensitivity. Haag's theorem is a case where this lack of insensitivity can lead to erroneous inferences about even low-energy phenomena.

In addition, the RG provides an explanatory physical and mathematical picture of why the apparently mathematically unsound manipulations of perturbative renormalization are so empirically successful. Since it is desirable for an interpretation of a theory to explain the successes of scientific practice, this is one reason to take the picture presented by the RG seriously.

The RG is relevant to interpretation in other ways. A renormalization group analysis is

[23] There is some preliminary work, which is still far from any applications, by Buchholz and Verch (1995).

necessary for understanding questions such as whether certain QFTs are trivial in the continuum limit and whether perturbative calculational methods are reliable. This means that the RG is crucial for telling us the range of applicability of various QFTs—which should be considered only effective field theories and which are at least candidates for being continuum theories. All this suggests that when interpreting QFT, calculational methods like the RG can be interpretively relevant—we cannot assume that all the interpretively relevant information is contained in the axioms. Indeed, mathematical physicists have long recognized that axiomatic QFT does not furnish specific dynamical information (Wightman, 1976; Horuzhy, 1990). This information requires investigation of specific Lagrangians, and the RG is a central tool in capturing both microscopic and macroscopic dynamical information for specific Lagrangians, even in the rigorous framework of constructive QFT.

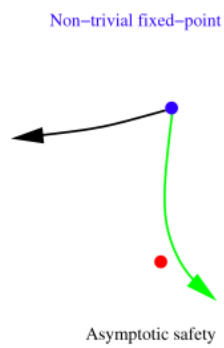

Figure 4: An RG trajectory (green line) coming from a non-trivial fixed point but passing close to a Gaussian fixed point (red dot). Figure taken from Rosten (2012, p. 186).

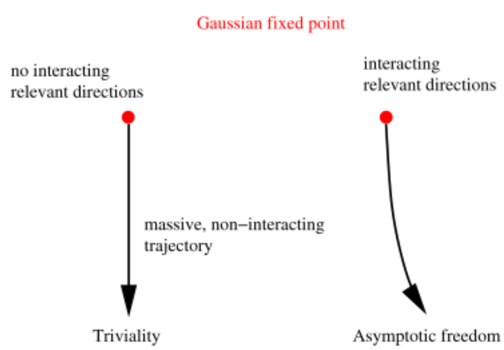

Figure 5: Triviality and asymptotic freedom. Figure taken from Rosten (2012, p. 186).

5.0 HOW APPLICATIONS INFLUENCE MATHEMATICAL RIGOR, SYNTAX AND SEMANTICS

5.1 INTRODUCTION

The last two chapters focused on the development of the renormalization group and the effective field theory picture as explanations of the success of perturbative renormalization, despite the latter's initial mathematical unintelligibility. Physicists came to this new picture of the physics and mathematics in QFT by starting from initially incomprehensible mathematics, applying inferential strategies appropriate to this initial stage, and gradually developing a more mathematically intelligible picture of perturbative renormalization. In this chapter I suggest that what happened in QFT is an instance of a general pattern of development that has occurred several times in the history of mathematics. We should expect this pattern to recur given how syntactic and semantic considerations interact in the development of new mathematics. The result is that we should be more careful about the way we interpret existing mathematical methods. The sanitized approach treats the mathematics of physical theories as a more definite entity than suggested by the developmental dynamics I describe in this chapter. This leads users of the sanitized approach to adopt an overly restrictive view of what the content of physical theories consists in.

Implicit in the sanitized approach is a common view on the relationship between mathematics and physics that has strongly influenced how we evaluate physical theories. Consider a mathematical equation that is part of some physical theory. Philosophers have often acted as though the mathematical content of such an equation is given independently of physical content. That is, they have often acted as though the mathematical import of this equation is one that is clearly given by existing rules of mathematics. For example, since Maxwell's field equations are formulated based on a *mathematical* spacetime continuum, it is supposed that the consequences, including physical consequences, we can draw from them should be only those licensed by the mathematics of field theory on continuous spacetime.[1]

A related view concerns the place of "rigor" in interpreting physical theories. Traditionally, mathematical rigor has been considered to be a feature distinct from the physics that we are modelling with our mathematics. Using a standard of rigor given by mathematics, one of the tasks of the philosopher of physics is to interpret physical theories using the most rigorous variant of a theory that is available. The idea is that there are standards of reasoning in mathematics that are given beforehand, independently of physical and historical context, and in our analysis of physical theories, we should emulate these standards as far as possible. This view imports standards of reasoning in mathematics directly into the context of physics and the philosophy of physics. It assumes that interpreters of physical theories have a fixed set of rules provided by existing mathematics that they ought to use—as far as possible—to draw their conclusions.

I will collectively refer the two views described above as the rigorist view. By this view, physicists' frequent violations of standards of mathematical rigor when applying their theories are purely pragmatic and have no foundational import. Quantum field theory provides notorious examples of how physicists willingly violate the syntactic rules given by mathematics. A typical rigorist response to these violations is to regard physicists' unrigorous reasonings as irrelevant to what the "real mathematics" underlying QFT is (Halvorson & Müger, 2006; Fraser, 2011). There is, on the one hand, whatever the true mathematically rigorous formulation of QFT is, and on the other hand, the tricks that physicists use to get results. The latter are merely pragmatic and do not reflect what the theory really says, which lies in the former.

My aim in this chapter is to take a second look at the mathematical and foundational import of physicists' violations of mathematical rigor. Syntactic rules in QFT were and are violated because the physical context has forced a reinterpretation of the original mathematics, thus licensing a different set of syntactic rules. Where the applications of QFT depart

[1]To the extent that one makes inferences not licensed by the mathematics of field theory on continuous spacetime, these are characterized as not being based on Maxwell's theory. They might be characterized as being based on pragmatic reasons, for example.

from the syntactic rules provided by our prior interpretations of the original mathematics, these departures can be pointers to modifying our naive interpretation of the original mathematics. We may find that the original formalism demands a different kind of mathematics from the one we had started with (Fig. 6). This analysis fits with my claims in Chapter 4 about the significance that renormalization group techniques ultimately had for the interpretation of QFT.

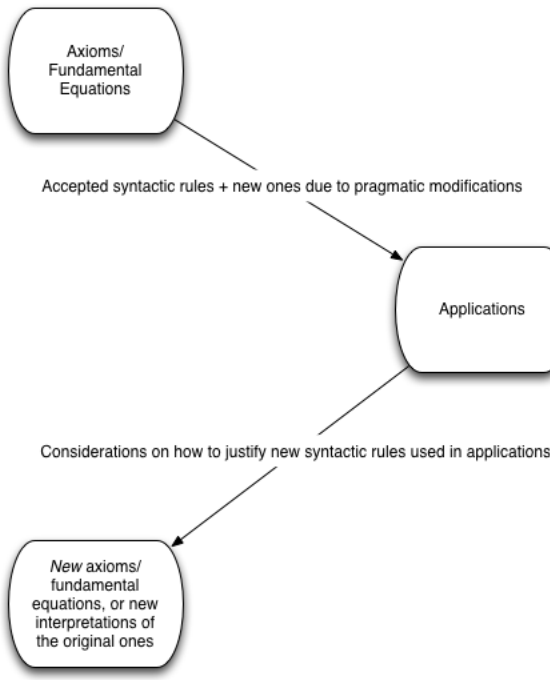

Figure 6: Applications upstream from their "foundations".

This pattern of development, in which the initially "unrigorous" syntax of applications changes our prior interpretations of the mathematics that the applications originated from, is one that is common and to be expected in the development of mathematical physics. I will follow Mark Wilson (2008) in calling a sequence of this type a *canonical pattern of development.* Since this pattern of development is easily found in the history of mathematics, there is nothing special about the case of QFT that necessitates labelling the deviance of applications from priorly given rules as *merely* pragmatic. We will see in Section 5.2 that the same pattern of development has occurred in the history of the operational calculus and in the history of divergent series. The mathematical oddities that underlie the empirical success of QFT are only to be expected given modest expectations of the reach of our language. Rather than assuming that the part of the theory that has already been rigorized is the best candidate for the content of the theory, we should pay more attention to the apparently unrigorous applications, as these are sources for changes in our understanding of the original mathematics, and therefore changes in our understanding of the physics and the mathematics that we need to describe the physics.

5.2 THE CANONICAL PATTERN OF DEVELOPMENT IN THE HISTORY OF MATHEMATICS

The canonical pattern of development proceeds as follows. In Stage 1, there is an extension of syntactic rules that have been proven sound in a certain domain into a domain where it is unclear if they are sound. I take syntactic rules to be rules delineating which manipulations of mathematical formulae are permissible, where the delineation is based on the symbolic form of the formulae only. In Stage 2, one tests how the extended rules work in the new domain by observing the patterns of results of calculations with the extended rules. By "patterns" I include considerations such as whether those results cohere with other methods of calculations based on known rules, whether the results are potentially helpful for other areas of mathematics, whether, in the case where the mathematics is used in certain physical applications, the results agree with physical measurements, and so on. These patterns determine what happens in Stage 3, where the extended rules are justified with an account of what they "mean". Often their "meaning" is given in terms of new mathematical definitions. These new definitions are then used to "clean up" whatever errors in usage occurred in the second stage—inevitably, some applications of the new syntactic rules in the second stage were warranted by the new definitions, while others were not.

In the rest of this section, I will illustrate this pattern using two examples from the history of mathematics: the case of Heaviside's operational calculus and the case of divergent series. Then in Section 3, we will see how the case of QFT resembles these developments in the history of mathematics.

5.2.1 Divergent Series

The canonical pattern of development can be seen in the history of summing infinite series. An infinite series is a sequence of terms $a_0, a_1, a_2, \ldots$, often denoted by (a_n) for brevity. The sum of such an infinite series is typically denoted by $\sum_{n=0}^{\infty} a_n$, where a_n denotes the nth term in the series. Typically each a_n is defined by an algebraic formula that may depend on n, such as $\frac{1}{n}$ or 2^{-n}. First-year undergraduates are typically taught that divergent series have no sum and that convergent series are the only kind of infinite series that have a sum. Convergent series are those whose partial sums, $\sum_{n=0}^{N} a_n = s_N$ (N finite), approach arbitrarily close to a fixed limit as one considers more and more terms in the series. This limit is then defined as the sum of the convergent series. More specifically, for a convergent series there exists a number s such that one can always find an N that makes $\sum_{n=0}^{N} a_n \to s$ as $N \to \infty$. s is then defined as the sum of the convergent series. I will refer to this kind of sum as the *conventional sum* of a series. Sometimes, series can be written such that their terms depend not only on the index n but also on a variable x. The series $1 + x + x^2 + \ldots$ is an example. In these cases, we can talk about the *domain of convergence* of a series: the range of values of x for which the series converges.

Divergent series are those that do not have a conventional sum. Divergent series include those with terms that oscillate around 0 without the oscillations getting smaller, as in $1-1+1-1+\ldots$, or those that grow indefinitely in their absolute values, such as $1-2+4-8+16+\ldots$, or even some whose terms grow slower with increasing N but not slowly enough to converge, such as $1+\frac{1}{2}+\frac{1}{3}+\ldots$. Although divergent series were used liberally by luminaries like Euler in the 18th century, they lost favour among mathematicians working in analysis in the 19th century, largely due to the influence of Cauchy (Laugwitz, 1989). Cauchy argued, contra Euler and other predecessors, that divergent series had no sum. However, in the late 19th century divergent series gained favour again and various accounts of how to define the sum of divergent series began to proliferate.

The history of summing divergent series illustrates the canonical pattern of development as follows. At first, possible ways of summing divergent series were suggested by way of extending the syntax relating to the sums of finite series. For example, the associativity of addition and subtraction was used to rearrange infinite series to put them in forms that suggested finite sums:

$$1 - 1 + 1 - 1 + \ldots = 1 - (1 - 1 + 1 - 1 + \ldots)$$

$$s = 1 - s$$

$$s = 1/2,$$

where I have taken $s = 1 - 1 + 1 - 1 + \ldots$. By what seems to be a straightforward algebraic manipulation, we find that the series sums to $1/2$.

However, not all plausible ways of extending the syntax of summation from sums with finitely many terms to sums with infinitely many terms give us $s = 1/2$. We could also arrange the above infinite sum[2] as follows:

$$(1 - 1) + (1 - 1) + (1 - 1) + \ldots = 0 + 0 + 0 + \ldots = 0 \tag{5.1}$$

Here we have again extended the syntactic rule of associativity to infinite sums, only to find that it gives us a different answer from the previous extension of the rule of associativity. How should we choose between these different possible extensions?[3]

In the second stage of the canonical pattern of development, the different methods of summation are tested in various domains, on various kinds of series. Mathematicians take note of when these methods agree or fail to agree with one another, when they give results that make or fail to make physical or geometrical sense, when they are fruitful for other areas of mathematics, and so on.

We have seen how different methods gave different sums for the same infinite series for the case of $1 - 1 + 1 - 1 + \ldots$. Importantly, many plausible methods gave $\frac{1}{2}$ as the sum of the series, though there some that gave other results, such as 0 (as mentioned above) and $\frac{2}{3}$ (Ferraro, 2008, 312). It was suspected that the correct sum was $\frac{1}{2}$ because, for example, important results in the theory of Fourier series depended on the sum being $\frac{1}{2}$ (Hardy, 1949,

[2] From now on, I will refer to a sum with infinitely many terms as an "infinite sum", without meaning to imply that the numerical value of that sum is infinity.

[3] I have highlighted only two very simple cases of extending syntax to find a sum for this series, but there are many others, including finding the mean of arithmetic means, geometrical methods, multiplying the infinite series of interest with another infinite series that sums to one, and so on.

31). Appendix A details how these disagreements were resolved in favour of $\frac{1}{2}$ with new and more general definitions of sums—Stage 3 of the canonical pattern of development.

Many different Stage 2-type considerations led to extensions of the concept of a sum of a series. An important Stage 2-type consideration was ensuring that the new method for summing divergent series gave the same results as the conventional sum when applied to convergent series. Another consideration in favour of new methods was agreement with the results of series expansion methods, such as Euler's $1 + x + x^2 + ... = \frac{1}{1-x}$. Yet another consideration was the ability to sum important series, such as Fourier series and Dirichlet series. In a similar vein, methods for summing divergent series were also favoured if they preserved some convenient properties relating to the addition, subtraction, or multiplication of different series, or if they allowed for certain important theorems to be extended to cover a wider domain. Finally, the usefulness of a summation method for other areas of mathematics, such as the theory of analytic functions, was also a Stage 2-type consideration.[4]

The considerations described above have been synthesized into what is often called the "theory of divergent series". The idea behind such a theory is to make generalizations concerning the various summation methods, including finding the "most general" summation method possible, one which can reproduce as many "non-pathological" sums as possible.[5] This constitutes Stage 3 in the canonical pattern of development. The new methods of summation defined in the theory explain the successes within a certain domain of the syntactic extensions in Stage 2, and also why those extensions were unsuccessful in other domains.

An important feature of the more general methods of summation that make up Stage 3 is that they offer a unified account of why the previous *ad hoc* summation methods were *correct* in some domain. A correctness proof for a new summation method would show that under the new method, the more primitive methods of summation, such as those that Euler used, would assign series their appropriate sum, where the latter is defined by the new method. Wilson (2008) has compared these correctness proofs to soundness proofs in logic: in both cases, we want to show that as long as we start with "premises" of a certain kind

[4] Readers who would like more details on how the specific summation methods and how these considerations applied to them should refer to Appendix A.

[5] The rider "non-pathological" is meant to rule out sums that were rejected as "incorrect" for various reasons, such as the sum $1 - 1 + 1 - \ldots = 0$ mentioned above.

and follow certain rules, we will obtain conclusions of a certain kind. I will suggest later that in QFT the renormalization group provides a similar proof of the correctness of perturbative renormalization.

5.2.2 The Operational Calculus

The operational calculus is a way of treating operations in mathematics as algebraic symbols. An operation, technically, is well-defined only when it is being applied to an object. Symbolically, the symbol denoting an operation ought to always be immediately to the left to the symbol denoting the object the operation is operating on. The operational calculus was a development in mathematics that algebraized operational symbols by letting operational symbols be manipulated independently, without necessarily having them always be explicitly denoted as acting on objects.

For example, consider the partial differential equation (PDE)

$$\frac{\partial^2 u}{\partial x^2} = \frac{\partial u}{\partial t}. \tag{5.2}$$

The first step of the operational calculus is to rewrite the right hand side of this equation as $D_t u$. By replacing the "$\frac{\partial}{\partial t}$" notation with a single algebraic symbol D_t, we rewrie the PDE as $\frac{\partial^2 u}{\partial x^2} = D_t u$ and make it resemble the familiar ordinary differential equation (ODE)

$$\frac{d^2 u}{dx^2} = pu. \tag{5.3}$$

In the familiar notion of an ODE, p denotes a number, not an operator like D_t. The general solution to such an ODE is

$$u = Ae^{-x\sqrt{p}} + Be^{x\sqrt{p}}, \tag{5.4}$$

where A and B are arbitrary constants.

Since D_t does not denote a number, so we cannot use the ODE solution (5.4) as a direct solution to the PDE $\frac{\partial^2 u}{\partial x^2} = D_t u$. However, by a syntactic analogy with the ODE solution, we can write down the "analogous" solution for the PDE, which I will call the *operational solution*:

$$u = e^{-x\sqrt{p}}\phi\left(t\right) + e^{x\sqrt{p}}\psi\left(t\right), \tag{5.5}$$

where instead of A and B we have the functions $\phi(t)$ and $\psi(t)$, and p denotes D_t, not a number. The syntactic analogy lies in the fact that it is tempting to apply the syntactic rules that hold for a *number* to an *operator* D_t.

At this point, because p is no longer a number, $e^{-x\sqrt{p}}$ and $e^{x\sqrt{p}}$ have yet to be defined. Heaviside usually only considers functions $\phi(t)$ and $\psi(t)$ that are either sinusoidal or step functions.[6] This restriction allowed him to formulate some rules for how the two operators would act on those functions. I do not have the room here to go into the details, but essentially, $e^{-x\sqrt{p}}$ and $e^{x\sqrt{p}}$ are expanded into "Taylor series" of the operator $\sqrt{p}$, and then various independently discovered rules for how powers of $\sqrt{p}$ act on sinusoidal and step functions are applied in order to obtain an explicit expression for u purely in terms of t, with the ps eliminated (Lützen, 1979). This expression $u(t)$ is what I will call the *ordinary solution* to (5.2)—it is the solution that one would also derive through ordinary methods of solving the PDE, which do not resort to the operational calculus. In the rest of this chapter, I will use the term *ordinary solution* to refer to a solution to a PDE or ODE which solves the PDE or ODE in the ordinary sense, i.e. independently of the operational calculus. In contrast, *operational solutions* like (5.5) do not solve the PDE or ODE in the ordinary sense, as they contain the uninterpreted expressions $e^{-x\sqrt{p}}$ and $e^{x\sqrt{p}}$.

By showing how one can turn certain PDEs into ODEs, which are usually easier to solve, the operational calculus offered more powerful methods for solving PDEs. This gain in power depended on an initial syntactic extension of algebraic rules into the realm of differential operators. As such, it illustrates the first stage in the canonical pattern of development described above. The syntactic similarity between the symbolization of operators and the usual algebraic representation of numerical variables made this possible. Next, we will see how other contributors to the operational calculus used the results of calculations from the extended syntactic rules as a guide to a better justification of the extended syntactic rules.

The second stage in the canonical pattern of development involves finding out the exact conditions for the success of the new syntactic rules. In this way, one can obtain clues for the work of Stage 3—clues to the kinds of new mathematics that will explain the success

[6] A step function is defined as having the value 0 for all arguments less than 0, and having the value 1 for all other arguments.

of the rules. In our present case, Stage 2 involves finding out exactly when it is legitimate to algebraize the differential operator to solve PDEs in the way described above. Often, there are alternative ways to "confirm" whether a solution obtained by the algebraizing method is correct. For example, one can obtain the solution using alternative methods and compare that solution with the solution obtained by the algebraizing method.[7] Another way of determining the conditions of success of the rules is to find a common reason behind the successful applications of the rules. This "reason" may take the form of a particular mathematical formula that, if we take to be true, will allow us to obtain the results of the operational calculus without relying solely on algebraic reasoning. Or, one may try to find conditions that justify parts of algebraic reasoning such as the commutativity of operators. Appendix B details the particulars of these Stage 2-type considerations, and explains how they led to different Stage 3-type new mathematical accounts of the operational calculus.

There are many possible Stage 3-type interpretations of the operational calculus. Integral transforms and algebraic approaches are among the most prominent examples.[8] These interpretations are not equivalent, each having its own advantages or disadvantages. About the algebraic method, Florin (1934) complains that manipulations with fractional powers of p must be defined in an *ad hoc* manner. He notes however that the algebraic method is simpler and more transparent than those based on integral transforms. The latter, it turns out, deal with fractional powers of p in a natural manner. But they have the disadvantage of requiring that the integrals that they use must be evaluated. This makes them unsuitable for "translating" mathematical expressions that contain both ps and xs.

Just as with correctness proofs in the theory of divergent series, different interpretations of the operational calculus in Stage 3 offer different proofs that some range of the syntactic extensions of Stage 2 would lead to correct results, where correctness is adjudicated according to the manipulations allowed by the interpretations of Stage 3. In this way, these interpretations justify Heaviside's operational calculus. Heavside's moves by themselves are not justified in the same way because he had no way of theoretically reasoning, prior to observing the outcomes of calculations, that a certain method of calculation, made under

[7]This kind of comparison between different theoretical methods has been described in more detail, in the context of early quantum field theory, in Chapter3.

[8]See Appendix B for details on these approaches.

certain conditions, will lead to correct results. His only method of justification was to see if the calculations turned out to fit the world or to fit the results from other methods. The algebraic and integral transform interpretations of the operational calculus, on the other hand, allowed them to prove correctness theoretically, prior to observing the outcomes of calculations. For example, under the integral transform picture, one can show that under a restricted range of conditions, the steps of reinterpreting a PDE as containing implicit integral transforms, integrating by parts, and solving the corresponding ODE will generate a solution that is an ordinary solution of the original PDE. In the next section, we will see how a similar correctness proof holds when we apply the picture provided by the renormalization group to QFT.

5.3 THE CANONICAL PATTERN OF DEVELOPMENT IN THE HISTORY OF QFT

I now move on to demonstrate how the canonical pattern of development has occurred in the history of QFT. By showing the similarities of the case of QFT with those of the canonical pattern of development we've seen in the cases of the operational calculus and divergent series, we can more easily see how apparently unrigorous, syntactically extended calculational methods in QFT pointed the way towards a new interpretation of the mathematics of QFT. Just as the meaning of the sum of a divergent series would have been prematurely constrained if we had stuck strictly to the rules given by Cauchy's mathematics instead of paying attention to certain important rule-violating calculations, the current interpretation of renormalization in QFT would have been prematurely constrained if we had insisted on ignoring the foundational significance of the apparently mathematically unlicensed procedures that physicists used.

5.3.1 Syntactic Extensions in QFT

There are two extensions of the syntactic rules of mathematics in QFT that concern us. The first is the use of perturbation theory in QFT. Perturbation theory is a mathematical technique in which the solution of the equations of motion of an interacting system is derived on the basis of a known solution of the equations of *free* motion. One assumes that the interaction component is "small" and tries to derive the solution for an interacting system as a "small modification" of the solution for a free system. Even though, if one uses perturbation theory in QFT, one can derive perturbation series in powers of the strength of the interaction, one has no principled reason to believe that this application of perturbation theory is correct. Generally speaking, these expansions only serve as solutions to the original differential equations for certain kinds of perturbations. To prove correctness[9] in the case of the perturbations used in QFT, one has to show that the solutions we get via perturbation theory are indeed solutions to the equations of motion of QFT.

Another syntactic extension occurs in a calculational method known as perturbative renormalization. This method is key to the empirical success of perturbation theory. For the purposes of this chapter, we do not need a detailed account of how it works. The main point is that historically, perturbative renormalization came about by a syntactic extension of the fact that when we have an equation $X = Y$, where X and Y are finite numbers, then $X - c = Y - c$ provided that c is finite. Perturbative renormalization first happened when physicists decided to try doing this even when c is infinite. Of course, not *any* subtraction of infinities led to useful results. Rather, there were some implicit indicators for when and where to subtract infinities. In Chapter 3, I provided some details on how they determined that the c on each side denotes the same physical quantity, and how they had reasons to suspect that c could be treated as a regular finite quantity. The reasoning strategies I described in that chapter embody Stage 2 of the canonical pattern of development—the experimentation with calculations based on syntactic extensions to determine where the extensions succeed and where they fail. Physicists detected certain patterns in these failures and successes that gave them clues as to what was responsible for the failures. Eventually, they diagnosed the

[9] In the sense described at the end of Section 5.2.1.

problem as an overly optimistic view of the range of applicability of QFT.

This diagnosis led to the ideas of effective field theory (EFT) and the renormalization group (RG). The basic idea of effective field theory is that QFT is applicable to only a limited range of length and energy scales. The RG is a way of justifying the formerly dodgy perturbative renormalization procedure within the physical picture presented by the EFT. We will next see how the advent of EFT and the RG encompass Stage 3 of the canonical pattern of development: how they provided us with a new physical and mathematical picture that justified the syntactic extensions we have discussed.

5.3.2 Reinterpretation of Renormalization and Field Equations

As discussed before, in the early stages of QFT, we had no theoretical proof that the processes of applying perturbation theory and of perturbative renormalization would lead to correct results. Even though they provided empirically successful results, there was no understanding of why they did so. However, we now know that if we take QFT to be applicable to only a limited range of length and energy scales, then we can show perturbative renormalization to be a correct reasoning process in many cases. We can also show in the EFT picture that the apparent need to subtract infinities is merely a consequence of an incorrect starting assumption that the strengths of interaction in QFT systems are independent of length scale. In the EFT picture, these interaction strengths (represented by the coupling parameters) vary with length scales and they do not indicate how the interaction proceeds at *all* length scales but only in a limited range of them.

With the help of the RG, we can show that the empirical success of perturbative renormalization can be understood by considering our empirical observations to be macroscopic measurements of a more complicated microscopic picture. The RG gives us a proof that the infinitely many coupling parameters that exist on a microscopic scale give rise to effects that can be ignored when we make macroscopic measurements. In addition, because one can apply RG methods *non-perturbatively*, we do not have to show that perturbation theory is correct before being able to use the RG. In fact, the RG in concert with the EFT picture offers a correctness proof for perturbative renormalization within perturbation theory: if we accept the EFT picture, then we can use the RG to prove that perturbative renormalization is simply a method of rescaling the coupling parameters in our calculations so as to derive the correct macroscopic results.

Thus, we can see the EFT and RG as justifying previously unjustified calculations that were based on syntactic extensions like perturbative renormalization. They justify these calculations by reinterpreting previous mathematical derivations within a new picture of the world in which coupling constants are not constant but vary with scale, and QFT applies to only a limited range of length scales. However, this justificatory stage of the canonical pattern of development, Stage 3, is possible only because of the previous stages in which mathematically unjustified syntactic extensions were tried and the patterns of failures and successes they led to noted as data that Stage 3 has to account for.

Importantly, Stage 3 has not been fully carried out yet in QFT. Many calculations in QFT still rely on uses of perturbation theory that have not been verified to be non-perturbatively sound. The claim that quantum chromodynamics has a continuum limit, for example, has so far been based on perturbative calculations, and it remains to be seen if non-perturbative calculations will confirm this. Nonetheless, the advent of EFT and the RG have at the very least started Stage 3, opening up more possibilities for showing how perturbation theory as used by physicists is sound.

5.3.3 Overview of the Canonical Pattern of Development in QFT

The story I have recounted is relevant to the rigorist view in the following way. When QFT was first formulated by extending the quantization process from non-relativistic quantum mechanics to field theory, the field equations we derived from such a syntactic extension *prima facie* described quantum fields on continuous spacetime. That is to say, the superficial syntax of the field equations suggested that these fields were defined at every point in spacetime. Furthermore, the same extension of syntax gave us Lagrangians in which the dynamics of the fields were scale-independent.

When we actually tried to get sensible numbers out of this syntactically extended theory, we found that we had to resort to mathematically odd manipulations such as subtracting infinities from both sides of an equation. These manipulations lay outside the set of mathematical inferences licensed by the original conception of a field theory in continuous spacetime—the syntactical machine of rigorous mathematics as we knew it could not produce these inferences. At this point, one might be tempted to say that physicists used these unjustified manipulations for only pragmatic reasons, so we should not take these manipulations to have any foundational significance for the theory. In other words, one might be tempted to view Stage 2 of the canonical pattern of development as consisting in trial-and-error merely for the purpose of empirical prediction.[10]

However, Stage 3 of the canonical pattern of development shows how Stage 2 does have foundational significance for the theory. In Stage 3, we discover that we can justify, both mathematically and physically, the apparently *ad hoc* procedures of Stage 2 provided that we accept a certain physico-mathematical picture of the meaning of QFT—that based on EFT and the RG. This picture is now the one accepted by most practising quantum field theorists. Thus, even though perturbative renormalization is not a rigorous procedure according to physicists' initial interpretation of the mathematics of QFT, it can be one, or at least a shorthand for one, according to their new interpretation. This outcome shows that it is unwise to assume that the mathematical content of a theory is given prior to the applications of the theory. Sometimes, the content of a theory is not fully contained in its fundamental equations because the way in which we originally formulated and understood the fundamental equations may turn out to be inadequate to the applications we expect to put the theory to. In this case, a physico-mathematical revision and reinterpretation of mathematics is called for, and the apparently mathematically unlicensed moves made in the applications can turn out to be key to this reinterpretation.

[10]This is in effect what many philosophers of QFT, like Fraser (2011) and Halvorson and Müger (2006), have done. It is part of their justification for paying more attention to the variant of QFT known as algebraic QFT, even though algebraic QFT is barely used by physicists and has had very little empirical success.

5.4 CONCLUSION

Examples from the history of mathematics suggest that it is not at all uncommon for apparently unrigorous mathematical manipulations to change our understanding of which mathematical rules are acceptable. This change generally occurs in order to make sense of the success or fruitfulness of said apparently unrigorous manipulations. With this change comes a change in our understanding of the meaning of the mathematics. This includes changes in what syntactic rules we take to be acceptable, and in how and whether certain mathematical manipulations are justified.

I argued that we can see the same pattern of development in the history of QFT. Originally, it seemed as though perturbative renormalization made no mathematical or physical sense. However, the RG and the EFT have explained why it is such an empirically successful procedure, and we got to them by trying to make sense of perturbative renormalization. A rigorist view, like that of many philosophers of QFT, would have prematurely stunted progress in understanding QFT with its insistence that physicists' unrigorous calculations are irrelevant to the theoretical content of QFT.

Applying this lesson to contemporary problems in QFT, it is indeed the case that those working in the rigorous tradition of constructive field theory, where they try to construct solutions to particular Lagrangian QFTs, are aware of the importance of paying attention to the unrigorous techniques often used in applications of QFT. The construction of rigorous solutions to QFT models requires using information about what works in perturbative renormalization. In general, constructive field theorists spend a significant amount of effort figuring out what perturbative calculations in QFT could possibly mean and how they could possibly be rigorized, as this is key to the success of their project.

All this supports my main claim in this chapter, that the rigorist view imposes an overly rigid view of the content of a physical theory. It does this in part because it enforces an overly rigid view of the content of mathematics in general. By restricting its vision to mathematics that is already in a rigorous framework, it fails to consider the possible significance of less well-developed mathematical methods. General considerations about how syntax and semantics interact suggest that we should expect these methods to be important for understanding the true significance of applied mathematics. The syntactic rules that we happen to start with are not always the best fit for the world—they may need to be retrofitted according to feedback from applications.

6.0 CAN SOLUTIONS BE SANITIZED?

6.1 INTRODUCTION

So far, I have argued against the sanitized approach towards the interpretation of theories on the grounds that many theoretical methods used by physicists are excluded by the sanitized approach but contain information that is relevant to interpretation. In this chapter I lay out another general reason for why we might expect the sanitized approach to miss out on interpretively relevant information. I use examples from classical continuum mechanics and quantum field theory to illustrate why an unsanitized approach is preferable.

In these examples, what I call the *solutions* for specific physical systems described by the theory provide information about the world that comes in very different mathematical formulations depending on the physical system in question. Furthermore, the appropriate mathematical formulations are still being worked out, even in "mature" physical theories like classical continuum mechanics. Thus, one cannot clearly delineate some mathematical object or set of mathematical objects that wholly captures the content of the theory. Instead, one has to consider the significance of effective computational techniques that point the way towards possible solutions, even if the latter have not been made rigorous yet. The sanitized approach, with its premature restrictions on the theory's content, effectively concentrates on only a small subset of information that the theory is capable of providing.

6.1.1 The Sanitized Approach

Laura Ruetsche (2011) offers a neat characterization of what I call the sanitized approach as follows. She divides the process of interpreting a theory into two phases. In the first phase, one specifies the set of mathematical structures that the theory uses to represent reality. This includes accounts of all the possible temporal histories of systems falling under the theory. At the end of the first phase, all we have is a set of *mathematical* structures. In the second phase, one characterizes the *physical* instantiations of these structures.[1]

Ruetsche calls her view the the "standard account" of theory interpretation, for good reason. The first phase of interpretation that she describes is particularly common in the philosophy of physics literature. One might, for example, isolate a set of mathematical structures that allegedly represent all systems in classical mechanics, then figure out whether the theory so defined is deterministic, supports some ontology, explains the relevant phenomena, and so on (Earman, 1986; Belot, 1998; Allori, 2013). Furthermore, even though Ruetsche's second phase of interpretation may contain elements of the "semantic view" of theories, her first phase is used by adherents of both the "semantic view" and the "syntactic view" of theories. Hans Halvorson, who holds the syntactic view, also takes the first step of interpretation to involve giving the theory a mathematical description, because "having such a description greatly facilitates our ability to draw inferences securely and efficiently" (Halvorson & Müger, 2006). Importantly, this mathematical description must meet mathematicians' standards of rigor.[2] This element of the sanitized approach is also fairly standard in the literature, where it is common to take lack of rigor in a computational technique as a sign that it is not relevant to interpretation (Fraser, 2011).

I plan to argue against the above account of the first phase of interpretation, which I phrase as follows:

(M) When interpreting a theory T, the first step is to specify a set of mathematical structures that represent reality in the theory, where such structures are defined according to mathematicians' standards of rigor.

I argue against **(M)** using the examples of classical continuum mechanics and quantum

[1] For example, one may take a differentiable manifold, specified in the first phase, to instantiate spacetime in the second phase.

[2] We can see this in how Halvorson says that the standards of formalization for a physical theory were at first given by some first-order language, but then were later loosened to those corresponding to the standards of professional mathematicians. In addition, Halvorson characterizes an alternative approach to interpretation based on physicists' less rigorous techniques as a "new way", different from his own, of understanding interpretation (Halvorson & Müger, 2006).

field theory. In both theories, we would be ill-advised to stick to **(M)** because of the unpredictable mathematical multifariousness of what I will call "solutions" to "problems" that occur in the theory. Solutions are crucial to the task of interpretation, as they describe the behavior of possible physical systems compatible with the theory, and thus are among the structures that Ruetsche thinks should be specified in the first phase of interpretation. Problems merely provide a set of equations that we have to solve to get solutions—the equations alone, with no solution, do not provide an explicit description of the systems in question. Often, when the sanitized approach is applied, philosophers end up considering only problem settings within the theory or only the very simplest solutions in the theory, even though most problems in the theory require further mathematics to be solved. For example, philosophers have often taken Maxwell's theory of electromagnetism to be just Maxwell's equations for the vacuum, dismissing the intricacies of getting solutions to these equations as irrelevant because they involve approximations or are merely pragmatic (Muller, 2007; Zuchowski, 2013).

Solutions in physics, I argue, tend to be mathematically multifarious in an unpredictable manner, and many of them are as yet unknown. Thus, in our present state of knowledge, they cannot be assumed to be characterized by some set of mathematical structures, even if all the problems can be couched in the same mathematical formalism, such as in a set of equations having the form of Maxwell's equations.[3]

The upshot of all this is a general argument in favour of paying more attention to the ways in which problems are solved in physics and the myriad mathematical forms these involve, instead of dismissing these as mere pragmatics. The characterization of a theory's content offered by the sanitized approach is insufficient to determine the mathematical nature of the solutions in a theory. But the solutions are relevant to interpretation. Thus, the mathematical structures of interpretive interest are not automatically singled out by the mathematical structures that philosophers like Halvorson and Ruetsche take to be of prime philosophical interest.

Notably, this lack of determinacy about which mathematical structures contribute to a

[3]That is, the problems may in their symbolic presentation have the same form, but the unknowns in their equations may refer to very different kinds of mathematical structures. We will see how this happens in Section 6.3.

theory's content does not force us to give up on mathematical rigor. Despite Halvorson's fears, we can still "draw inferences securely and efficiently" even if we reject **(M)**. In QFT and classical continuum mechanics, we can in many situations construct solutions in a rigorous manner. It is just that these solutions cannot all be characterized by a predetermined set of mathematical structures. Rigor in these constructions means ensuring that one uses only clearly defined mathematical structures to build up the solutions. It does not, however, mean that the mathematical nature of a solution is determined purely by the mathematical formalism of the problem it solves.

6.2 PROBLEMS AND SOLUTIONS IN GENERAL

The distinction between problems and solutions is not one that can be defined in purely mathematical terms. However, it is a distinction that appears often in both mathematics and physics. In physics, one typically is faced with a set of equations, often differential equations, that define the problem. These equations typically contain one or more unknown functions that describe the physical system of interest.[4] The objective then is to obtain an evaluation of these unknown functions, so that we have a description of the physical system of interest.[5] This evaluation is known as a solution to the initial problem.

We can illustrate the problem-solution distinction with a simple example from classical mechanics. Consider Newton's second law, $F = m\frac{d^2x}{dt^2}$. Thus stated it does not yet describe the specific behavior of a system—it is merely a problem *schema*. We need the specific form of F to even get an equation that, if solved, gives us the specific behavior of a system. For example, in the case of an object in free-fall near the surface of the earth, we might insert $F = mg$, and this suffices to let us solve the equation $mg = m\frac{d^2x}{dt^2}$ to obtain a function $x(t)$ which describes the position of the body over time. $x(t)$ is the solution to the problem $mg = m\frac{d^2x}{dt^2}$, and it describes a possible evolution of a system obeying $m\frac{d^2x}{dt^2} = mg$.

In short, $F = m\frac{d^2x}{dt^2}$ presents a general schema for solving problems, but for a fuller

[4]This simplifies things a little. As we will see, sometimes the unknown entities are not functions but more complicated entities, that nevertheless are what we would think of as describing the system of interest.

[5]The evaluation may proceed numerically, or it may an analytic expression of the function.

description of the system, we need special force laws, which are different in different physical situations, that indicate what form F takes. The fact that the schema has a unified form, however, often lures philosophers into assuming that the mathematics of the theory has a more unified and predictable content than it actually has.

In classical continuum mechanics, there is also a distinction between more general equations that apply to every system and more specific equations that differ from situation to situation. The former are known as field equations and the latter as constitutive equations. Field equations apply to all materials, whereas constitutive equations differ depending on the material being modelled. Rubber and steel, for example, obey the same field equations, but their constitutive equations differ. In order to say something about the behavior of a specific material, we need the constitutive equations in addition to the field equations. This is analogous to how special force laws are necessary in order for $F = m\frac{d^2x}{dt^2}$ to describe how a specific system behaves. In continuum mechanics, the equations to be solved are typically partial differential equations (PDEs). These define the problem. Solving a PDE means evaluating the unknown "function" that features in the PDE.[6] This evaluation would give us a description of how the system behaves in time, space or both.[7]

In short, just as with $F = m\frac{d^2x}{dt^2}$, in continuum mechanics we have a general schema that provides a unified symbolism for all problems, but a description of a particular system requires specifications based on the materials involved. Only then can a problem be specified and solved.

As one might expect at this point, the same situation holds in QFT. The key entity in QFT, the one providing a general schema for obtaining descriptions of particular QFT systems, is the partition function or generating functional:

$$Z = \int \mathcal{D}\phi e^{\int d^4x \mathcal{L}[\phi]}, \tag{6.1}$$

where ϕ is the quantum field of interest, $\mathcal{L}[\phi]$ is the Lagrangian which determines the dynamics of ϕ, and $\mathcal{D}\phi$ indicates some kind of measure over the possible values of ϕ.

[6] I put "function" in scare quotes because we will see later that the unknown entity can be interpreted to be something other than a function.

[7] That is, PDEs can describe only static situations, in which case we are solving only for variation in space, but they can also describe time evolution, in which case we are looking for variation in time and space.

The form of $\mathcal{L}$ differs between different Lagrangian models.[8] Each model describes a particular particle physics phenomenon—the quantum electrodynamics Lagrangian describes electromagnetic interactions, the quantum chromodynamics Lagrangian describes strong force interactions, and so on. Thus, $\mathcal{L}$ is the analogue of continuum mechanics' constitutive equations. We *solve* a Lagrangian model characterized by $\mathcal{L}$ if we provide a mathematically well-defined evaluation of the right-hand side of Equation 6.1 that satisfies certain conditions which will be described in Section 6.4. All observable quantities in QFT can be calculated from an evaluation of Z. Equation 6.1 sets up a problem schema by providing a formalism from which we could *possibly* get to an evaluation which gives us the observables that we want,[9] while a *solution* is that evaluation. We will see in Section 6.4, however, that without further specifications and modifications, the nature of which depend on the form of $\mathcal{L}$, the right-hand side of Equation 6.1 is a mere formalism, not something that denotes a definite mathematical structure.

In all three examples above, we saw that the theory has a problem schema that can be expressed in a unified symbolic form. However, to get individual problems, we have to supply further information specific to each problem. Later on we will see that the mathematical forms of the solutions to these specific problems vary significantly between different problems. Thus, even though the unified symbolic form of the problem schema tempts us into attributing to the theory the kind of mathematical closedness and predictability implied by (**M**), this falls apart when we look at the mathematical characters of the solutions.

6.3 PROBLEMS AND SOLUTIONS IN CONTINUUM MECHANICS

My main aim in this section is to describe how the mathematical nature of the solution to a problem in continuum mechanics is not given just by the mathematical setting of the problem. Often, our notion of the appropriate space of structures from which we should draw our solutions, and even of the meaning of the derivatives that appear in the equations

[8] I mean "model" here in the loose sense of the physicist who is modeling a specific system, not the set-theoretic sense.

[9] I say "possibly" because not all problems have solutions.

characterising the problem, will change depending on the physical context. In other words, the correct solution may involve mathematics that is not explicitly present in the problem, because we reason to the need for additional mathematics using considerations beyond the mathematics of the problem. Furthermore, this additional mathematics is relevant to our interpretation of the physical situation.

Partial differential equations (PDEs) are central to continuum mechanics. On the face of it, in order for PDE to hold of a physical system, all the derivatives that appear in the PDE must exist. After all, how can we say that $\frac{\partial^2 u}{\partial t^2} = \frac{\partial^2 u}{\partial x^2}$ for a particular function u if either $\frac{\partial^2 u}{\partial t^2}$ or $\frac{\partial^2 u}{\partial x^2}$ does not exist for that u? Thus, *prima facie* the problem setting of a PDE demands that the solution be one in which all the nth-order derivatives of the solution that appear in the PDE exist. Solutions of this kind are known as *strong solutions*. It may seem, then, that the mathematical setting of the PDE suffices to fix the mathematical nature of the solution. As interpreters of the theory, we may then want to say something about how the systems satisfying the PDE in question are continuous in space and time, for example.

However, once we actually set about finding solutions to important physical systems described by PDEs, this naive preconception is undermined. In many physical situations it is prudent to allow for what are known as *weak solutions*.[10] Weak solutions lack some of the derivatives that appear in the PDE. Essentially, they or their relevant nth-order derivatives are discontinuous at certain points, meaning that the PDE does not strictly hold at those points. However, elaborate mathematics has been developed to allow us to rigorously treat such weak solutions and to define clearly the senses in which we may consider them solutions to the original PDE. I will now describe a few of the available methods for defining weak solutions.[11]

One common way to define weak solutions is to reinterpret the original PDE as an integral equation. Essentally, what we are doing is saying that the original PDE does not hold strictly at individual points, but only over an extended region of space. This reinterpretation allows more singular solutions to be considered as ordinary solutions of the integral equation, and we can use this criterion to define a weak solution to the original PDE.

[10]These are also known as "generalized solutions".

[11]The descriptions of the methods below are all taken from Tao (n.d.).

Another way to define weak solutions is to invoke the mathematical apparatus of distribution theory. Roughly speaking, distributions are entities that are defined by the numbers they give when we integrate them together with sufficiently smooth "test functions" over an extended region of space. A distribution can be a weak solution to a PDE. When we invoke distributional weak solutions, we have altered the solution space of the PDE from that of functions differentiable to some nth order to that of some set of distributions.

Yet another way of defining weak solutions is to introduce a regularizing parameter ϵ to construct a series of approximating equations to the original PDE, indexed by ϵ, where $\epsilon = 0$ indexes the original PDE. The idea is that if we can find solutions to the approximating equations, then we can take the limit as $\epsilon \to 0$ on the solutions to the approximating equations. If this limit exists, then the solution at that limit is the weak solution to the original PDE. For example, we can find a weak solution to the inviscid Burgers equation $\partial_t u + u\partial_x u = 0$ by solving the viscous Burgers equation $\partial_t u - \epsilon\partial_{xx} u + u\partial_x u = 0$ for small ϵ. Then we take the limit on the solution to the viscous Burgers equation as $\epsilon \to 0$.

This enumeration of ways of defining weak solutions is not exhaustive—other definitions such as variational solutions, stationary solutions, viscosity solutions, penalised solutions and so on have proven to be applicable in physics. These methods of defining weak solutions show that the mathematical nature of solutions to a PDE cannot be determined just from the original PDE, partly because the meaning of what a "solution" to a physical problem is is not a solely mathematical problem, but also because the meaning of the original PDE can be reinterpreted to allow a different class of solutions than what the original setting seems to allow for. Importantly, even though these different definitions of weak solutions may be related, they are not always equivalent. So, the fact that different physical situations call for different definitions of weak solutions does suggest that the mathematical nature of solutions fragments across different physical systems, even if these all fall in some sense under the same "theory" of continuum mechanics. The mathematician Terry Tao points out:

> it is often the case that the behaviour of PDE depends quite sensitively on the exact structure of that PDE (e.g. on the sign of various key terms), and so any result that captures such behaviour must, at some point, exploit that structure in a non-trivial manner; one usually cannot get very far in PDE by relying just on general-purpose theorems that apply to all PDE, regardless of structure (Tao, 2008).

The mathematical multifariousness of solutions in continuum mechanics matters for philosophers because what the theory says about the world depends on the nature of solutions in the theory. If solutions with singularities are allowed, this could mean that the theory says that shock waves occur in the relevant physical system. A distributional solution might indicate a shock wave, but it might also indicate that the PDE should be interpreted as providing answers to measurements over extended regions of space, rather than as describing what happens at points of space. If one adheres strictly to **(M)** and identifies a predefined set of mathematical structures with the theory, one is prematurely restricting the set of possible solutions of the theory. There remain many problems in continuum mechanics that are unsolved, and we ought not to legislate beforehand what their solutions will be like. The theory may say much more multifarious things about the world than we might expect from looking at the simplest solutions.

In the following sections, we will see how similar ambiguities and mathematical complexities occur in the solutions of QFT.

6.4 PROBLEMS AND SOLUTIONS IN QUANTUM FIELD THEORY

In this section I raise two problems for **(M)** when it comes to quantum field theory. The first problem is that when we look at the attempts to rigorously derive solutions in constructive QFT,[12] we will see that different problem settings in QFT require different kinds of mathematical structures for their solutions, and the mathematical nature of these solutions is in no sense contained in the mathematics of the problem settings. Rather, figuring out what kind of solution is appropriate for a particular problem setting in QFT requires careful attention to many other factors not contained in the mathematics of the problem setting. One such factor is the empirically successful calculational methods of perturbative QFT. These methods are mostly not mathematically rigorous, but mathematicians who try to construct rigorous solutions to QFT problems often use information gleaned from these methods to construct their solutions.

[12]See Chapter 2 for an outline of the different varieties of QFT.

The second problem is that what counts as a rigorous constructive solution in QFT is determined in part by a kind of correspondence with perturbative "solutions" that physicists have derived through mathematically unrigorous methods. These two problems make it hard to achieve **(M)**. In the first place, we cannot determine in advance what kinds of mathematics we will need to describe solutions of the many unsolved problems in QFT. In the second place, it is hard to see how a purely mathematical definition of a solution can guarantee that all the structures that fall under it will also have the abovementioned semi-formal property of correspondence.

6.4.1 The First Problem: Giving a Meaning to the Functional Integral

Constructive QFT is the effort to construct, according to the usual standards of mathematical rigor, solutions to Lagrangian models in QFT, and to prove that these solutions satisfy certain axioms that we expect to apply to all Lagrangian models. Thus, it is the project of evaluating the right-hand side of Equation 6.1.[13] Before evaluating the expression, however, we have to provide a meaning to it. One has to define what the integral over function space could possibly mean. This includes showing that a well-defined measure (in the sense of probability theory) exists that would allow us to make sense of the integral over function space. Furthermore, on the evaluation side of things, one has to ensure that the expression converges.

As I have emphasized, Equation 6.1 *by itself* is a mere formalism because we have not yet assigned a meaning to the functional integral. Because it is a mere formalism, it is possible to have multiple definitions of the functional integral which we regard as reasonable. And indeed, it turns out that within CQFT there exist several ways through which one may define the functional integral.

One major barrier to defining the functional integral is that many of the more naive attempts to define it imply that the integral is divergent and therefore not meaningful. Because of this, one has to resort to more roundabout ways of defining the integral. These

[13] It is worth noting that physicists have their own methods of evaluating the same expression, but these methods typically do not live up to mathematicians' standards of rigor. However, we will see later that constructive field theorists often use information from such non-rigorous methods to guide their rigorous constructions.

roundabout ways involve not explicitly integrating over infinitely many degrees of freedom, but instead taking a limit on a more manageable expression that involves fewer degrees of freedom.[14] If the limit exists, then the functional integral is meaningful. The expression involving fewer degrees of freedom is known as a *regularized* version of the functional integral. The process of converting the original integral into some analogue with fewer degrees of freedom is known as *regularization*.

Even though regularization is a necessary step to defining the functional integral, there are many ways to carry out this regularization. One of the most popular is a lattice regularization, in which one diminishes the degrees of freedom by considering spacetime as a lattice rather than as a continuum. One defines a "lattice" version of the integral, evaluate the integral in this lattice form, then takes the limit of the values of the integral as the lattice spacing goes to zero. This limit is then defined to be the continuum formulation of the integral, that is, the right-hand side of Equation 6.1.

Another regularization method restricts the degrees of freedom in the integral by imposing "cutoffs" in momentum and space. Essentially, the original functional integral, which ranged over fields of all momenta and all of spacetime, is restricted to fields of momenta below a certain "cutoff" high momentum and to a finite volume of spacetime. The right-hand side of Equation 6.1 is then defined as what we get when we take the limit of the cutoff expression as the momentum cutoff goes to infinity and as the spacetime cutoff goes to infinity.

Both regularization methods face further technical difficulties. One method may work better than the other when it comes to constructing Z for a particular Lagrangian $\mathcal{L}_1$, but the other method may be superior if we consider another Lagrangian $\mathcal{L}_2$. In effect, our definition of what the functional integral is changes depending on the Lagrangian.[15]

In short, as a first step we already see that the formalism of Equation 6.1 can be made to correspond to different kinds of mathematical structures. It will not do to say, "No, really

[14] In this context, fewer degrees of freedom corresponds to fewer possible values that ϕ can take on—that is, the integral with fewer degrees of freedom is restricted to some smaller subset of possible ϕs than the original integral.

[15] This is a statement of the current situation in CQFT. Perhaps there is some Future Science in which one can prove that one method of regularization works for all physically interesting $\mathcal{L}$s, but there is no evidence for this claim.

the limits of the lattice-regularized expression and the cutoff-regularized expression are both the same mathematical structure, namely the right-hand side of Equation 6.1," because the latter has no independent definition other than being defined as the limit of some regularized expression.[16] This is analogous to how, in the case of weak solutions of PDEs, we cannot insist that the different definitions of weak solutions all refer to the same mathematical thing, since there is no general mathematically defined Weak Solution that encompasses all the individual definitions.

The fact that different ways of constructing Z are not equivalent is further supported by the fact that constructive field theorists acknowledge that the failure of one method of construction does not imply that a solution does not exist. Sometimes one method of construction can work where others fail (Wightman, 1986; Rivasseau, 1991). It depends on the Lagrangian one is dealing with. Once again, the situation is analogous to that of solutions to PDEs. The fact that one kind of weak solution exists (or not) to a PDE does not in general imply that other kinds of weak solution exist (or not), because the different definitions of weak solution are not mathematically equivalent.

Besides regularization methods, there are other dimensions on which constructions of Z can differ. One such dimension is the form of the "bare Lagrangian" that one starts out with. In QFT, the Lagrangian is a scale-dependent entity. That is, the form of the Lagrangian changes depending on the length scale (or, equivalently, the energy scale) at which the phenomenon of interest occurs. In evaluating the right-hand side of Equation 6.1, one initially takes $\mathcal{L}$ to be some "bare Lagrangian" of a certain form, which will however have to be modified in the process of the construction, due to the presence of multiple length and energy scales in the functional integral. In four dimensions, this modification takes the form of "counterterms" which one adds to the bare Lagrangian. The nature of these counterterms is generally determined by studying the calculational methods that physicists have found to work in non-rigorous perturbative QFT. Nothing in the axioms of QFT, in Equation 6.1, or in any of the rigorous methods involved, tells us which counterterms to use. We have no recourse but non-rigorous perturbation theory. This is yet another dimension on which

[16] Again, this is with reference to current mathematics. Perhaps in some future more general mathematical framework, both limits can be shown to be the same thing in that framework.

Equation 6.1 is really just a bare formalism to be filled in with further information, rather than something mathematically unambiguous.[17]

6.4.2 The Second Problem: Correspondence with Physicists' Methods

A large part of the motivation of CQFT is to show that the Lagrangian models that physicists have used with such empirical success do in fact have solutions that are rigorously defined. Physicists have long used their own "solutions" in the form of renormalized perturbation series, but these are obtained in mathematically dubious ways. Nonetheless, the empirical success of these series suggest that they are *like* solutions. We want the rigorously constructed solutions of CQFT to be connected, somehow, with this empirical success, and thus with the renormalized perturbation series. We also want to make sure that the Lagrangian we construct a solution for is "the same" Lagrangian that the physicists use. This is more involved than it appears because the physicists' Lagrangian is not mathematically well-defined as it stands. While physicists can write down formally that $\mathcal{L} = \frac{1}{2}\left((\partial\phi^2)^2 - m^2\phi^2\right) - \frac{\lambda}{4!}\phi^4$, say, this is not enough to constitute defining $\mathcal{L}$ rigorously, because the rules with which they manipulate $\mathcal{L}$ are not well-defined.

Suppose, then, that I show that for a particular $\mathcal{L}$, the right-hand side of Equation (6.1) can be rigorously constructed. Arthur Wightman asks:

> How can you answer the question, "What problem did you solve?" The answer would be "I solved the problem of showing that certain limits existed and that they had certain properties." But you never write down any condition which fixed the theory you were talking about. We argue, of course, that conventional renormalized theories are fixed by choosing coupling constants and masses (Wightman, 1986).[18]

Coupling constants and masses are the constant coefficients that occur in an expression of the $\mathcal{L}$ used by physicists. For example, in $\mathcal{L} = \frac{1}{2}\left((\partial\phi^2)^2 - m^2\phi^2\right) - \frac{\lambda}{4!}\phi^4$, the coupling constant is λ and the mass is m. In identifying a model by its renormalized coupling constants and

[17] It is important to note that even though the choice of counterterms is determined using information from non-rigorous perturbation theory, the construction itself is still rigorous. In perturbation theory the unrigorous part is not the addition of counterterms *per se* but rather the taking of limits on perturbation series, the assumption that perturbation series have a meaning without checking conditions of validity, the failure to define functional integrals properly, and various other things. Constructive field theory patches these gaps, but it does not eschew the use of counterterms.

[18] Wightman is using "theory" where I would use "model".

masses, Wightman is acknowledging that a construction in CQFT counts as a solution to a Lagrangian model if it corresponds, somehow, to the physicists' renormalized theories with certain coupling constants and masses.

Much of the work in CQFT revolves around defining this correspondence relation. One option is to say that the correspondence exists if the rigorously constructed Z is *asymptotic* to the renormalized perturbation series. A function is asymptotic to a series expansion if, roughly speaking, the successive terms of the series provide an increasingly accurate description of how fast the function grows. Asymptotic series need not be convergent.[19] However, whether a function is asymptotic to a series depends on the methods available for summing the series. CQFT is engaged in finding new ways to sum physicists' perturbation series so as to relate them to the non-perturbative constructions of Z. This means that the notion of "correspondence" is expanding as we find new ways to sum perturbative series and possibly prove asymptoticity. It would thus be premature to follow **(M)**'s dictates by delineating in advance a class of structures that includes all the possible solutions. This class may expand as we add more summation techniques to our arsenal.

To complicate matters further, what we really want from a solution is for the *continuum* Lagrangian, that is, the one we get after taking the relevant limits on the cutoff integral or the lattice, to correspond to the Lagrangian that is used by physicists in perturbative QFT. This can happen even if the bare Lagrangian one starts with in CQFT contains different terms than the bare Lagrangian that physicists use to get their perturbative results. Thus, even if one fails to construct the ϕ^4_4 model, say, using a bare Lagrangian $\mathcal{L} = \frac{1}{2}\left((\partial\phi^2)^2 - m^2\phi^2\right) - \frac{\lambda}{4!}\phi^4$, which is what physicists use in perturbation theory, this does not rule out the possibility of constructing the same model using a bare Lagrangian $\mathcal{L} = \frac{1}{2}\left((\partial\phi^2)^2 - m^2\phi^2\right) - \frac{\lambda}{4!}\phi^4 - \mu\phi^6$ in a lattice context (Gallavotti & Rivasseau, 1984).[20] Both these bare Lagrangians could give rise to Zs that are asymptotic to the renormalized perturbation series that physicists calculate from the bare Lagrangian $\mathcal{L}$ using perturbation theory. Thus, they could both be possible starting points for a construction of ϕ^4_4. This creates further ambiguities in the construction process, since it is not even clear what form of Lagrangian should be the input

[19] See Erdelyi (2010) for a technical definition of an asymptotic expansion.

[20] The difference between these two Lagrangians, which consists in an entirely new term ϕ^6, cannot be accounted for by counterterms, which serve only to change coefficients in a Lagrangian that already exist.

for the right-hand side of (6.1). There is a kind of underdetermination problem here: the formalism of Equation (6.1) and the criterion of correspondence to renormalized perturbation series are not sufficient to determine what kind of mathematical structure a rigorous solution would be. Given this, it would be unwise to legislate in advance what the solution to a specific Lagrangian model should look like.

I have said enough, I believe, to show that the routes to constructing a definition of the right-hand side of Equation 6.1 are multifarious and not mathematically equivalent.[21] One type of construction may work for given Lagrangian model while another might not. The latter type of construction may, however, be well-suited to another Lagrangian model. Equation 6.1 alone does not tell us which is the correct construction for a given $\mathcal{L}$. Rather, the considerations mentioned above provide some guidance, and of course one ultimately has to attempt the construction in excruciating detail to determine if it is suitable. The formalism of Equation 6.1 does not determine the mathematical nature of the solution for a particular Lagrangian model. The mathematical meaning of Equation 6.1, in fact, is exactly what is being worked out in the construction process, not something that is declared beforehand. This mathematical meaning, and the mathematical meaning of the solution, fragments across different specific $\mathcal{L}$s. Furthermore, we have not constructed any solutions yet for models in four-dimensional spacetime, which are exactly the ones that describe the actual world. Thus, contrary to **(M)**, insofar as solutions to Lagrangian models are part of QFT and insofar as said solutions are relevant to the interpretation of QFT, we ought not to take the referent of "quantum field theory" to be some already well-defined mathematical structure. While the various axioms of QFT are well-defined, the axioms do not contain the detail of information, ontological or otherwise, that the solutions do. Once we take into account the solutions, the wisdom of **(M)** is thrown into doubt.

Note that my claim is not just that **(M)** is problematic unless we have the *specific* solutions for all problems that fall under the theory. For **(M)** might still be useful if we could figure out the broad mathematical nature of the specific solutions for all the problems in a theory—if we could, for example, say that they are all C^∞ functions or something like that. Such a broad characterization would still provide interpretively relevant information.

[21] Again, I cannot rule out an equivalence proof in some future mathematics.

Rather, my claim is that we cannot even give a *general* mathematical characterization of the solutions on the order of saying that they are all structures of a certain type. Specific unsolved problems have a tendency to demand new mathematical structures and new understandings of existing formalisms for their solutions, and this is especially the case in QFT.

As in the continuum mechanics case, in QFT a contributing factor to the failure of **(M)** is the fact that the notion of a solution is not wholly defined by mathematics, or at least not by existing mathematics. This is because the solutions to Lagrangian models in QFT are supposed to correspond in some way to the kinds of perturbative "solutions" which physicists have been using, which are not mathematically well-defined themselves. Since the mathematical structures representing the possible histories of QFT systems must contain among them solutions to Lagrangian models, these structures cannot be specified in the way demanded by the sanitized approach.

6.5 CONCLUSION

I have argued in this chapter that we ought to reject the methodological requirement **(M)**—the requirement of taking the referent of a physical theory to be a well-defined mathematical structure. The requirement is inadvisable both in classical continuum mechanics and quantum field theory, because even if the problem settings in those theories can be given a uniform mathematical description, their solutions cannot. Since solutions are the closest thing we have to explicit descriptions of the physical systems allowed by a theory, they are part of what should be interpreted when we interpret a theory. Thus, if we want to include solutions in our interpretations of QFT and continuum mechanics, we cannot apply **(M)** to these theories. Neither theory corresponds to a well-defined mathematical structure because their solutions fragment into mathematical structures of various natures depending on the specific problem setting at hand and the physical context, and the appropriate mathematical structures for many problems are still being worked out.

In both cases part of the reason for this is that the notion of a *solution* to a problem is not one that is completely defined by mathematics alone. In continuum mechanics, weak solutions were admitted as legitimate solutions partly because the singularities in weak solutions corresponded to phenomena such as shock waves that we had observed in the target physical systems. There is no "pure" mathematical reason for admitting weak solutions as legitimate solutions instead of restricting the class of legitimate solutions to strong solutions. In CQFT, the project is to construct mathematically rigorous structures that "correspond" to the non-rigorous Lagrangian models that physicists have been successfully using. Since the latter are not mathematically well-defined, the relation of correspondence cannot be one that is mathematically well-defined. This provides a certain leeway and ambiguity in what counts as a solution in CQFT. It is also part of the reason why new mathematics plays an important role in CQFT. Defining the notion of correspondence means trying to give rigorous meaning to the calculational methods of physicists. This often leads to new mathematics because said methods are successful in a way that we do not completely understand. Part of elucidating the notion of correspondence is improving our mathematical understanding of these methods. Given this continuous influx of new mathematics into CQFT, it would be premature to limit QFT prematurely to some already known mathematical structure, as **(M)** proposes.

I suspect that this point about solutions generally being more mathematically multifarious and less well-understood than their problem settings is a general one in physical theory. Philip Davis (2009) has argued that in mathematics generally, it is not clear that there is a homogeneous conception of what it means to solve a problem. One could also view the issue as one in which the initial problem setting, as a mere formalism, contains multiple possible mathematical interpretations. Often finding the correct solution involves first figuring out what the correct mathematical interpretation of the formalism of the problem setting is. Thus, in PDEs one must first figure out which function space the derivative operators are acting on, and in QFT one must figure out what the functional integral means. Vincent Rivasseau, one of the main figures involved in obtaining solutions to models of QFT, endorses this view of the flexibility of mathematical interpretations inherent in many problems:

> in mathematics non-existence theorems, although quite common, rarely remain the last word on a subject. Often a problem with no solution is simply badly formulated and has to wait until the proper formalism in which it does have a solution is found (Rivasseau, 1991, p. 271).

Philosophers of science would do better to recognize the flexibility inherent in figuring out which mathematics is part of the content of a physical theory. Problems in physics are not problems in pure mathematics. Part of solving a problem in physics is figuring out the appropiate mathematical formalism to couch it in. The choice of formalism is not always handed to us on a plate by the "fundamental equations". We should interpret theories with a more accommodating eye to the mathematics involved in applying the theory to specific systems.

7.0 SUMMARY AND CONCLUDING REMARKS

In this dissertation I have argued for a more unsanitized approach towards interpreting physical theories, as opposed to the sanitized approach of taking a theory to refer to a set of mathematical objects. My narrative took two prongs. The first, in Chapters 3 and 5, called for more flexible ways of interpreting mathematical formalisms in light of certain patterns of development that tend to recur in the history of mathematics and physics. The second, in Chapters 4 and 6, looked at contemporary mathematical methods in physics and argued that the sanitized approach misses out on the interpretive import of many of these methods. We saw that even when we stick to conventional standards of mathematical rigor, the mathematics required to describe physical systems compatible with the theory may not simply be contained in the fundamental equations or axioms of the theory. Instead, more careful attention to the mathematics used in specific applicational contexts is required. Unfortunately, in many cases this mathematics is still under development and we cannot legislate in advance what the correct mathematics will look like.

The general approach I have taken in this dissertation can be extended to many other cases in the philosophy of physics where the sanitized approach is implicitly assumed. In general, philosophers of physics have tended to focus more on the import of what they regard as the fundamental equations of a theory, rather than the mathematics required for specific applicational contexts. However, if the lesson that I gather from the case of QFT is correct, paying more attention to how we get actual solutions from the fundamental equations may be useful for interpretation and not just "mere pragmatics". My approach also takes us closer to the practice of physicists by suggesting that in some cases the apparently unrigorous "heuristics" used by them may be interpretively relevant. Standards of reasoning that are prevalent in pure mathematics may not be as helpful for philosophers and physicists, because sometimes these standards are overly constraining for the development of new mathematics that physics often demands. It is my hope that philosophers of physics will keep these lessons in mind in their future practices.

APPENDIX A

DEFINITIONS OF SUMS OF DIVERGENT SERIES

One possible way of giving a meaning to the sum of divergent series is to consider the average of the arithmetic means of a series. This idea came from considering convergent series with positive, decreasing terms, such as

$$\sum_{0}^{\infty}\left(\frac{1}{2^n}\right) = 1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \dots$$

This series has the conventional sum 1. The arithmetic means of the partial sums of this series, $\frac{1}{N+1}\sum_{k=0}^{N}\sum_{i=0}^{k}\left(\frac{1}{2^i}\right)$, where N is finite, approximate 1 to a closer and closer degree as N increases. Indeed, one can prove that for such a series the convergent sum of the series is equivalent to the limit as $N \to \infty$ of the arithmetic mean of the partial sums of the series. Since the differences between consecutive partial sums in such a series decrease as $N \to \infty$, one would intuitively expect the average of the partial sums to approximate the sum for large enough N.

These considerations for what works for a certain kind of paradigmatic convergent series can then undergo syntactic extensions to suggest summation methods for divergent series. Some divergent series have a well-defined "average" of those of their partial sums for arbitrarily large N. Since with convergent series this average would be the conventional sum of the series, perhaps this way of defining a sum could lead to finite sums for divergent series. This is exactly the step of syntactic extension that characterizes Stage 1 of the canonical pattern of development.

Formally, we can state this extended definition of a sum as follows. Let s_k be the partial sum containing the first $k+1$ terms of the sequence (s_n). Define

$$c_m = \frac{s_0 + s_1 + \ldots + s_m}{m+1}.$$

If c_m tends to a limit s as $m \to \infty$, we define s as the C_1-sum of (s_n).[1]

With this technique you can assign C_1-sums to some series that do not have a *conventional sum* as defined above. For example, if two series (a_n) and (b_n) have conventional sums and are multiplied together by Cauchy's rule to give the series

$$c_n = \sum (a_0 b_n + a_1 b_{n-1} + \cdots + a_n b_0),$$

it can be shown that this series may not have a conventional sum as (c_n) may be divergent. But it can be proven that

$$\frac{C_0 + C_1 + \cdots + C_n}{n+1} \to AB,$$

where A and B are the conventional sums of (a_n) and (b_n) respectively, and C_n is the nth partial sum of $\sum c_n$. Thus, the product of $\sum a_n$ and $\sum b_n$ has a C_1-sum even if it does not necessarily have a conventional sum.

Similarly, the divergent series $1 + 1 - 1 + 1 - \ldots$ has a C_1-sum of $\frac{1}{2}$ even though it lacks a conventional sum. This results agrees with other alternative ways of summing that series, though not all of them.

At the same time, the C_1-sum counts as an *extension* of the conventional sum because in every case where the conventional sum gives a value s, the C_1-sum gives the same value.

There are many other extensions of the conventional sum proposed by Cesaro, Hölder, Borel, and others that can sum various kinds of divergent series besides the ones mentioned here (Knopp, 1951, §59). These extensions were typically motivated by some form of syntactic extension from more established mathematical rules, such as rules that work for sums of finitely many terms or rules that work for convergent infinite series. This constitutes Stage 1 of the canonical pattern of development. In Stage 2, prospective definitions of sums were compared with respect to their consistency with one another for divergent series, and with respect to their agreement with conventional sums for convergent series.

[1] I take this terminology from Knopp (1951, p. 464).

For example, the C_1-sum agrees with conventional sums in the cases where the series are convergent. For the divergent series $1 - 1 + 1 - 1 + \ldots$, the C_1-sum gives the sum $\frac{1}{2}$, which happens to agree with other ways of summing the series, such as Euler's method in which he uses the expansion

$$\frac{1}{1-x} = 1 + x + x^2 + x^3 + \ldots$$

and puts $x = -1$. Furthermore, it turns out that the anomalies mentioned above, in which other methods of summation assign sums like 0 and 2/3 to the series in question, can be reinterpreted as being based on mistaken principles. One consequence of the new definitions of sums of series is that adding zeroes to a series can change the summability or sum of the series. Thus, $1+0-1+1+0-1+1+\ldots$ has the sum $\frac{2}{3}$ but cannot be identified, under C_1 summation, with the series $1 - 1 + 1 - 1 + \ldots$ (Hardy, 1949, 59). This consequence also makes sense of results like getting $\frac{2}{3} = 1-1+1-1+\ldots$ from the expansion $\frac{1+x}{1+x+x^2} = 1-x^2+x^3-x^5+x^6+\ldots$: the former expression should instead be interpreted as $\frac{2}{3} = 1+0-1+1+0-1+\ldots$. Finally, (5.1) can be explained by the general failure of associativity in addition or subtraction when it comes to divergent series. The fact that, under C_1 summation, $1+0-1+1+0-1+1+\ldots = \frac{2}{3}$ but $1 - 1 + 0 + 1 - 1 + 0 + \ldots = \frac{1}{3}$ shows that assocativity does not hold when it comes to divergent series.

Another way in which new definitions were chosen with respect to their similarity with older ones was by the extent to which they preserved fundamental operations that applied to convergent series. Ford (1960, 87) lists a few such operations that many of the new definitions preserved. One of them is the following: if two divergent series $\sum_{n=0}^{\infty} u_n$, $\sum_{n=0}^{\infty} v_n$ have the sums s_1 and s_2 respectively, then one can impose the condition that the series $\sum_{n=0}^{\infty} (u_n \pm v_n)$ has the sum $s_1 \pm s_2$, according to the same summation method used to sum the divergent series. Another is Abel's theorem on the product of series, which says that if $\sum u_n = U$, $\sum v_n = V$, $w_n = u_1 v_n + u_2 v_{n-1} + \cdots + u_n v_1$, then $\sum w_n = UV$. Abel had proved that this held if $\sum w_n$ converges, but Cesaro proved that it also held for series that have C_1-sums (Tucciarone, 1973).

Yet another way of evaluating different definitions was to consider the fruitfulness of certain definitions for other areas of mathematics. For example, given a series (a_n), one might consider the corresponding power series $f(x) = \sum_{n=0}^{\infty} a_n x^n$. If we focus on those series for which $f(x)$ converges iff $-1 < x < 1$, we could postulate that we should retain only those definitions of sums for which the sum of (a_n) is $\lim_{x \to 1^-} f(x)$. It turns out that this way of restricting allowable definitions is useful in the study of analytic functions (Ford, 1960, 83). Finally, choices of definitions for types of sums also depend on certain definitions being able to sum mathematically and physically important series like Fourier series and the Dirichlet series (Tucciarone, 1973).

APPENDIX B

INTERPRETATIONS OF THE OPERATIONAL CALCULUS

In the history of the operational calculus, syntactic extensions were checked using conventional methods of obtaining solutions to differential equations. Carson and Cooper were among those who used ordinary methods to solve differential equations in order to confirm that Heaviside's algebraic methods gave the same results (Lützen, 1979). Cauchy and Heaviside also used more conventional methods to verify the results of the operational calculus. Cauchy solved a differential equation in both the operational way and the conventional way, confirming the correctness of the operational result (Petrova, 1987). Heaviside compared multiple conventional methods with his operational method in order to obtain the operational result $(p)^{1/2} H(t) = 1/\sqrt{\pi t}$, where $H(t)$ is the Heaviside step function (Petrova, 1987).[1]

One step in Stage 2 that contributed towards Stage 3 was to consider what conditions were required in order for the calculations in Stage 2 to succeed. Heaviside had proceeded as though operators were commutative: that if f were a function and A, B operators, then $ABf = BAf$. Algebraic methods showed that this could be the case if f and some of its derivatives vanished at 0. Specifically, Florin (1934) found that for differential equations, converting $D_t f$ into $pf - pf(0)$, where f is a function, led to commutativity. Separately, Berg found that if we followed Heaviside in assuming that f is always multiplied by the Heaviside step function H, then $p^{-1}pf(t)H(t) = f(t)H(t) - f(0)H(0)$, so that Heaviside's calculus

[1]From now on I will use p to denote D_t in order to accentuate the algebraic aspect of the operational calculus.

worked only if $f(0)H(0) = 0$ (Lützen, 1979, 188-9). All this led to the idea that initial values are important in explaining the success of the operational calculus—Heaviside had succeeded only because the functions he operated on and some of their derivatives vanished at 0. It also suggested that if one wanted to give a general account of the operational calculus that did not rely on special initial values, then one had to find an algebra of operators in which either the differential operator or the integral operator has to be redefined—one cannot be the inverse of the other. The importance of initial values and the restrictions on inverse operators gleaned from these Stage 2-type considerations pointed the way towards possible Stage 3-type interpretations. Mikusinki's rigorous algebraic account of the operational calculus eventually derived the substitution rule $D_t f \to pf - p(0)$ as a theorem (Lützen, 1979, 190).

Integral transformations were another way of interpreting the operational calculus.[2] T. J. Bromwich found that after the operational solution $f(p)$ to a PDE was found, that is, a solution like (5.5), one can derive the ordinary solution $h(t)$ to the PDE by the formula

$$h(t) = \frac{1}{2\pi i} \int_{c-i\infty}^{c+i\infty} \frac{f(p)}{p} e^{pt} dp, \tag{B.1}$$

where for the purposes of integrating the right-hand side, one treats p like a scalar variable. This formula is closely related to what we would nowadays call an *inverse Laplace transform*, namely

$$h(t) = \frac{1}{2\pi i} \int_{c-i\infty}^{c+i\infty} f(p) e^{pt} dp.$$

For convenience's sake I will continue the discussion using this modern inverse Laplace transform and its inverse, the *Laplace transform*

$$f(p) = \int h(t) e^{-pt} dt.$$

While Bromwich gave no rigorous account of why the inverse Laplace transform worked, van der Pol provided the following explanation. Instead of only applying the Laplace transform at the last step to derive the ordinary solution from the operational solution, the original PDE is to be reinterpreted from the outset as having a Laplace transform applied

[2] The following material on integral transformations is largely taken from Lützen (1979).

to it. This move turns the PDE into an ODE with one less variable. For example, (5.2) is to be reinterpreted as

$$\int \frac{\partial^2 u}{\partial x^2} e^{-st} dt = \int \frac{\partial u}{\partial t} e^{-st} dt.$$

Defining $U = \int u e^{-st} dt$ and integrating by parts, we eventually end up with the ODE

$$\frac{d^2 U}{dx^2} = u(x, 0) + sU.$$

When $u(x, 0) = 0$, this is the same as the ODE (5.3) obtained above using a brute algebraization of the D_t operator. One can then derive the ordinary solution by applying the inverse Laplace transform, as Bromwich had done without clearly understanding why. Thus, by interpreting Heaviside's and Bromwich's manipulations as shorthand for an initial transform from the t-domain to the s-domain, one can understand how the PDE can be changed into an easier-to-solve ODE by algebraizing suitable operators when the initial value of the function is zero. While Bromwich had supplied part of Stage 2 of the canonical pattern of development, van der Pol's explanation of how to reinterpret the original PDE supplied part of Stage 3.

The operational calculus could also be interpreted in terms of other integral transformations, such as Fourier transforms. One important observation was that in all integral transform interpretations, a key move was to transform the differental operator D_t into a multiplication operator αp, where α is a numerical constant. This led Florin (1934) to articulate some general conditions that integral transformations must satisfy in order to be useful for reinterpreting the operational calculus. These conditions included the inverse Laplace transform and Bromwich's transformation, but also expanded the class of integral transformations that would work for this purpose.

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