Reduction, Emergence and Renormalization

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Abstract

In previous work, I described several examples combining reduction and emergence: where reduction is understood *à la* Ernest Nagel, and emergence is understood as behaviour or properties that are novel (by some salient standard). Here, my aim is again to reconcile reduction and emergence, for a case which is apparently more problematic than those I treated before: renormalization.

Renormalization is a vast subject. So I confine myself to emphasizing how the modern approach to renormalization (initiated by Wilson and others between 1965 and 1975), when applied to quantum field theories, illustrates both Nagelian reduction and emergence. My main point is that the modern understanding of how renormalizability is a generic feature of quantum field theories at accessible energies gives us a conceptually unified family of Nagelian reductions.

That is worth saying since philosophers tend to think of scientific explanation as only explaining an individual event, or perhaps a single law, or at most deducing one theory as a special case of another. Here we see a framework in which there is a *space* of theories endowed with enough structure that it provides a family of reductions.

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1 Introduction

1.1 Defending two morals

Renormalization is undoubtedly one of the great topics—and great success stories—of twentieth-century physics. Also it has strongly influenced, in diverse ways, how physicists conceive of physical theories. So it is of considerable philosophical interest. I propose to honour the memory, and the philosophical legacy, of Ernest Nagel by relating it to his account of inter-theoretic relations, especially reduction.[1]

I confess at the outset that I have an axe to grind: or more playfully, a hobbyhorse to ride. In previous work (especially 2011, 2011a), I argued that reduction and emergence are compatible. I took reduction of theories *à la* Nagel: as deduction of the reduced theory, usually using judiciously chosen definitions of its terms (bridge-laws), from the reducing one. And I took emergence as behaviour or properties that are novel (by some salient standard). In claiming this compatibility, I was much indebted to discussions by both philosophers and physicists that emphasized the importance of limiting relations between theories; especially the work in recent years by Batterman, Berry and Kadanoff.[2]

Thus one of my leading ideas was that reduction and emergence are often combined by one theory being deduced as a limit of another, as some parameter $N \to \infty$: the novel behaviour occurs at the limit. This was one of the four main Morals of my (2011), which I labelled '(Deduce)'. Another Moral was that the limit $N = \infty$ is not physically real, but an idealization, and that what is physically real is a logically weaker, yet still vivid, novel behaviour that occurs already for finite N. Thus I labelled this Moral, '(Before)', meaning 'emergence (in a weaker form) before getting to the limit'.

I illustrated these two Morals with four main examples (and urged that there were many others).[3] One main example was drawn from thermal physics: phase transitions, like boiling, freezing and melting. If, as usual, phase transitions are taken to require singularities of thermodynamic quantities, then under very general conditions, a statistical mechanical description of a phase transition, i.e. a description

[1]This paper is based in part on the Nagel Memorial Lecture, Columbia University, 2 April 2013. I am very grateful to Columbia University for the honour of giving this lecture; and for gracious hospitality and discussion. I also thank Sidney and Yvonne Nagel for heart-warming information about Ernest Nagel's life.

[2]For example: Batterman (2002, 2010), Berry (1994), Kadanoff (2009, 2013). It will be clear below, starting in Section 1.2, that I interpret Nagelian reduction broadly, in particular as not requiring formalised languages or deductions. This means there is less dispute between me and some "anti-Nagelian" authors, such as Batterman himself, than there might seem.

[3]I will not discuss my other two Morals: they were about supervenience being scientifically useless, and about the description of the system becoming unrealistic for very large N. In fact, I think these Morals also hold good more widely. But in Section 5.1, I will touch on another previous topic: whether the limit $N \to \infty$ is mathematically singular, and whether this causes trouble for reduction.

in terms of the system's microscopic constituents, requires that there be infinitely many such constituents (Kadanoff 2009 Section 1.5, 2013 Section 2.2). Thus with N as the number of constituents, the limit $N \to \infty$ is called 'the thermodynamic limit". Our obtaining, in this limit, a description of a phase transition illustrates (Deduce). But the description is surely an idealization, since a boiling kettle contains a finite number of atoms—illustrating (Before).[4]

In this paper, I propose that renormalization also illustrates (Deduce) and (Before). But I do not aim just to pile up illustrations. Renormalization is of special interest, for two reasons. The first relates to my opening remark, that it is of great physical importance. So we should consider how it bears on philosophical accounts of reduction, and related ideas like explanation.

The other reason arises from the discussion of phase transitions. There is a class of phase transitions called 'continuous phase transitions' (or 'critical phemonena'), which are hard to understand quantitatively, because they involve many length scales. Indeed it was only by using the modern approach to renormalization (initiated by Wilson and others between 1965 and 1975) that they were quantitatively understood (even for classical, not just quantum, systems). Some authors have argued that these examples cause trouble for reduction, or at least for Nagel's concept of it; (e.g. Batterman 2002, 2010, 2011). But I think Nagelian reduction, understood broadly, and thus my reconciling Morals, apply here also (my 2011, Section 7; Bouatta and Butterfield 2011).

However, I will not rehearse that dispute again. I propose instead to discuss renormalization as applied to quantum field theories about sub-atomic physics, rather than to theories in statistical mechanics, about solids, liquids and gases. This is a vast subject: even bigger than renormalization's description of phase transitions in statistical mechanics. And I will limit myself to showing how the modern approach to renormalization, when applied to quantum field theories, illustrates Nagelian reduction. My main claim will be that the modern understanding of how renormalizability is a generic feature of quantum field theories, at the sorts of energy we can access in particle accelerators, gives us a conceptually unified family of Nagelian reductions. (This claim will be developed in Sections 2.1, 4.2.1 and 4.3.) So here, the emphasis will be on illustrating the Moral (Deduce), as much as (Before).

This endeavour has three motivations, apart from the pleasure of honouring Nagel's legacy. First, philosophers tend to think of scientific explanation as only explaining an individual event, or perhaps a single law, or at most deducing one theory as a special case of another. Here we see a framework in which there is a *space* of theories endowed with enough structure (called the *renormalization group*) that it provides a family of reductions. Besides, each reduction will illustrate the Morals, (Deduce) and (Before). In fact, the parameter N that goes to infinity will be distance (or equivalently: the reciprocal of energy); and the emergent behaviour that is deduced in the

[4]Fine recent discussions include: Batterman (2011), Kadanoff (2009, 2013), Menon and Callender (2013) and Norton (2012,2013).

infinite-distance, i.e. zero-energy, limit will be the renormalizability of the theory.

The second and third motivations arise from the current philosophical discussion about the modern approach to renormalization. As I mentioned, this approach applies equally to statistical mechanical theories of solids, liquids and gases, and to quantum field theories about sub-atomic physics. But philosophical discussion has emphasized the former (especially, reduction vs. emergence in continuous phase transitions): suggesting that it is now worth scrutinizing the latter. Agreed: there is also reason to resist this change of focus. The quantum field theories of interest here, viz. interacting theories in four spacetime dimensions, are *not* mathematically well-defined: at least not yet! So one might say that philosophical scrutiny should wait until such time as they are well-defined. But I will later (starting in Section 2.2) urge that despite these theories' lack of rigour, the time is ripe for philosophically assessing them.

On the other hand, some of the philosophical literature about renormalization (including that about interacting quantum field theories) is "anti-Nagelian". For some authors hold that the modern approach to renormalization, and-or associated ideas like universality and effective field theories, make trouble for Nagelian reduction, and-or for broader doctrines like reductionism. I have already mentioned Batterman (2002, 2010, 2011). Also, Bain (2013, 2013a) holds that effective field theories make trouble for Nagelian reduction; and Cao and Schweber (1993) hold that the modern approach to renormalization and effective field theories prompt pluralism about ontology and anti-reductionism in methodology. So this is my third motivation: I will want to reply, albeit briefly, to these authors.

1.2 Prospectus: Nagel endorsed

To help frame what follows, I will first state the plan of the paper, and then briefly adumbrate a Nagelian view of reduction.

In Section 2, I announce my main claim: that the dwindling contribution, at lower energies, of non-renormalizable interactions amounts to a set of Nagelian reductions. Then I urge that we can and should philosophically assess quantum field theories that are not mathematically well-defined. In Section 3, I sketch the traditional and modern approaches to renormalization. With that exposition in hand, Section 4 returns to philosophy, and argues for the main claim just mentioned: that renormalizability at the energies we can access fits well with Nagel. Finally, in Section 5 I respond to some of the anti-Nagelian discussions mentioned at the end of Section 1.1.

Turning to Nagel's account of reduction: I admire, even endorse, this account, as modified by Schaffner (1967, 1976, 2006). But I will not try to review, let alone defend, the details; for two reasons. First, Nagel's stock-price is rising. Recent defences of Nagel's account, and similar accounts such as Schaffner's, include e.g. Endicott (1998), Marras (2002), Needham (2009), Dizadji-Bahmani, Frigg and Hartmann (2010). And Schaffner has recently given a masterly review of this literature, and defence of his own account (2013). In short, Nagel hardly needs *my* endorsement. Second, I have elsewhere given details of my endorsement (2011a, Sections 2 and 3). Some of those details will surface in Sections 4 and 5. But for the moment, I just set the stage by recalling the bare bones of the Nagelian account I endorse. I will begin, in (i) to (iii) below, by describing reduction as a logico-linguistic relation between theories. But then I will emphasize, in (1) and (2), that there is *no* requirement that the theories' languages. or the deductions of the reduced theory's claims, or the definitions of its terms, must be formalized.

Recall the initial idea. Theories are taken as sets of sentences closed under deducibility; so the intuitive idea of reduction—that a theory T_1 reduces another T_2, if T_2 is a part of T_1—becomes the idea that T_2 is a sub-theory of T_1. That is: as a set of sentences, it is a subset of T_1. But Nagel modifies this in three main ways. It will be clear that modifications (i) and (ii) answer accusations that deducibility is too strong a notion to express reduction of one theory to another; and that (iii) answers the accusation that it is too weak.

(i): T_2 may well have predicates, or other vocabulary, that do not occur in T_1. So to secure its being a sub-theory, we need to augment T_1 with sentences introducing such vocabulary, such that T_2 is deducible from T_1 as augmented. For a predicate, the simplest, and so most usually discussed, form for such a sentence is a definition in logicians' sense, i.e. a universally quantified biconditional, specifying the predicate's extension by an open sentence in the vocabulary of T_1. In short: we allow that T_2 is deducible, not from T_1 on its own, but from T_1 augmented with a set of judiciously chosen sentences ('bridge-laws'): Nagel (1961, pp. 354-358; 1979, pp. 366-368).

(ii): What is deducible from T_1 may not be exactly T_2, but instead some part, or close analogue, of it. There need only be a strong analogy between T_2 and what strictly follows from T_1. Nagel called this *approximative reduction* (1979, pp. 361-363, 371-373).

(iii): These modifications, (i) and (ii), still allow there to be aspects of T_2, even aspects that are essential to its functioning as a scientific theory, that are not captured by any corresponding aspects of T_1. For example: they allow that a bridge-law could be so long, say a million pages, so as to defy human comprehension—and thus prevent T_1 giving us any understanding of T_2, or of its subject-matter. So Nagel suggested (1961, pp. 358-363) that in each bridge-law, the part from T_1 (e.g. the open sentence in the language of T_1 that is the *definiens* in logicians' sense of the predicate from T_2) should play a role in T_1. So it cannot be a million pages long; and it cannot be a very heterogeneous disjunction.

I endorse this Nagelian account. There are three main points to make, by way of clarifying and defending it; (setting aside details, e.g. about accommodating functional definitions, which I give elsewhere: 2011a, Sections 2 and 3).

(1): The first and most important point is that, although the account takes theories as sets of sentences, and (i)-(iii) have a logic-chopping appearance, there is no requirement at all that the language of the theories, or the notion of deducibility, be formalized (let alone first-order). Nagel and his supporters (including Schaffner et al. listed above) of course know that scientific theories are not formalized, and are hardly likely to be, even in mathematized subjects like physics. But that does not prevent one undertaking to assess whether there are relations of reduction between theories (in Nagel's sense; or indeed, in another one). The informality merely makes one's specification of the theories in question somewhat vague (as are (ii) and (iii) themselves, of course); and so one's ensuing assessments are correspondingly tentative.

Thus in particular: there is no requirement that (i)'s 'definitions' that enable a deduction to go through must (a) be only of predicates; or (b) use as compounding operations only the Boolean and quantificational ones familiar from elementary logic. They can perfectly well use advanced mathematical operations: e.g. limiting operations, as emphasized in Section 1.1's discussion of (Deduce). Nor is there any requirement that (ii)'s deductions be formalized: they need only be valid by the (informal!) standards of mathematics and physics.

We can already see how this Nagelian account can fit with the Morals, (Deduce) and (Before). The idea is: T_2 with its emergent behaviour corresponds to an $N = \infty$ limit of T_1. In some cases, it is not exactly true: nor even, true by the lights of T_1. But (Before) holds: a weaker yet still vivid version of the novel behaviour occurs at finite N, and strictly follows from T_1.[5]

(2): Second: By saying above 'Nagelian account', and citing only Nagel himself, I do not mean to tie my colours exclusively to his mast. In fact, I am inclined to concur with Schaffner's revisions, giving what Schaffner (1977) first dubbed the GRR ('general reduction replacement') model of reduction. For example, Schaffner's GRR model develops (ii)'s notion of analogy, as a way of expressing approximate reduction. For details, I recommend Schaffner (2013): it situates the GRR model in the landscape of the reception of Nagel's proposals (Section II) and discusses partial reductions in detail, with an optics example (Sections IV to VI). But my claims in this paper (especially Sections 4 and 5) will not turn on differences between Nagel himself and Schaffner.

(3): Points (1) and (2) have been irenic: they present the Nagelian account as a broad church. But on another controversy, I dig my heels in. Thus some reject

[5]Two clarifications. (1): I agree that the tenor of Nagel's writings—and of his times—also suggests a narrower, and I admit implausible, account requiring that bridge laws be biconditionals, and even that theories, and so deductions, be formalized. (Thanks to Bob Batterman and Jim Weatherall for emphasizing this.) (2): Agreed, the Nagelian account uses the syntactic view of theories, which is usually opposed to the semantic view. But I contend that the syntactic view, with no requirement of formalization, can describe perfectly well the phenomena in scientific theorizing that advocates of the semantic view tout as the merits of models. Indeed, I think this is hardly contentious. For to present a theory as a set of models, you have to use language, usually (at least in part) by saying what is true in the models; and on almost any conception of model, a model makes true any logical consequence of whatever it makes true. Thus I happily join Nagel in accepting the syntactic view of theories—while of course admitting there remains room for debate: for the most recent exchange, cf. Glymour (2013) and Halvorson (2013). But cf. (3) below, for a critique of the syntactic view which is more radical than the semantic view.

the idea of a scientific theory (whether on the syntactic, or the semantic, view) as a useful category for the philosophical analysis of science. Besides, some support this rejection by taking as their examples the mathematically ill-defined quantum field theories that will be our topic from Section 2 onwards; saying that they should not count as theories. As an aspiring Nagelian, I of course disagree! But I postpone giving my reasons till Section 4.1.

2 Introducing quantum field theory

As I announced in Section 1.1, my overall aim is to argue that the modern approach to renormalization illustrates Nagelian reduction; and that my morals (Deduce) and (Before) hold good. To expound this, it will be clearest first to emphasize the conceptual aspects of the physics, without regard to Nagel (this Section and the next); and thereafter, to turn to philosophy, urging that the physics illustrates Nagelian reduction.

More specifically: in the rest of this Section, I will: say a bit more about my main claim (Section 2.1); admit that the theories with which we are concerned are not mathematically rigorously defined (Section 2.2); but then hold that they are defined well enough that we can and should assess them philosophically (Section 2.3). Then in Section 3, I will give more details about the two approaches to renormalization, and how the modern approach shows that renormalizability is a generic feature of theories at low energies. Thereafter, I emphasize philosophical issues.

2.1 Renormalizability at accessible energies: grist to Nagel's mill

Renormalization is essentially a framework of ideas and techniques for taming the infinities that beset quantum field theories. It is usual, and it will be helpful here, to distinguish two approaches to the subject: a traditional one, and a modern one.

The traditional approach had its first major successes in 1947-1950, in connection with quantum electrodynamics (QED). In those years, figures like Dyson, Feynman Schwinger and Tomonaga showed that their formulations of QED, using the ideas and techniques of renormalization, agreed with the phenomenally accurate experiments (largely at Columbia University) measuring shifts in the energy-levels and the magnetic moment of an electron in an atom, due to vacuum fluctuations in the electromagnetic field. After these triumphs of quantum electrodynamics, this approach continued to prevail for two decades. For us, the main point is that it treats renormalizability as a necessary condition for being an acceptable quantum field theory. So according to this approach, it is a piece of good fortune that high energy physicists can formulate renormalizable quantum field theories that are empirically successful. Indeed, great good fortune: for since about 1970, high energy physicists have elaborated the *standard model*. This combines quantum electrodynamics with renormalizable quantum field theories for forces other than electromagnetism—the weak and strong forces, that are crucial for the physics of the atomic nucleus; and for forty years, the standard model has had stunning empirical success.

But between 1965 and 1975, another approach to renormalization was established by the work of Wilson, Kadanoff, Fisher etc. (taking inspiration from ideas in the theory of condensed matter, i.e. liquids and solids, as much as in quantum field theory). For us, the main point is that this approach explains why the phenomena we see, at the energies we can access in our particle accelerators, are described by a renormalizable quantum field theory.

In short, the explanation is: *whatever non-renormalizable interactions may occur at yet higher energies, their contributions to (the probabilities for) physical processes decline with decreasing energy, and do so rapidly enough that they are negligible at the energies which are accessible to us.* Thus the modern approach explains why our best fundamental theories (in particular, the standard model) have a feature, viz. renormalizability, which the traditional approach treated as a selection principle for theories.

From a philosophical perspective, this point is worth emphasizing, quite apart from its scientific importance. For philosophers tend to think of scientific explanation as only explaining an individual event, or perhaps a single law, or at most deducing one theory as a special case of, or a good approximation of, another. This last is of course the core idea of Nagel's account of inter-theoretic reduction.

But the modern approach to renormalization is more ambitious: it explains, indeed deduces, a striking feature (viz. renormalizability) of a whole class of theories. It does this by making precise mathematical sense of the ideas of a *space of theories*; and a flow on the space, called the *renormalization group* (RG). It is by analyzing this RG flow that one deduces that what seemed "manna from heaven" (that some renormalizable theories are so empirically successful) is to be expected: the good fortune we have had is generic.[6]

Besides, this point is, happily, *not* a problem for Nagel's account of inter-theoretic relations. For my main claim will be that it provides a conceptually unified *family of Nagelian reductions*. That is: the explanation, using renormalization group ideas, of why contributions to physical predictions from non-renormalizable interactions dwindle at lower energies, amounts to a set of Nagelian reductions. For a renormalization scheme that defines a flow to lower energies amounts to a set of bridge-laws that enable a deduction *à la* Nagel, from a theory describing high-energy physics, of a low-energy theory. And because the same scheme shows how many similar high-energy theories flow to correspondingly similar low-energy theories, we have, not just a set, but a

[6]More generally, the idea of a flow on a space of theories, passing between energy regimes, has been has immensely fertile throughout physics: so fruitful that although Nobel Prizes tend to be awarded for experimental work, several have been awarded for theoretical advances associated with this idea.

conceptually unified family, of Nagelian reductions.

Returning to the Morals, (Deduce) and (Before), introduced in Section 1.1: these will again be illustrated. For from a high-energy theory with one or more non-renormalizable interactions, we deduce, by adjoining suitable bridge-laws, the low-energy theory. We take the parameter N to be the reciprocal of energy; (or equivalently, as we shall see in Section 3.1: to be distance). In the limit of zero energy (or infinite distance), a non-renormalizable interaction makes zero contribution. Thus renormalizability is the emergent behaviour which is deduced; and of course, what is physically real is the regime of low energies, or large but finite distances, in which the contributions of non-renormalizable interactions are negligible but not exactly zero.

I will spell out this main claim in Section 4.3, after expounding renormalization. And I will support it with ancillary claims, about the status of the theories concerned (Section 4.1), multiple realizability (Section 4.2) and the idea of effective field theories (Section 5).

But before describing renormalization (Section 3), I should address the fact that the quantum field theories we will be concerned with—like quantum electrodynamics, and the other theories of the weak and strong forces that make up the standard model—are *not* mathematically rigorously defined; so that some philosophers are wary of analyzing them, and some even argue that they should not count as physical theories. I will maintain that they are well enough defined as physical theories that a philosophical assessment of them is appropriate; and more specifically, that in such an assessment, a reasonably precise notion of physical theory—indeed, Nagel's notion—applies to them. I will urge this, in two stages. First: I will urge in Sections 2.2 and 2.3, i.e. before describing renormalization, that although they are ill-defined, the time is ripe for their philosophical assessment. Second: after I describe renormalization—and so, after we get a better sense of why these theories are ill-defined—I will suggest that they nevertheless pass muster as theories in Nagel's sense (Section 4.1).

2.2 Heuristic but successful theories

In both physics and its philosophy, there is of course a balance between heuristic, informal work and mathematically rigorous work. The quantum field theory community of course does both kinds of work; but philosophers of quantum field theory have tended to concentrate on the second. In this and the next Subsection, I will urge that the time is ripe for philosophical assessment of the first kind of work—which this paper aims to instantiate. I will urge this, first, in general terms; and then in Section 2.3, in relation to simplifications at higher energies revealed by the modern approach to renormalization.

Quantum electrodynamics, and the other theories that make up the standard model, are not known to be rigorously/mathematically definable. In short: they are all *interacting quantum field theories*, the interaction being, for example, between an electron and the electromagnetic field (due to the electron's electric charge). But after more than seventy years of effort, we still do not know how to rigorously define their central theoretical notion, viz. a path integral that gives amplitudes (i.e. complex square roots of probabilities) for various quantum processes—except in some special cases. (The special cases involve certain types of interaction, and-or a reduced number of dimensions of space, and-or imagining there is in fact no interaction, e.g. that all particles have zero electric charge, so that the electromagnetic interaction is "turned off"—called a *free*, as against an *interacting*, theory.)[7]

What to do? Since the 1930s, the main strategy adopted has been to calculate each amplitude using a perturbative expansion. This is a power series, i.e an infinite sum $\sum g^n A_n$ with terms including successive powers of a number g. The broad idea is that:

(i): g is usually small, in particular less than one (so that $g^n \to 0$ as $n \to \infty$), which helps successive terms to get smaller, and the series to converge; and

(ii): physically, g is a *coupling constant*, whose value encodes the strength of interaction between two fields: e.g. the electric charge of an electron, encoding the strength of interaction between the electron field and the electromagnetic field.

But there are several severe troubles, conceptual as well as technical, about these perturbative expansions. The main one is that even if g is small, the other factor in the nth term, viz. A_n, is typically *infinite*. This is quantum field theory's notorious problem of infinities: which, as we will see, is addressed by renormalization. Why the A_n are infinite, and how renormalization addresses this by introducing a cut-off and then analysing what happens when the cut-off tends to a limit, will be taken up in Section 3.1 et seq. For the moment, I just confess at the outset that the overall problem of infinities will *not* be fully solved by renormalization, even by the modern approach (Section 3.2). The infinities will be tamed, even domesticated: but not completely eliminated.

And yet, it works! For example: in quantum electrodynamics even the first few terms of the perturbative series give predictions that match experimental results (like those in the famous Columbia experiments of Foley, Kusch, Lamb, Retherford et al.) upto eleven significant figures; (cf. Feynman 1985, pp. 6-7, 115-119; Schweber 1994, p. 206f.; Lautrup and Zinkernagel 1999; Bricmont and Sokal (2004, p. 245)). This is like predicting the measured diameter of the USA to within the width of a human hair.

So the situation combines mathematical *lacunae*, even embarrassments, with supreme empirical success: an odd situation. In any case, for more than seventy years, the quantum field theory community has divided its labours between two kinds of work: (i) developing the heuristic formalisms of perturbation series etc. (of course often using rigorous mathematics to do so); and (ii) pursuing a mathematically rigorous definition of the central notion, the path integral, and allied notions. Of course, the distinction between (i) and (ii) is vague. There is really a spectrum of kinds of work, and plenty of synergy between the different kinds; although, physics being a practical

[7]For a glimpse of these issues, cf. e.g. Jaffe (1999, 2008), Wightman (1999).

and empirical subject, far more quantum field theorists do work at the first heuristic end of the spectrum than at the second rigorous end.

In recent decades, philosophers of science who have approached quantum field theory have tended to emphasize the second kind of work: especially a framework called 'algebraic quantum field theory'. That is understandable: their temperament and training of course emphasizes rigorous argument, and one would not want philosophical discussions to be turn on proposals from physics that are fallible and-or temporary, e.g. because they depend on some specific model.

But we philosophers certainly need to engage with the first kind of work. It is not just that it rules the roost in physics. Also, it harbours a wealth of results and methods which we can be almost certain are now permanently established; and yet which are conceptually subtle, and so cry out for philosophical assessment—even before "all the theorems are in". And the material summarized in Section 3—the main ideas of renormalization—is among that wealth.

It may at first seem rash to say, after Kuhn and his ilk, that we can be almost certain that some features of quantum field theory are now permanently established. But I mean it. I will not try to defend a general realism or cumulativism about physical theory (let alone science in general).[8]

But I do want to cite a specific argument, developed by Steven Weinberg in a series of articles, and summarized in the opening Chapters of his magisterial treatise (1995, pp. xx-xxi, 1-2, 31-38; and Chapters 2 to 5; 1999, pp. 242-247): which is hardly known among philosophers of science, even philosophers of quantum field theory.[9] Weinberg shows that any theory combining the principles of special relativity and quantum mechanics, and with a plausible locality property (viz. the cluster decomposition property), must at low energies take the form of a quantum field theory. Weinberg's motivation for developing the argument is not just novel pedagogy, i.e. to teach students an approach to quantum field theory other than the usual one of quantizing classical fields. The argument also shows that the framework of quantum field theory would still stand, even if one day we have to throw out our specific currently favoured theories, based on quantizing classical fields (including quantum electrodynamics). As he says: 'If it turned out that some physical system could not be described by a quantum field theory, it would be a sensation. If it turned out that the system did not obey the rules of quantum mechanics and relativity, it would be a cataclysm' (p. 1).

Agreed: any such argument, however brilliant, is only as persuasive as its premises; and the Kuhnian, or more generally the sceptic, may deny that the principles of spe-

[8]For example, cf. Psillos (1999, 2009). But note that some authors suggest a role, in defending scientific realism, for the modern approach to renormalization. The idea is that since the renormalization group protects low-energy physical theories from high-energy effects, we can, and indeed should, believe that these theories will not be overturned by later discoveries about high energies; e.g. Bricmont and Sokal (2004, p. 252-254). I shall return to this in Section 5.3.

[9]So far as I know, the only detailed discussion is Bain (1999): who construes the argument as an example of demonstrative induction.

cial relativity and quantum mechanics are now permanently established. Rebutting that denial would of course require the general arguments I have just ducked out of attempting. But I cannot resist urging, against the Kuhnian and sceptic, that a theory achieving accuracies like one part in 10^{11} is getting so much right about nature that much of what is claimed by its central principles will be retained.

2.3 Simplifications at higher energies

There is also another reason to be optimistic about diving in to philosophically assess heuristic quantum field theory. In brief, the point is that some interacting quantum field theories are in much better mathematical shape than others. Indeed, some are in good enough shape that it is reasonable to hope they *will* be rigorously defined; so that a philosophical assessment, even now, is not foolhardy. More precisely: the modern approach to renormalization (Section 3.2) classifies (the perturbative expansions of) quantum field theories in part by how they behave at successively higher energies, and it turns out that some of our current theories are very well-behaved at high energies. The main case is the theory of the strong force ('quantum chromodynamics': QCD). Its good behaviour is that as the energy gets higher, the strong force gets weaker (according to the perturbative analyses we are now capable of). That is, the theory tends towards a theory in which the "particles" (more precisely: excitations of a quantum field) do not "feel each other" (more precisely: evolve in time independently of each other). Such a theory is called *free*, meaning 'non-interacting'. So this limiting behaviour for successively higher energies is called *asymptotic freedom* (Wilczek 2005). This striking simplification of the theory makes it reasonable to hope that it will be defined rigorously, i.e. independently of perturbative analyses.

I should emphasize that on the other hand, quantum electrodynamics gets more badly behaved as the energy gets higher. That is unfortunate: and not just for our physical understanding, but also because it fosters a misleading impression among philosophers. For there are three obvious factors that make philosophers tend to think of quantum electrodynamics, when the topic of quantum field theory is mentioned.

(i): It was historically the first quantum field theory to have its infinities tamed, in the sense of being shown renormalizable, by the traditional approach to renormalization (Section 3.1). (But as mentioned in Section 2.2, here 'tamed' does not mean 'wholly eliminated'; and nor are the infinities eliminated by the modern approach. This is of course a central aspect of the theories being so far ill-defined.)

(ii): The electromagnetic force is familiar from everyday life, unlike the weak and strong forces.

(iii): *Prima facie*, quantum electrodynamics is a much simpler theory than our quantum theories of the weak and strong forces. That is: all these theories' central postulate is a Hamiltonian. (This is the function encoding the various fields' contributions to energy, that dictates how they evolve in time. It is roughly equivalent to instead specify a related function, the 'Lagrangian'.) The Hamiltonian of quantum electrodynamics is much simpler than that of these other two theories. Besides, following (ii) above: its form is familiar from classical electromagnetic theory, i.e. Maxwell's equations. Thus I say '*prima facie*' because in studying these theories, it is the Hamiltonian that one first meets and analyzes. It takes considerably more study—historically, it was a stupendous achievement—to show that at high energies, the comparison of simplicity can be reversed: that the boot is on the other foot, in that quantum chromodynamics tends to a free theory, while quantum electrodynamics' behaviour gets worse and worse.

These three factors make quantum electrodynamics the archetype, or salient example, of a quantum field theory, for philosophers and other non-specialists. That is understandable, even reasonable. But this theory's bad behaviour at high energy, and its thereby being (probably!) *not* rigorously definable, has the unfortunate consequence of giving philosophers the impression that probably all interacting quantum field theories are doomed to not being rigorously definable. That is an unfair extrapolation: it is reasonable to hope that some such theories, even important theories that we believe to describe nature (viz. quantum chromodynamics), are rigorously definable (Besides, asymptotic freedom is not the only kind of good behaviour at high energy that makes this hope reasonable: two others are called 'conformal invariance' and 'asymptotic safety'.)

So much by way of motivating a philosophical assessment of interacting quantum field theories. Enough said: be it wise or rash, what follows is an exercise in that *genre*.[10]

3 Renormalization

Renormalization is a vast subject, and I can only scratch the surface. In this Section, I will just sketch some ideas of both the traditional and the modern approaches (Sections 3.1 and 3.2); following Baez's helpful introductions (2006, 2009). As announced in Section 2.1, the overall idea will be that while the traditional approach took renormalizability as a selection criterion for theories, the modern approach explains it as a generic feature at accessible energies.[11]

[10]A complementary discussion of the timeliness of such assessments is in Bouatta and Butterfield (2012, Section 2.2). For examples of the views of theoretical physicists, both heuristic and mathematical, about the prospects for interacting quantum field theories, or at least asymptotically free ones, to be rigorously definable, cf. (i) the exchange between Gross and Jaffe in Cao (1999, pp. 164-165); (ii) Jaffe (2008).

[11]Further details are in a companion paper (Butterfield 2013). For slightly more technical introductions, I recommend: (i) Wilson's *Scientific American* article (1979) and Aitchison (1985)'s introduction to quantum field theory, especially its vacuum, which discusses renormalization in Sections 3.1, 3.4, 3.6, 5.3, 6.1; (ii) Teller's philosophical introduction (1989); and as surveys that include some of the history: (iii) Kadanoff (2009, 2013), emphasizing renormalization in statistical mechanics; (iv) Cao and Schweber (1993) and Hartmann (2001), emphasizing renormalization in quantum field theory—to which I will return in Section 5.3.

3.1 The traditional approach

3.1.1: The task: corrections needed : Consider the task of calculating the strength of a source from the measured force felt by a test-particle. In classical physics, this is straightforward. Consider a classical point-particle acting as the source of a gravitational or electrostatic potential. There is no problem about using the measured force felt by a test-particle at a given distance r from the source, to calculate the mass or charge (respectively) of the source particle. Thus in the electrostatic case, for a test-particle of unit charge, the force F due to a source of charge e is given, in appropriate units, by $F = e/r^2$ (directed away from or towards the source, according as the charges of the test-particle and the source are of the same or opposite sign). We then invert this equation to calculate that the source's charge is: $e = F.r^2$.

This straightforward calculation of the source's mass or charge—in the notation of Section 2.2, the coupling constant g—does not work in quantum field theory! There are complicated corrections we must deal with. These depend on the energy and-or momentum with which the test-particle approaches the source. A bit more exactly, since of course the test-particle and source are equally minuscule: the corrections depend on the energy or momentum with which we theoretically describe, or experimentally probe, the system I called the 'source'. We write μ for this energy (and say 'energy', not 'energy or momentum'). These corrections to the coupling constant g, depending on μ, will be centre-stage in both the traditional and the modern approaches to renormalization.

So we write $g(\mu)$ for the *physical coupling constant*, i.e. the coupling constant that we measure: more exactly, the coupling constant that we calculate from what we *actually* measure, in the manner of $g = g(F)$ above, in the simple electrostatic example. Thus the notation registers that $g(\mu)$ is a function of μ.

The need for corrections then means that $g(\mu)$ is not the same as the *bare coupling constant*, g_0 say, that appears in the theory's fundamental equations (like e) in the electrostatic example). But we expect $g(\mu)$ to depend on g_0. So we write $g(\mu) \equiv g(\mu, g_0)$. And our task is to invert this equation, writing $g_0 = g_0(g(\mu))$, and so to assign a value to g_0 that delivers back the measured $g(\mu)$ for the various energies μ with which we observe the system: or more generally, for the various energies μ for which we are confident of the value of $g(\mu)$.

This statement of our task needs to be refined. In fact, we need three refinements, which I will treat in order:

(i) introducing a cut-off;

(ii) letting the cut-off go to a limit;

(iii) allowing for extra terms in the Hamiltonian.

But if we can succeed in the task, as refined, we will say that the interaction in question, or the theory that describes it, is *renormalizable*. After these refinements, I will report that the theories in the standard model are indeed renormalizable.

3.1.2: The cut-off : This task is daunting, because as announced in Section 2.2, our technique for calculating an amplitude, viz. power series, seems to break down: the factor A_n in the nth term $g^n A_n$ is typically infinite. The reason is that A_n is an integral over arbitrarily high energies. To try and get a finite answer from our formulas, the first thing we do is impose a *cut-off*. That is: we replace the upper limit in the integral, ∞, by a high but finite value of the energy, often written Λ: we require by *fiat* that the contribution to the integral from higher energies is zero. So with k representing energy, we require that $\int_\Lambda^\infty dk \; ... \equiv 0$. (There are other less crude ways to secure a finite answer—called *regularizing* the integrals—but I will only consider cut-offs). Thus the physical coupling constant, $g(\mu)$, is a function, not only of the bare coupling constant g_0 and of μ itself of course, but also of the cut-off Λ:

$$g(\mu) \equiv g(\mu, g_0, \Lambda). \tag{3.1}$$

There are two immediate points to make.

First: in quantum theory, energy (and momentum) are like the reciprocal of distance; in the jargon, 'an inverse distance': energy $\sim$ 1/distance. (And so distance is like an inverse energy.) So high energies correspond to short distances; and so to short wavelengths and to high frequencies. So the cut-off Λ corresponds, in terms of distance, to a cut-off at a small distance d. That is: by imposing the cut-off to get finite answers, we are declaring that any fields varying on scales less than d do not contribute to the specific process we are calculating.

Second: It turns out to be possible, and is very convenient, to express all dimensions in terms of length. Thus we can also trade in the energy-scale μ for an inverse length, say $\mu \sim 1/L$ where L is a length. So we re-express the physical coupling constant as a function of L: we will use the same letter g for this function, so that we write $g(L) \equiv g(\mu)$. Thus eq. 3.1 becomes:

$$g(L) \equiv g(L, g_0, d). \tag{3.2}$$

Thus our task can now be stated as follows. We are to measure $g(L)$ (better: to calculate it from what we really measure, like the force F in the simple electrostatics example) and then invert eq. 3.2, i.e. write $g_0 = g_0(g(L), d)$, so as to calculate which value of the bare constant would give the observed $g(L)$, at the given d.

3.1.3: Letting the cut-off d go to zero : Broadly speaking, the exact value of the cut-off is up to us.[12] But of course, the theory and its predictions should be independent of any human choice. And if our theory is to hold good at arbitrarily short lengths (arbitrarily high energies), we expect that g_0 goes to a limit, as d tends to zero (at least at some appropriate L: such as the observed L).

[12] Agreed: for some perturbative analyses of some problems, the physics of the problem will suggests a range of values of d that are sensible to take. That is: the physics suggests that no phenomena on scales much smaller than d will contribute to the process we are analysing.

If this limit exists and is finite, i.e. $\in \mathbb{R}$, we say: the interaction or theory with we are concerned *is finite*. But most successful quantum field theories are *not* finite. The paradigm case is QED, for which the limit is infinite. That is: for arbitrarily high cut-offs, the bare coupling constant g_0 becomes arbitrarily high. Mathematically, this is like elementary calculus where some function $f(x)$ tends to infinity as x tends to infinity, e.g. $\lim_{x\to\infty}\sqrt{x} = \infty$. But of course this last is 'just' the infinity of pure mathematics. But here we face a *physically real infinity* viz. as the value of the bare coupling constant.

The consensus, on the traditional approach to renormalization, is that this physically real infinity is *acceptable*. Accordingly, the adjective 'renormalizable', with its honorific connotations, is used. That is: If g_0 tends to a limit, albeit perhaps $\pm\infty$, we say the theory is *renormalizable*. So in particular: QED is renormalizable in this sense, though not finite.[13]

3.1.4: Allowing for extra terms : It turns out that to write down a renormalizable theory, we may need to add to the Hamiltonian function (equivalently; Lagrangian function) one or more terms to represent extra fields, or interactions between the given fields, even though we believe the bare coupling constant for the extra fields or interactions are zero. The reason is that the interaction might have a non-zero *physical* coupling constant at some scale L; i.e. $g(L) \neq 0$.

So now we should generalize the notation slightly to reflect the fact that there are several, even many, coupling constants to consider; as well as several, even many, possible interactions (terms in the Hamiltonian). So suppose that there are in all N physical coupling constants, $g_1(L), g_2(L), ...g_N(L)$, occurring in the various terms/interactions in our theory.

We similarly generalize slightly our definition of renormalizability. A theory which secures that each bare coupling constant goes to a limit as d tends to zero, by using either (i) no extra terms, or (ii) at most a finite number of them, is given the honorific adjective: *renormalizable*.

The consensus, on the traditional approach, is that renormalizability in this sense is a necessary condition of an acceptable theory. Clearly, this seems a reasonable view. That is, renormalizability seems a mild condition to impose, since its definition has accommodated a succession of complications about the idea of assigning a bare coupling constant: we have had to allow for dependence on the energy μ, on the cut-off, and for extra terms.

3.1.5: Our good fortune : So much by way of explaining the idea of renormalizability. How do the quantum field theories we "believe in", or "take seriously" fare?

[13] But I should add that despite this consensus, most physicists would admit to some discomfort that the bare constant should be infinite in the continuum theory. Thus great physicists like Dirac have been very uncomfortable (cf. the citations in Cao (1997, pp. 203-207)); and Feynman himself calls renormalization 'a dippy process' and 'hocus-pocus' (1985, p. 128).

That is: are the theories which are our best descriptions of the electromagnetic, weak and strong forces, renormalizable in the sense just discussed? Yes they are; though they are not finite.

This circumstance seems a piece of great good fortune. At least, echoing the remarks just above: we are likely to feel it is a relief after our: (a) having to admit that we can so far only define the theory perturbatively (Section 2.2); and (b) having to make corrections to bare coupling constants which turned out to require a succession of complications: (i) dependence on μ (L), (ii) a cut-off that then goes to infinity, and (iii) extra terms.

But we will now see that according to the modern approach to renormalization, this great good fortune is not so surprising. In a certain sense, *renormalizability is generic* at the low-ish energy scales we can access.

3.2 The modern approach

The key initial idea of this approach is that instead of being concerned with good limiting behaviour as the cut-off $d \to 0$, we instead focus on how $g(L)$ varies with L.

Indeed, if we envisage a number of coupling constants, say N for N possible interactions, then the "vector" of coupling constants $(g_1(L), ..., g_N(L))$ represents a point in an N-dimensional space; and as L varies, this point flows through the space. And accordingly: if we envisage a theory as given by a Hamiltonian which is a sum of terms representing different possible interactions, then this space is a space of theories. Jargon: we say the coupling constants *run*, and the flow is called the *renormalization group flow.*

This simple idea leads to a powerful framework, with rich consequences not just in quantum field theory, but in other branches of physics, especially statistical mechanics and the theory of condensed matter. But I shall concentrate on how it explains why a theory about phenomena at the low (or low-ish!) energy scales we can access, is renormalizable. That is: it explains the good fortune reported at the end of Section 3.1 as being generic. This discussion will introduce some jargon, indeed "buzz-words", such as 'fixed points' and 'universality'.

3.2.1: Good fortune explained: non-renormalizable terms dwindle at longer distances : There are of course various controversies about explanation. But it is surely uncontroversial that one very satisfying way to explain the good fortune reported at the end of Section 3.1 would be to show: not merely that some given theory is renormalizable; but that *any* theory, or more modestly, any of a large and-or generic class of theories, is renormalizable. Such an argument would demonstrate that our good fortune was "to be expected". (Admittedly, such an explanation, whether for a single theory, or for a class of them, will have to make some other assumptions about the theory or theories: a point I will stress in Section 3.2.2 below. So it is only relative to those assumptions that the good fortune is explained, and to be expected.)

This is what the modern approach to renormalization gives us, with its idea of a space of theories, on which there is a flow given by varying the energy-scale μ or L.

More precisely and modestly: this approach does not show that any of a large and-or generic class of theories has, at the comparatively low energies and large length-scales we can access, literally *no* non-renormalizable terms. Rather, the approach shows that for any such theory—"with whatever high-energy behaviour, e.g. non-renormalizable terms, you like"—the non-renormalizable terms dwindle into insignificance as energies become lower and length-scales larger. That is, in Section 3.1's notation: the physical coupling constant for non-renormalizable terms shrinks. For such terms: as $\mu \to 0$ (i.e. $L \to \infty$), $g(\mu) \equiv g(L) \to 0$.

The explanation is based on a simply stated criterion for when a theory is renormalizable: more precisely, for when a term is renormalizable. It is a matter of the dimension (as a power of length) of the bare coupling constant in the term. Namely: this dimension needs to be less than or equal to zero. The criterion is due to Dyson, and is sometimes called *Dyson's criterion.*

More precisely: suppose that the bare coupling constant g_0 has dimensions of length^D. Then it turns out that the corresponding physical coupling constant $g(L)$ will scale roughly like L^{-D}. That is:

$$g(L)/g_0 \sim (L/d)^{-D} \ . \tag{3.3}$$

Thus if $D > 0$, the exponent on the right-hand side will be negative; so when L is very small, i.e. much smaller than d, the right hand side is very large. That is: the physical coupling constant will be large compared with the bare one. That is a sign of bad behaviour at small distances L, i.e. high energies. At least, it is bad in the sense that the large coupling constant will prevent our treating the interaction represented by the term as a small perturbation. So it is unsurprising that such a term is non-renormalizable in the sense that Section 3.1 sketched.

But now, instead of considering the case of very small L (so that a non-renormalizable term's positive exponent D makes for a large physical coupling constant): *look at the other side of the same coin.* That is: when L is much larger than the cut-off d, and $D > 0$ (i.e. the term in question is non-renormalizable), then the right hand side of eq. 3.3 is *very small.* That is: the physical coupling constant is very small. So at large distances, the non-renormalizable interaction is weak: "you will not see it".

There are two main points I should make about this explanation, before addressing the Nagelian themes of explanation and reduction (Section 4). The first point is about how non-trivial the *explanans*, i.e. eq. 3.3, is. The second point will somewhat generalize the discussion, from a physical not philosophical viewpoint; and will introduce some jargon.

3.2.2 Decoupling high-energy behaviour : That at large distances, a non-renormalizable interaction is weak follows immediately from eq. 3.3. But that does not make it obvious! A good deal of theory needs to be assumed in order to deduce eq. 3.3. After all, there is of course no *a priori* guarantee that interactions that are strong at short distances should be weak at long distances. To show this "decoupling" of high-energy behaviour from the low-energy behaviour was a major achievement of Wilson and many other physicists, e.g. Symanzik (1973), Applequist and Carazzone (1975). I will not go into details, but just remark that it can be shown under very general conditions, even within the confines of a perturbative analysis.

3.2.3 The renormalization group flow : So far, my talk of the renormalization group flow has been restricted in two ways, which I need to overcome.

(a): A flow can have a *fixed point*, i.e. a point that is not moved by the flow: think of sources and sinks in elementary discussions of fluid flow. For the renormalization group flow, this would mean a set of physical coupling constants $(g_1(L), ..., g_N(L))$ that is unchanged as the length-scale L increases further. Jargon: the behaviour of the system is *scale-invariant*: "you see the same behaviour/theory/physical coupling constants, at many different length-scales". This can indeed happen: the kind of phase transition mentioned in Section 1.1, i.e. continuous phase transitions, provides vivid examples of this. Such a point is called an *infra-red fixed point.* Here, 'infra-red' is used on analogy with light: infra-red light has a longer wavelength, lower frequency and lower energy, than visible light.

(b): So far, we have had in mind one trajectory, maybe leading to a fixed point. But many trajectories might lead to the same fixed point; or at least enter and remain in the same small region of the space. If so, then the 'vectors' $(g_1(L), ..., g_N(L))$ at diverse early points on a pair of such trajectories representing dissimilar theories lead, as L increases, to the same fixed point, or at least to the same small region, and so to similar theories. That is: when you probe at low energies/long distances, "you see the same or similar physical coupling constants". Jargon: This is called *universality.* And the set of 'vectors' that, as L increases, eventually lead to the given fixed point, is called, on analogy with elementary discussions of fluid flow, the point's *basin of attraction.*

But note that universality should really be called 'commonality' or 'similarity', for two reasons. (i): There can be different fixed points, each with their own basin of attraction; and (ii) for a given basin, the vector of physical coupling constants does not encode *everything* about the system's behaviour, so that systems with the same vector will not behave indistinguishably. But jargon aside: Section 4.2 will urge that universality is essentially the familiar philosophical idea of multiple realizability.

Finally, I can summarize this Subsection's main point, that non-renormalizable interactions dwindle at large length-scales, by combining the jargon I have just introduced with the previous jargon that a *free* theory is a theory with no interactions. Namely: the infra-red fixed point of a theory *all* of whose interaction terms are non-renormalizable is a free theory.

4 Nagelian reflections

So much by way of introducing quantum field theories (Section 2) and renormalization (Section 3). In this Section and the next, I turn to this material's bearing on philosophy. As announced in Section 1.2, I will undertake: first, the positive task of fitting this material to Nagel (this Section); and then, the negative task of replying to some anti-Nagelian discussions (Section 5). Both Sections will connect with Section 1's endorsement of a broadly Nagelian account of reduction, and its Morals (Deduce) and (Before).

In this Section, I first urge that although our quantum field theories are not well-defined (Section 2.2), they pass muster as theories in Nagel's sense (Section 4.1). Then in Section 4.2, I will argue that universality (cf. Section 3.2.3) is essentially the familiar philosophical idea of multiple realizability. Then in Section 4.3, I make my main claim: the explanation, using renormalization group ideas, of why contributions to physical predictions from non-renormalizable interactions dwindle at lower energies (cf. Section 3.2.1), amounts to a family of Nagelian reductions.

4.1 Endorsing the idea of a theory

In recent decades, various philosophers have for various reasons criticized or even rejected the notion of theory: even in relaxed versions that make no requirement of a formal language, and whether construed syntactically or semantically. They say that the notion of a scientific theory over-emphasizes the linguistic, logical or semantic aspects of science at the expense of other important, but less formal or tidy aspects. For example: the non-formal aspects of explanation and confirmation; the role of analogy and metaphor; the creation and application of models (in scientists', not logicians', sense!); instrumentation, experiment and simulation; and more generally, the embedding of a theory in wider practices, both cognitive (research and pedagogy) and non-cognitive, in laboratory, lecture-room and society at large.

Obviously, these aspects, both individually and taken together, militate against 'theory' being precisely defined: just because they are informal and complicated. And they are so various that they favour no single revision of the notion of a theory. Their collective effect is rather to suggest 'the scientific theory' is no longer a useful—or at least, not a crucial—category for the philosophical analysis of science.[14]

Besides, this philosophical critique has been supported by historical and sociological studies: including studies of the very theories I have discussed, viz. quantum field theories. Here, I have in mind the work of such authors as Galison (1997) and Kaiser (2005). For example: Kaiser, in his monograph about Feynman diagrams and thus also the history of quantum electrodynamics, urges that 'the scientific theory' is not a

[14]Nor is theory the only category, traditionally central to the philosophy of science, to be thus questioned. The notion of a law of nature, or even a law of a specific theory, has also been rejected; e.g. Cartwright (1983), Giere (1995).

useful historiographical category (2005, pp. 377-387). As you might guess, his reason is essentially that these theories are not rigorously defined—prompting the suggestion that approximation techniques, like perturbation series, Feynman diagrams etc., are "all there is to the physics".

Obviously, I cannot here reply to this general philosophico-historical critique of theory; nor even to Kaiser and the other authors about quantum field theories. So I will just say what I consider the main justification for using 'theory' as I have done repeatedly in Sections 2 and 3. This reply will lead us back to the syntactic conception of theories, and so return us to Nagel's proposed analysis of reduction.

My main point is that we should distinguish theory in general, from specific theories. That is: we must distinguish two issues. They are superficially similar. For both have two corresponding parts: for each of them concerns:

(Def): whether 'theory' can be precisely defined, faithfully to its root meaning;

(Use): whether 'theory' is a useful unit or category for the philosophical analysis of science.[15]

But within both (Def) and (Use), we must distinguish two issues: viz. whether 'theory' means:

(Gen): theory in general, or

(Spec): a specific theory.

Obviously, the developments in general philosophy of science just adumbrated concern (Gen). They suggest that the notion of a physical (or more generally: scientific) theory cannot be precisely defined, and-or is not a useful unit or category for analysis. And also obviously: Nagel, by the very act of proposing an analysis of inter-theoretic reduction, rejects these suggestions; (even setting aside how the rest of his account of science deploys the notion of theory). I join him in this, though I have ducked out of justifying the rejection.

But my concern in Sections 2 and 3 has been with (Spec): with some specific theories, viz a handful of quantum field theories. So agreed: I need to address the questions (Def) and (Use) for each such theory—though I do not propose to do so here. Again, this leads back to the question whether we should be instrumentalist about these theories.

Here, I want just to emphasize the evident but important point that this paper's main enterprise does *not* depend on retaining the general notion of theory, i.e. on joining Nagel (and me) in rejecting the recent developments' 'anti-theory' suggestions. That is: one can be sceptical or agnostic about the general issues, i.e. one can say 'No' or 'Maybe' to (Def) and-or (Use) under meaning (Gen)—while confidently saying 'Yes' to (Def) and-or (Use) for a specific physical theory.

For consider: the terms with which one might hope to define the general notion of

[15]Of course (Def) and (Use) are related in various ways. In particular, since precision aids analysis, one expects a 'Yes' to (Def) to count in favour of a 'Yes' to (Use); but since after all, analysis can be illuminating even without all its terms being precisely defined, a 'Yes' to (Def) is not a pre-requisite of a 'Yes' to (Use).

physical or scientific theory are very different from those with which one might hope to define a specific theory. The first group of terms would include logico-linguistic items such as 'sentence', 'model', 'deductive closure', and maybe also more philosophical items such as 'confirmation', 'explanation' and 'domain of application'. But the second group would include items specific to the theory concerned, such as 'energy', 'momentum' and 'Lagrangian'. So one might well think ill of the first group of terms, i.e. be sceptical or agnostic about usefully defining the general notion of theory, while also thinking well of the second group, i.e. while being confident that a specific physical theory can be precisely defined; (though of course for interacting quantum field theories, 'precisely defined' must be taken as logically weaker, i.e. less demanding, than rigorous definition within pure mathematics). Indeed: ever since Section 2, I have often identified a quantum field theory with its Hamiltonian (or Lagrangian), i.e. with a specification of the quantum fields and their postulated interactions. So in this usage of 'theory' for a specific theory, as in (Spec), one might well confidently say 'Yes' to (Def) and-or (Use).[16]

4.2 Universality is multiple realizability

In Section 3.2.3, I introduced 'universality' as jargon for the idea that dissimilar theories might have similar infra-red behaviour: in particular, the same infra-red fixed point. Here I wish to: (i) point out that universality is essentially the familiar philosophical idea of multiple realizability, and (ii) make two ancillary comments.

As to (i), I fortunately do not need to worry about the exact definition of multiple realizability: i.e. about whether multiple realizability requires more than a property's being disjunctive, for example by the disjunction being sufficiently heterogeneous (according to some, maybe vague, standard), or by the property being functional (i.e. quantifying over properties). For the examples usually given of dissimilar theories having similar infra-red behaviour will undoubtedly count as examples of multiple realizability: the theories are strikingly dissimilar, and the infra-red behaviour strikingly similar.

In fact, the examples usually given are from statistical mechanics, rather than quantum field theory. As mentioned in Section 3.2.3, they concern continuous phase

[16] This claim was in play in the differing attitudes of Feynman and Dyson during the early 1950s to quantum electrodynamics (QED), in the light of its bad behaviour at high energies. Roughly speaking, Feynman is less demanding. He held that the results and calculational techniques of perturbative QED, including the eponymous diagrams, are so fruitful and accurate that one should "run with it". (He of course would not care whether we should label this body of doctrine with the honorific word 'theory'.) Dyson, trained as a pure mathematician, was more demanding. He was disheartened by the apparent impossibility of a rigorous definition of the theory. (For the history of this disagreement, cf. Schweber (1994, pp. 564-572) and Kaiser (2005, pp. 175-195, 246-248, 358), who calls it 'the Feynman-Dyson split'.) Thus my claim puts me with Feynman: and I am happy to call the body of doctrine a 'theory', and so to disagree with Kaiser's conclusion (ibid., pp. 377-387) that there is no theory hereabouts.

transitions. The dissimilar theories are of utterly different quantities in utterly different systems. For example, one such quantity is the difference of the densities of water and steam, in a system that is a mixture of water and steam; another quantity is the density difference, in a mixture of two phases of liquid helium; a third is the magnetization of a piece of iron or nickel. Despite these systems being so disparate, they show some similar infra-red behaviour. Besides, this behaviour is striking because it is quantitative, exact and concerns an arcane quantity. Namely, it concerns the values of the exponents (called 'critical exponents') in the power laws governing these quantities' values at temperatures close to that at which the continuous phase transition occurs.[17]

So much by way of illustrating universality, and its being multiple realizability. Turning to my two ancillary comments: the first is in effect a note of caution to philosophers, about the success of Section 3.2's renormalization group framework in explaining such universality: in particular, its correctly predicting the exponents in these power laws for countless such systems. This success is much celebrated, including in the philosophical literature about phase transitions; and rightly so.

But beware: this praise can give the impression that only with the renormalization group did physics cotton on to the broad idea of:

(a) starting with a description of a system including details about short distances (equivalently, as in Section 3.1.2: high energies); and then

(b) systematically dropping information from the description (often called 'coarse-graining'), so as to get from a 'microscopic' description to a 'macroscopic' (long distance, low energy) one.

That is *not so.* Coarse-graining a microscopic description to get a macroscopic one has of course long been endemic in physics. The reasons are obvious—the microscopic description is often intractably complicated, involving a vast number of degrees of freedom; and the strategies are familiar—e.g. partitioning the state-space, especially by defining collective variables.

Besides: even for the striking examples above, viz. the universality of critical exponents, some approaches to calculating these exponents, that were developed long before the renormalization group, had considerable, albeit partial, success. That is of course unsurprising, since the idea of coarse-graining is so natural. But it is worth emphasizing, to avoid philosophers getting the impression that only with the renormalization group did physics pick up on the idea of multiple realizability.[18]

[17]More specifically: such a transition occurs only under specific conditions, in particular at a specific temperature, the *critical temperature* T_c (which is *not* itself universal). Close to this temperature and other conditions, the value of some quantity, $v(Q)$ say, is given by a power of the difference between the actual temperature T and T_c: $v(Q) \sim |T - T_c|^p$, where p is the power (also known as: exponent). Here, p might be positive, so that Q's value is zero at the transition: this occurs for the density differences and magnetization I mentioned, for which p is about 0.35 (and is called β). But for other sets of (again, mutually disparate) quantities Q, their shared power law has a negative exponent p, so that the value of each quantity Q diverges at the phase transition.

[18]I think this false impression is fostered by some philosophers' praise of the renormalization group;

My second comment is a brief Nagelian one. Namely: I think that multiple realizability, and thus universality, does not cause problems for Nagelian reduction. Obviously, that is good news for me, since I have endorsed a Nagelian account of reduction (Section 1.2; and 2011a, Sections 3.1.1 and 4.1.1). I will not go into detail since my reasons derive from Sober (1999); and anyway, I have developed them elsewhere (2011a, Section 4.1).[19]

4.3 Renormalizability deduced at low energies as a family of Nagelian reductions

Taking the last two Subsections' pro-Nagelian conclusions in my stride, I turn to my main claim, announced in Section 2.1: that the modern understanding of how renormalizability becomes generic ('emerges') as we consider theories at lower and lower energies amounts to a conceptually unified family of Nagelian approximative reductions.

The idea is clear from the details of Section 3.2.1. A renormalization scheme that defines a flow to lower energies (equivalently: long distances) amounts to a set of bridge-laws that enable a deduction *à la* Nagel, from a theory describing high energy (short distance) physics, of a low energy (long distance) theory. The fact that contributions to physical predictions from non-renormalizable interactions dwindle as we consider lower and lower energies, means that only renormalizable terms are significant at low energies—and become more significant as the energy gets lower. So we have Nagelian approximative reduction. And my Morals (Deduce) and (Before) of Section 1.1 are both illustrated, with the emergent behaviour being renormalizability, and the parameter N being the distance L (or reciprocal of the energy μ). Furthermore, because the same renormalization scheme shows how many high energy theories flow to corresponding low energy theories, we have, not just a set, but a conceptually unified family, of Nagelian reductions.

To sum up, I claim:

> The deduction from a given theory T_1 that describes (perhaps using non-

e.g. Batterman (2002, pp. 37-44), Morrison (2012, p. 156, p.160 (both paragraph 2)). Incidentally, the two main previous approaches to calculating critical exponents are mean field theory and Landau theory. For a glimpse of what these are and their predictive limitations, in the context of condensed matter, cf. e.g. Kadanoff (2009, Sections 2,3; 2013, Sections 1.2.4-4, pp. 147-164) and Binney et al. (1992, pp. 22, 176-177, 183-184). For example, mean field theory implies that the value of p, in the previous footnote, is 0.5, as against the actual 0.35.

[19] To summarize: Sober mostly targets Putnam's (1975) and Fodor's (1974) claims that multiple realizability prevents reduction, especially in the philosophy of mind: claims which Kim endorsed and developed into a rival account of reduction (1999, p. 134; 2005, pp. 99-100; 2006, p. 552). For critiques of Kim, cf. e.g. Marras (2002, pp. 235-237, 240-247) and Needham (2009, pp. 104-108). Within philosophy of physics, perhaps the most extended claims that multiple realizability causes trouble for Nagelian reduction are by Batterman, especially in his earlier work. But I postpone discussing him until Section 5.1.

> renormalizable terms) high-energy physics, of an approximately renormalizable theory T_2 describing low energy physics, is a Nagelian reduction. Besides: for different pairs of theories T_1 and T_2, varying across the space of quantum field theories, the reductive relation is similar, thanks to a shared definition of the renormalization group flow, i.e. of the renormalization scheme.

I will fill this out with five short remarks, rehearsing previous material. As regards the philosophical assessment of Nagelian reduction, the most important of these remarks are (3), about approximate reduction, and (4), about unity among a family of reductions.

(1): I have specified a theory by a Hamiltonian or Lagrangian. Recall the vector of physical coupling constants $g_1(\mu), ..., g_N(\mu)$ which I first introduced in Section 3.1.4, and took as a point in a space of theories at the start of Section 3.2. Recall also my defence of this notion of a specific theory (as against the notion of a theory in general) in Section 4.1.

(2): A renormalization scheme that defines a flow towards lower energies (a scheme for coarse-graining so as to eliminate higher-energy degrees of freedom) amounts to the set of definitions or bridge laws ((i) of Section 1.2) needed to make the deduction of T_2 from T_1 go through.

(3): Since at low energies any non-renormalizable terms in T_2 still make non-zero, albeit dwindling, contributions to the theory's predictions (probabilities for various processes), we have here an approximative reduction ((ii) of Section 1.2); though the approximation gets better and better as the energy decreases.

(4): A given renormalization scheme (definition of the flow) works to show that many theories T_1 lead to approximately renormalizable low-energy theories T_2. This unity is striking. Hence this Section's title's use of the word 'family': since family' connotes resemblance, which 'set' does not.

(5): Agreed: no single renormalization scheme works to prove that all possible theories have dwindling non-renormalizable contributions in the infra-red. And as I have admitted: the proofs concerned are often not mathematically rigorous. But the various renormalization schemes that have been devised do not contradict one another, and in fact mesh in various (often complicated) ways. So it is fair to say that a generic quantum field theory has dwindling non-renormalizable contributions in the infra-red.

So to sum up: the modern approach to renormalization gives a stunning case of explaining something that is, on the traditional approach, a coincidence. The coincidence is that the theory in question, e.g. quantum electrodynamics, has a feature, viz. renormalizability, that seems crucial to it "making sense". This feature turns out to hold for the whole standard model of particle physics, which combines quantum electrodynamics with quantum theories of the weak and strong forces. Thus the coincidence is so large and important that it can seem like manna from heaven; or more prosaically, it can seem that renormalizability is in some way an *a priori* selection principle for quantum field theories. But adopting the modern approach, we can deduce that what seemed manna from heaven is in a sense to be expected.

A final Moral. As I mentioned in Section 1.1, I think philosophers should take note, not just of this specific achievement, but of the general idea of a *space of theories*. This fosters a novel and more ambitious kind of explanatory project than the familiar ones of explaining an individual event, or a single law, or a theory as a part of, or a good approximation to, another. Namely: to explain a feature of a whole class of theories in a unified way in terms of the structure of the space of theories.[20]

5 Ongoing controversies: effective field theories

My claims in Sections 4.2 and 4.3 have a controversial edge to them. For some have argued that the modern approach to renormalization prompts (i) anti-Nagelian morals, and more broadly (ii) anti-reductionist morals. So in this final Section, I will reply briefly to some of these claims. I begin with Batterman, who is perhaps the best known of the authors concerned (Section 5.1). Then I introduce the idea of *effective field theories*: an idea which the modern approach to renormalization has fostered, and on which the anti-Nagelian and anti-reductionist morals of Bain, and of Cao & Schweber, are based. So I first expound the idea (Section 5.2) and then reply to these authors (Section 5.3).

5.1 Batterman: singular limits?

Batterman has long argued that Nagel's account of reduction (more generally: inter-theoretic relations) does not fit explanations using the renormalization group (cf. especially his 2002, 2010, 2011). But I will be very brief about his views, for two (admittedly partial) reasons. First, he focusses on the explanation of critical exponents in continuous phase transitions (cf. Section 4.2), and thus on the renormalization group in statistical mechanics, rather than in quantum field theories for sub-atomic physics. Second, I have already elsewhere given a Nagelian reply (2011, Section 7; 2011a Sections 3 and 4.1; Bouatta and Butterfield 2011).

Agreed: as so often in academic controversy, peace has not yet broken out! So here is a bit more detail, to fill out Section 4.2's closing comment that multiple realizability is no trouble for Nagel. As mentioned in footnote 19, Kim believes it is, and develops his own account of reduction, which he calls a 'functional model'. It takes reduction to include (a) functional definitions of the higher-level properties P etc. and (b) a lower-level description of the (variety-specific) realizers of P etc., and of how they

[20]Nor is the renormalization group the only example of this idea in physics. Another example is catastrophe theory. Roughly speaking: this takes a theory to be given by a potential function (similarly to it being given here by a Hamiltonian or Lagrangian); the space of potential functions is then endowed with structure such as a topology, in terms of which features of potentials such as their being structurally stable are then explained.

fulfill the functional roles spelt out in (a). Batterman agrees with Kim that multiple realizability spells trouble for Nagel, and that Kim's model is not Nagelian. But he rejects Kim's model because it does not sufficiently recognize features common across different varieties of realizer, and so does not help explain the relative autonomy of the "higher level". He even suggests programmatically that the understanding of universality provided by the renormalisation group (and so *contra* my footnote 18's praise of mean field theory and Landau theory) could help us recognize such common features and explain the autonomy of the higher level—even for properties far removed from physics, such as pain. (For details of these views, cf. Batterman (2002, pp. 65, 67, 70-72, 73-75 respectively).)

Of course, there is a lot more to Batterman's views; and besides, they have changed over time. Both these points are shown by his (2010). Its example of multiple realizability is Gibbs' discussion of how in his two main frameworks for statistical mechanics (called 'the canonical ensemble' and 'the micro-canonical ensemble'), different quantities are the analogues, as the number N of constituents in the system goes to infinity (the thermodynamic limit), of the thermodynamic quantities, entropy and temperature. Batterman expounds this example in relation both to Gibbs himself, and to the connection between the renormalization group and probability theory.[21]

As regards Nagelian reduction, Batterman uses this example to make two main points. The first is a peace-pipe for Nagel; the second, about singular limits, less so. First, he agrees that where the limit—$N \to \infty$ in our notation—is not singular, the Nagelian 'conception of derivational reduction will likely hold ... because we can take the limiting relations as providing us with something like the bridge laws appropriate for Nagel-like reduction' (2010, p. 166); and he adds that expressing these relations will typically require mathematical operations, not the universally quantified biconditionals of philosophical discussions. This peace-pipe for Nagel is offered again, with the details of the Gibbsian example of multiple realizability, later on (p. 174, paragraphs 3 and 4).

I of course endorse this quotation, since I think the Nagelian can and should admit multiple realizability and the need to go beyond biconditionals: recall that for me and my fellow Nagelians, there is no requirement for a formal language (cf. (1) of Section 1.2).

Second, Batterman emphasizes that the limit is often mathematically singular;[22] and in such cases, he contends, 'it is best to give up talk of 'reduction' altogether and

[21]To see the idea of this connection, recall that the central limit theorem says, roughly speaking, that the sample averages obtained by independent sampling tend to a normal distribution as the sample size N increases; and this is so for independent sampling of any of a wide class of probability distributions. This suggests that increasing N defines a flow in the space of distributions under which any distribution in the class flows to the normal distribution. So in the jargon of Section 3.2.3: the class is a basin of attraction, and the normal distribution is a fixed point. Cf. also Batterman (2013, pp. 275-278).

[22]As he had in previous work; and as do his kindred spirits, Berry and Kadanoff, cited in Section 1.1.

to speak instead of 'intertheoretic relations'. In this paper, Batterman's example of such a singular limit is his favourite one: continuous phase transitions, also known as critical phenomena. (I think his best exposition of this example is in his (2011, Sections 3 and 5).)

The topic of singular limits is a large one, which I cannot take up; but I will make two comments.

(1): Batterman softens this apparently anti-Nagelian point by saying that the singularities concerned 'are not genuine obstacles to some kind of general limiting (reductive?) relation between the theories after all'; and adding that in this regard, he has changed his view (p. 176, and footnote 9 respectively).

(2): Whether a limit is singular can be a subtle matter. It is not just that there are various mathematical types of singularity. Also, a physical phenomenon or set of phenomena can be modelled both by a formalism with a singular limit, and by one with a continuous limit. And in some such cases—cases that are striking and so rightly emphasized by Batterman and others—the second formalism with a continuous limit is at least as adequate and rigorous as the first. In short: philosophers should beware of loose talk that limits are singular.[23]

5.2 Effective field theories

I turn to explaining how the modern approach to renormalization has fostered the idea of *effective field theories*. The reason goes back to Section 3.2.1's explanation of non-renormalizable contributions dwindling at long distances. I will first emphasize how that explanation does not depend on spacetime being a continuum (Section 5.2.1); and then describe how this suggests (though it does not imply!) a 'tower' of merely effective theories (Section 5.2.2).

5.2.1 Spacetime might not be a continuum

Recall Section 3.2.1's explanation of non-renormalizable contributions dwindling at long distances, using the scaling equation eq. 3.3: which, to repeat it, was

$$g(L)/g_0 \sim (L/d)^{-D} \ . \tag{5.1}$$

It is clear that this explanation does not depend on our theory (with all its terms, including non-renormalizable ones) being true, or even approximately true, at arbitrarily short distances. Our theory only needs to be approximately true at suitable intermediate distances. More precisely: it only needs to secure eq. 5.1 holding for any non-renormalizable interaction at a range of scales which is wide enough to include L

[23]Cases where rigorous algebraic quantum theory gives a *continuous* limit include: (i) spontaneous symmetry breaking, both in the $N \to \infty$ limit of quantum statistical mechanics, and in the $\hbar \to 0$ limit of wave mechanics (Landsman 2013, Section 3); and (ii) the emergence as $N \to \infty$ of superselection (Landsman 2007, Section 6.1-6.7, my 2011, Section 6)).

being sufficiently larger than the cut-off d, so that with the given positive dimension D of the bare coupling constant, the left hand side of eq. 5.1 is small enough that we will not see the interaction. (That is, as I put it in Section 3.2.1.: the right hand side of eq. 5.1 is small enough to make the left hand side small enough that "you will not see it".)

We can put the same point in more physical terms, and in terms of energies. Maybe at very high energies, spacetime does not behave like a continuum. But provided the theory is "true enough" at some high, maybe even inaccessible, energies in the sense that it validates eq. 5.1, then we can deduce that at much lower, in particular accessible, energies, "we see only renormalizable interactions". That is: our theory's predictions have significant contributions only from renormalizable interactions.

In short: we can be agnostic about whether—indeed, we can even deny that—our theory describes physics in a continuous spacetime. All we need is that it is approximately true at suitable intermediate distances, in the sense just specified.

Here we meet a widespread jargon. A theory that is taken to be approximately true in a given regime (of energy and-or length, and-or some other parameters) is called *effective*. The adjective is used especially when the theory is known or believed to be *only* approximately true, because it is derived from a theory with better epistemic warrant, by adopting certain approximating and-or idealizing assumptions (assumptions which go beyond merely specifying the regime, i.e. range of parameters, concerned).

So we can sum up Section 3.2.1's explanation of what I called 'our good fortune' by saying: from studying the renormalization group flow, we deduce (subject to the substantive assumptions gestured at in Section 3.2.2!) that effective low-energy theories are renormalizable. This leads in to the next point.

5.2.2 Effective theories only?

Section 3.2.1's explanation prompts the rhetorical question: why worry about non-renormalizable terms? Although they induce bad behaviour, i.e. a large coupling, at short distances, this bad behavour is invisible at the larger distances we can access. So why not countenance non-renormalizable terms, at least for inaccessibly high energies?[24]

Of course, the words 'worry' and 'countenance' are vague. What you are inclined to worry about, and correspondingly what you are willing to countenance, will depend on your background attitudes to quantum field theories: for example, on how confident you are about using them at high energies, and about accepting results obtained from a heuristic formalism, rather than by rigorous mathematical proofs. So there are bound to be several possible positions. Here I will develop one position, often called the *effective field theory programme* (or: approach). It is based, not on

[24]Note the contrast with the idea, on the traditional approach to renormalization, that renormalizability is a necessary condition for a theory to be acceptable; cf. Section 3.1.4.

confidence about the two topics above, but on an opportunistic or instrumentalist attitude to being *unconfident* about them.[25]

There are of course two main factors that prompt a cautious or sceptical attitude towards the framework of quantum field theory.

(1): One is just that interacting quantum field theories (in four spacetime dimensions) are at present mathematically ill-defined (Section 2.2).

(2): The other factor is the expectation that at sufficiently high energies, the framework breaks down, to be replaced by a theory or theories using a different framework. This break-down might occur only at the vast energies associated with quantum gravity: the replacement theory being perhaps a version of string theory, or some other current contender for a theory of quantum gravity. Or the break-down might occur at intermediate energies, energies far higher than we can (and probably: ever will) access, but well below those of quantum gravity: there are proposals for new frameworks at these energies, such as non-commutative geometry.

Either or both of these factors prompt one to be cautious about drawing from quantum field theory conclusions about ontology. Or rather: conclusions about the ontology of phenomena at very high energies, or very short distances. But these factors should *not* suspend all discussion of ontology in the light of physics, or even in the light of quantum field theory; for four reasons.

(a): Whatever the phenomena at very high energies turn out to be, whatever the theoretical framework for describing them, and whatever ontology that framework suggests, we have every reason to expect that the facts at those energies determine, i.e. subvene, the facts at the lower energies we can access.

(b): And given the great success of quantum field theory, we have every reason to expect that the facts at those very high energies imply a quantum field theoretic description at the lower, accessible, energies.

(c): This last point can be strengthened. Recall from Section 2.2 Weinberg's argument that physics at accessible energies must be described by a quantum field theory, *even if* the framework breaks down higher up, e.g. because of gravity. In short: any quantum theory that at low enough energies obeys special relativity and satisfies cluster decomposition (which is plausible, since it has the flavour of a locality assumption), must be a quantum field theory.

(d): Besides, whoever said that ontology concerns only "the supervenience basis", i.e. the putative set or level of facts that determine (subvene) all other facts? That is: there is plenty of scope for ontological discussion of supervening ("higher level") facts and theories: in particular, there is scope for ontological discussion of quantum field theory.

But these two factors also suggest that even below the energy scale at which the entire framework of quantum field theory breaks down, there may, for all we know,

[25] Recalling Section 2.3, we can already glimpse why several positions are possible. For there I cited results showing a realistic quantum field theory's (viz. QCD's) good behaviour at arbitrarily short distances, and thus better prospects for being rigorously definable.

not be any single quantum field theory which is more fundamental than the others, in the sense that each of them is derived from it by assuming extra conditions that specify the derived theory's regime (of energies and types of interaction considered etc.). That is: as the energy scale gets higher and higher (while remaining below the scale at which the entire framework of quantum field theory breaks down), physics could, for all we know, be described by a succession of quantum field theories, each of which accurately describes the phenomena at a certain range of energies, but becomes inaccurate above that range. And when it becomes inaccurate, it may also become even more badly behaved, mathematically.

This scenario is often called the *tower of effective field theories*. But the phrase can be misleading, for two complementary reasons.

(i): First, as I mentioned when defining 'effective', at the end of Section 5.2.1: the adjective is often used when the theory is known or believed to be only approximately true, because it is derived from another theory with better warrant, by adopting approximating and-or idealizing assumptions. But note: in this scenario, the theories in the envisaged tower are *not* required to be derivable from some other theory: in particular, one with better warrant for being taken as exactly true ('fundamental'), because it also covers higher energies. Rather, each theory is simply accurate in its energy range, and inaccurate beyond it.

(ii): Second: the word 'tower' suggests an infinite tower. But as I noted in (2) above, there are good reasons (concerning quantum gravity, if nothing else) to think that at *some* energy, the entire framework of quantum field theory breaks down. This implies that, considered as a programme or approach for quantum field theory, the effective field theory programme can, and should, take it that the tower is probably finite.

But setting aside misleading connotations: the main point here is that this scenario gets some support from Section 3.2.1's explanation of "our good fortune", viz. that any non-renormalizable interactions (terms), though they would be important at higher energies, will make a dwindling contribution to all processes, as the energy scale is reduced. For this explanation implies that we cannot get evidence about which non-renormalizable interactions, if any, operate at inaccessibly high energies. Whatever they are—and whatever bad short-distance behaviour they suffer (eq. 5.1, with $L << d, D > 0$ so that both sides of the equation are large)—we will not see them. So why worry about non-renormalizable interactions (terms)? Thus for all we know, or could ever know, the scenario of the tower holds good: there is no fundamental quantum field theory, and various non-renormalizable interactions operate at various inaccessibly high energies.

So much by way of sketching the effective field theory programme. We can sum it up as urging that, regardless of how and why quantum field theory might break down at very high energies: we have no reason in theory, nor experimental data, to deny the scenario of the tower—a succession of theories, each accurately describing physics in its energy range, and inaccurate beyond it.

5.3 Bain, Cao and Schweber: against reductionism

What should we make of the effective field theory programme, from a philosophical viewpoint? Of course, I must postpone a detailed discussion to another occasion. But I shall make two obvious, yet important, points. They also set the scene for brief replies to discussions by Bain, Cao and Schweber which urge, albeit in different ways, that effective field theories give evidence against "reductionism".

(1) First: the vision of the tower of effective theories bears on the broad, and maybe perennial, debate between instrumentalist and realist views of physical theories, and of science in general. For the vision is that for all we know, there is a sequence of quantum field theories, each accurate within, but not beyond, its own energy-range, that are *not* each derivable from some single theory (viz. as an approximation or limit describing that range). This 'for all we know' is reminiscent of the instrumentalist's leading idea of the under-determination of theory by data. All the more so, when we note that in the present state of knowledge, no one—not even an expert in effective field theories—can show that there is at most *one* such sequence: there could be several towers, even with some of them sharing some 'floors' in common.

(2) Second: whatever we could eventually come to believe (whether on present evidence and much reflection, or on the basis of much more evidence, even evidence about inaccessibly high energies that we surely cannot get) about there being such a tower of theories, and whatever the upshot of the broad debate between instrumentalist and realist views—my main claim (Sections 2.1, 3.2.1, and 4.3) *is unaffected.* For we must distinguish 'reductionism' in its various possible senses, from the claim that Nagelian reduction is illustrated in some, or even many, cases. Agreed: some more precise versions of the idea of the tower of theories would, if true, be a counterexample—and a striking and important one—to various precise senses of 'reductionism'. But I have of course not defended any such sense; but only that the 'flow to the infra-red' illustrates Nagelian reduction (in a broad sense: Section 1.2).

Indeed, *au contraire*: it seems that in general, the tower of theories is good news, rather than bad, for my main claim. For it promises more illustrations of Nagelian reduction than my previous defence (Section 4.3) described. For at each 'floor' of the tower, there is an effective field theory accurately describing an energy-range, so that one can envisage the description it gives of the lower end of its energy-range being reduced *à la* Nagel by a renormalization scheme (flow to the infra-red) being applied to its description of the upper end of its energy-range.

With these two points in hand, I turn to reply to: (i) Bain (2013, 2013a) and (ii) Cao and Schweber (1993). My reply to Bain will be very brief, since the focus is sharp: we plainly disagree about my main claim. Cao and Schweber have larger and different concerns: they discuss reductionism, and several other 'isms', but do not mention Nagelian reduction. I will engage a little with these 'isms', so that my reply to them will be longer. But for both (i) and (ii), my main emphasis will lie in

(2) above: that my main claim is unaffected by the rise of the effective field theory programme.

5.3.1 Bain Bain (2013) reviews effective field theories, first technically (Sections 2 to 4) and then in relation to reduction and emergence (Sections 5 and 6). Similarly, in his (2013a) he first reviews effective field theories technically and interpretively (Sections 2 and 3), and then discusses emergence. The two papers are similar, and I will concentrate on (2013). Even so, there are many details I cannot address.[26] I will only register my disagreement with Bain's statements:

> [Because] the steps involved in the construction of an effective field theory typically involve approximations and heuristic reasoning ... [and] identifying the high-level variables ... it will be difficult, if not impossible, to reformulate the steps involved in the construction of an effective field theory ... in the form of a derivation (2013, Section 6.1 end, p. 246-247).

and

> The dynamical laws of an effective field theory and its high-energy theory are different, and a difference in dynamical laws entails a difference in theorems derived from those laws. Thus an effective field theory is not a sub-theory of its high-energy theory; hence one cannot say that an effective field theory reduces to its high-energy theory, on this [Nagelian] view of reduction (2013, Section 6.2(a), p. 247).

(There are similar statements in his (2013a): e.g. p. 262 (a) (b) and note 7, p. 264 paragraph 3, p. 266 paragraph 2.)

The reasons for my disagreement are obvious from Section 1.2's account of broad Nagelian reduction. As to the first statement: (1) of Section 1.2 emphasized that there is no requirement of formalization. And I submit that according to the standards of informal mathematical or physical reasoning, many a specification of a renormalization scheme, and implementation of it so as to define a flow in the space of theories, would count as a derivation. In particular, its reliance on approximations and heuristics, and even on insight or creativity to identify the correct variables to manipulate, does not prevent its being a derivation.[27]

As to the second statement: (ii) of Section 1.2 emphasized Nagel's idea of approximative reduction (which is also emphasized in Schaffner's GRR account). That

[26] One pertinent example is that Bain (2013 Section 6.3) asks whether effective field theories illustrate Batterman's proposal (cf. Section 5.1) that reduction fails when the relevant mathematical limit is singular. On the other hand, since Bain's Sections 5 and 6 also discuss Cao and Schweber and their critics, my Section 5.3.2 below will implicitly engage with some of what he says. In any case: my thanks to Bain for correspondence; indeed, I believe we now agree.

[27] At the end of Section 5.3.2 below, this last point, that Nagelian reduction of course allows that formulating a bridge law can require insight or creativity, will recur.

is: what is deducible from T_1 (here, the high-energy theory) may not be exactly T_2 (here, the effective theory), but only some part, or close analogue, of it. The standard example is that you cannot deduce Galileo's law of free fall (that acceleration during free fall is constant) from Newton's theory, which says that the acceleration increases slightly as the body gets closer to the centre of the Earth and so feels a stronger gravitational force. Thus, since Newton's theory surely does reduce Galileo's law, Nagel says that reduction only requires deduction within error bars; or in more positive words, deduction of a corrected version of T_2. Similarly, say I, for effective field theories: i.e. for the dwindling contribution of non-renormalizable terms at long distances/low energies.

5.3.2 Cao and Schweber As I mentioned in footnote 11: Cao and Schweber (1993) is a survey of renormalization in quantum field theory. Much of it surveys the history; (it draws on Cao (1993) and Schweber (1993), in a contemporaneous anthology, Brown (1993), emphasizing history). But this historical survey has a philosophical sting in the tail. Cao and Schweber begin their thirty-page discussion of 'philosophical ramifications' (1993, Section 4, pp. 69-90) by saying that the modern approach to renormalization, and effective field theories, imply

> a pluralism in theoretical ontology, an anti-foundationalism in epistemology, and an anti-reductionism in methodology ... in sharp contrast with the neo-Platonism implicit in the traditional pursuit of quantum field theorists, which ... assumed that, through rational (mainly mathematical) human activities, one could arrive at an ultimate stable theory of everything (p. 69).

They then develop the three 'isms' listed, in subsequent subsections (e.g. Section 4.1 on pluralism). The discussion is wide-ranging: for example, it touches on the theory of meaning for scientific terms (p. 75) and the philosophy of pure mathematics (p. 81-83). It is also vigorous, indeed broad-brush. For example: (i) about their own position:

> The empiricist position in epistemology that is supported by the recent developments in renormalization theory is characterized by its anti-essentialism and its anti-foundationalism, its rejection of a fixed underlying natural ontology expressed by mathematical entities, and its denial of universal, purely mathematical truths in the physical world (p. 77).

and (ii) against the opposition:

> The latest example of such an overly grandiose and totalizing conception of physical theory is the search for the theory of everything by superstring theorists (p. 74).

Fighting talk! Admittedly, as I said in (2): some precise versions of the tower of theories would, if true, be a counterexample to various precise senses of 'reductionism'. All parties can agree to that. The task must be to state such precise versions and senses, and to assess whether the technical scientific developments instantiate them. As I said, I myself must postpone that to another occasion. Here it suffices to make three points. The first is critical of Cao and Schweber. The second is a *caveat*, and leads into the third, which returns to my main claim.

First: Unsurprisingly, Cao and Schweber's controversial claims have attracted criticism. I shall consider three main replies: Huggett and Weingard (1995, 172 and 187-189), Hartmann (2001, Section 4.2, 297-299) and Castellani (2002, 263-265).[28] Among Cao and Schweber's three 'isms', they focus on pluralism and anti-reductionism. As I read them,[29] their main reply is that in the present state of knowledge, we have no compelling reason, even for energies for which we can be confident of the quantum field theory framework (and so: independently of considerations of quantum gravity), to believe in what in (1), at the start of this Section, I called the 'vision': viz. a tower of theories that are *not* each derivable from some single theory, as an approximation describing physics at its own energy range. Thus Castellani sums up:

> The fact that current quantum field theories are now seen as effective field theories does not imply any specific thesis about the existence of a final theory ... The effective field theory approach does not imply that the idea of a theory being more fundamental than another is meaningless (2002, p. 264-265).

I of course agree.

Second, a *caveat*: Several passages in Cao and Schweber appear to admit this point, i.e. what I just called these critics' 'main reply'. Thus it is not really clear what Cao and Schweber believe the development of physics (up to twenty years ago, i.e. 1993) *implies* or at least *supports* (their words; p. 69), by way of 'philosophical ramifications'—as against what it merely suggests. Some examples:

(i): They concede that derivation of our successful theories (i.e. the standard model) from some single (indeed: renormalizable and unified) theory describing higher energies is possible; (p. 65, start of (i)). Similarly, in some of their other contemporaneous work. Thus Schweber writes:

> [One can take] the viewpoint that quantum field theory will ultimately yield a fundamental theory. If a complete renormalizable theory at infinitely short distances were available, one would be able to work one's

[28]Other philosophical discussions of effective field theories include Weinberg (1999, pp. 246-250), Redhead (1999, pp. 38-40) and Cao (1997, Section 11.4 pp. 339-352).

[29]I say 'as I read them' since I am summarizing disparate discussions. For example, Huggett and Weingard distinguish different strengths of the EFT approach (of course placing Cao and Schweber among the stronger versions). Hartmann addresses the three isms *seriatim* in the light of his fine preceding surveys of renormalization in general (his Section 2.2-2.3) and of the interplay between theories, models and effective field theories, in particular in QCD (his Section 3).

> way up [i.e. to longer distances, lower energies: JNB] to the effective theory at any larger distance in a totally systematic way by integrating out the heavy fields of the theory (1993, p. 154).

(ii) They concede that their opponents, i.e. believers in such a single or fundamental theory, have tenable replies to some of their arguments. For example: their question why renormalization as a topic makes for such fruitful exchanges between quantum field theory and statistical physics (pp. 72-73) can be answered in a 'realist-essentialist' way by appealing to the 'unity of physical phenomena on the ontological plane, and/or the universality of physical truths on the epistemological plane' ((p. 73).

Third: In one of their more precise statements (under 'pluralism in ontology', their Section 4.1), Cao and Schweber take as distinctive of their own view (and so as rejected by 'monist' or 'reductionist' opponents) the claim that it is impossible

> to infer the complexity and novelty that emerge at the lower energy scales from the simplicity at higher energy scales, *without any empirical input* [their emphasis]. The necessity, as required by ... EFT, of an empirical input into the theoretical ontologies applicable at lower energy scales is fostering [a picture in which the energy scales are] layered into quasi-autonomous domains, each layer having its own ontology and associated "fundamental" laws (p. 72).

But I think most of us, even many self-styled monists and reductionists, would happily agree (a) that empirical input is needed; and (b), concerning the second half of the quotation, that the lower energy scales, or more generally special sciences ('layers'), are largely autonomous, with their own distinctive concepts, laws and methods.[30]

The point here is of course wider (and thankfully, more elementary!) than discussions of quantum field theory. One sees it in basic discussions of Nagelian bridge laws. To formulate the bridge laws that enable an inference of the reduced theory ('the complexity and novelty') from the reducing ('the simplicity'), one of course needs empirical input—namely, to suggest what in the former might be associated with what in the latter. And that need remains, even if: (a) after the formulation and then successful reduction, the bridge law is glossed as a definition in logicians' sense (e.g. for a predicate: a universally quantified biconditional); and-or (b) over time, terms' meanings change so that the bridge law comes to seem analytic or a matter of convention.

Besides, as many authors stress: one often needs, not just empirical input, but high scientific creativity. Examples are legion. A standard example is Maxwell's formulation of a bridge law identifying electromagnetic waves with visible light. He was led to it by deducing from his electromagnetic theory the speed of the waves,

[30] Thus this quotation provides another passage exemplifying my *caveat* just above: it is not really clear to me what Cao and Schweber intend.

and noticing that it matched the measured speed of light. Without that empirical input, even the genius of Maxwell might not have formulated the bridge law. Another standard example is formulating which statistical mechanical quantities to associate with thermodynamic quantities (as discussed by Nagel himself, and Batterman cf. Section 5.1): again, empirical input and scientific creativity are required in order to bring the two theories into consistent contact, and *a fortiori*, to deduce the one from the other. Yet another standard example is elementary chemistry. Without empirical input and creativity, even the genius of Schrödinger (or indeed, the entire team of founding fathers of quantum mechanics!) could surely not have inferred from the many-body Schrödinger equation (with, say, the nucleus treated as fixed, and including spin terms) the existence of the elements, let alone some of their chemical behaviour, such as the structure of the periodic table.

This need for empirical input and for creativity is, I take it, uncontroversial. And applied to our topic of effective field theories, it is well made by Castellani in her summing up:

> The effective field theory approach does not imply anti-reductionism ...the effective field theory schema, by allowing definite connections between theory levels, actually provides an argument *against* [anti-reductionism]. A reconstruction (the way up) [i.e. derivation of lower energy behaviour: JNB] is not excluded, even though it may have to be only in principle (2002, p. 265).[31]

In any case: controversial or not, this point returns us back to my main claim, that a family of renormalization group trajectories flowing to the infra-red ('the way up' in Castellani's and Schweber's jargon) gives a unified family of reductions, in my broad Nagelian sense; (Sections 2.1, 3.2.1, and 4.3). As I emphasized in (2) at the start of this Subsection, this claim is not tied to any form of reductionism; and in particular, not to denying the tower of effective theories. It says 'Here, we find unified families of reductions'; but nothing like 'Here is a single grand reduction of all of particle physics'.

[31] This last sentence echoes Castellani's previous summary (p. 263): 'the effective field theory approach provides a level structure of theories, where the way a theory emerges from another [a notion she has earlier glossed, largely in terms of Nagelian reduction: JNB] is in principle describable by using RG methods'. Again, this returns us to my main claim.

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