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Physics Without Fundamentality

A Study into Anti-Fundamentalism about Particles and Laws in High-Energy Physics

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Abstract

Two assumptions pervade contemporary metaphysics: that there is a fundamental level to reality, and that physics will one day describe it. In the first part of this thesis, I consider whether physics may have a greater role for fundamentality metaphysics than that which it is typically accorded. In particular, I consider whether physics might contribute not just to questions of the content of an assumed fundamental level, but to the existence of such a level itself. I argue that if we are to use physics to do such a thing, it must be through what I call the 'internal' approach, in which fundamentality questions are addressed through the lens of extant physical theory. Through two case studies drawn from particle physics, I show that it is indeed possible to deny fundamentality through this means – or at least, that one may do so as legitimately as one may make other propositions of physicalistic metaphysics.

While this is a non-trivial achievement, the internal approach nevertheless imposes a profound limitation on the sort of fundamentality that we can use physics to deny, in that it precludes the denial of fundamental physical principles. This raises the question of whether such principles ought to be regarded as somehow more fundamental even than particles. I argue that this question is naturally construed as the question of whether we ought to regard the category of dynamical structures as more ontologically fundamental than the category of objects. The claim that structure is ontologically prior to objects is the signature claim of ontic structuralism, and in the second part of this thesis I consider whether it can be defended. I ultimately argue that structuralism can indeed be supported, but that it is only its moderate version that is vindicated.

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Abbreviations

The following abbreviations are used in this thesis.

QFT — quantum field theory.

EFT — effective field theory.

RP — renormalizability principle.

SIPs — strongly-interacting particles.

Chapter 1

Introduction

1.1 Two canonical assumptions of metaphysics

I begin by highlighting two assumptions that are salient in much contemporary metaphysics.[1] The first concerns 'an intuition commonly held by metaphysicians' – namely, the intuition 'that there must be a fundamental layer of reality, i.e., that chains of ontological dependence must terminate: there cannot be turtles all the way down'.[2] As pointed out by Schaffer, this assumption 'pervades contemporary metaphysics'.[3] The 'layers of reality' appealed to here are taken to be related by *ontological priority* relations, with the 'deepest' or most fundamental layer – or, as I will say, *level* – being defined as that which is ontologically prior to all of the others. Since the priority relations that structure the levels hierarchy are standardly assumed to form partial orderings, the fundamentalist intuition may be expressed by saying that priority relations comprise *well-founded* partial orderings.[4] Thus, for

[1]It can also be found in canonical philosophy of science; a classic example is Oppenheim and Putnam [1958], in which it is asserted that 'There must be a unique lowest level' (p409), which they take to be populated by elementary particles.

[2]Cameron [2008], p1. Note that Cameron uses 'ontological dependence' to denote (the converse of) ontological priority; I, however, will take the latter to be a more general relation, whose converse admits ontological dependence as a species but may permit other species as well. All this will, of course, be expanded on below.

[3]Schaffer [2003], p498.

[4]See for example Cameron *op. cit.*; Schaffer likewise 'assume[s] that the priority relations among actual concrete objects form a well-founded partial ordering... Well-foundedness is imposed by requiring that all priority chains terminate' ([2010],

example, if one takes supervenience relations with properties as their relata to constitute ontological priority relations, the intuition is that there must exist a set of properties on which all other properties supervene but that are not themselves supervenient on anything. Or, if it is mereological relations with objects as their relata that constitute the priority relations in question, the intuition is that there must exist a set of objects that compose everything but that do not themselves admit of proper parts.

Worlds in which every object resolves itself into a set of mereologically basic objects may be called 'atomic' worlds, while worlds in which at least some objects contain parts *ad infinitum* are often termed 'gunky' worlds. Underpinning the intuition that the world must be atomic is the 'anti-gunk worry' that, in a gunky world in which every object has proper parts,

> composition could never have got off the ground. If the existence of each complex object depends for its existence on the existence of the complex objects at the level below, and if we never reach a bottom level, then it is hard to see why there are any complex objects at all... In Schaffer's charming phrase, 'Being would be infinitely deferred, never achieved'.[5]

The 'worry' is presumably analogous for any other priority relations that one might identify.

The second salient assumption within metaphysics is that, whether conceived of as populated by objects, properties or laws, the fundamental level is *physical* in nature. The most familiar contemporary proponent of this thought is probably Lewis, who sees it as 'a task of physics to provide an inventory of all the fundamental properties and relations that occur' in an assumed fundamental supervenience basis for this world.[6] The belief that it is the job of physics to fill in the details of this basis is a pervasive one; Kim, for example, observes that 'the bottom level is usually thought to consist of elementary particles, or whatever our best physics is going to tell us are the basic bits of matter out of which all material things are composed'.[7]

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p37).

[5]Cameron [2007], p.6. The word 'gunk' as a term for objects all of whose parts themselves have proper parts was introduced by Lewis in his [1991].

[6]Lewis [1999], p292.

[7]Kim [1998], p15.

The philosophical position outlined here – and which I take to be deeply entrenched – is therefore constituted by two elements: (1) an *a priori intuition* that the structure of priority relations is well-founded – that is, that the actual world possesses a fundamental level – and (2) a *delegation to physics* in settling what that fundamental level is like. But it will be immediately obvious that this state of affairs is deeply dissatisfying from a naturalistic point of view. Under the assumption that the fundamental basis exists, physics gets a role in saying what it is like; *that* the basis exists in the first place, however, is relegated to armchair contemplation. But those, like me, who endorse a *naturalistic* approach to metaphysics will surely want physics to contribute to every aspect of our metaphysics, if at all possible, not for it to be pre-assigned piecemeal roles; we certainly do not want fundamental questions about the structure of reality to be answerable only to our intuitions and hunches about what must be so. The first question I shall be concerned with in this thesis, therefore, is whether physics can contribute to questions not just of the content of an assumed fundamental level, but of the structure of priority itself, and in particular to the question of whether we can use physics to deny that a fundamental level even exists. In a nutshell, I begin by asking: *Can physics deny fundamentality?* This is the question that serves as the springboard for this essay.

1.2 Structure of the thesis

This thesis is comprised of two parts. Part 1, entitled 'Denying the Existence of a Fundamental Level', will focus on the question introduced a moment ago, and thus upon whether we can use physics to deny the existence of a fundamental level. To this end, the labour will be divided as follows. In the next chapter, Chapter 2, I will continue my introductory remarks by defending the idea that it makes sense to conceive of the world as containing a levels structure – something that is obviously presupposed in the question of whether we can deny that there is a *lowest* level to it. With that in place, I will survey in Chapter 3 the major arguments in the contemporary literature for the existence of a fundamental level. After all, only if these arguments are found wanting will there be any point in considering whether physics in particular has any hope of denying that existence. But found wanting they will be, and the discussion of them will lead naturally to a consideration, in Chapter 4, of another argument that is prominent in the contemporary literature, this one put forward by Schaffer. Schaffer's argument is unusual, however, in that it argues *against* the fundamentalist intuition that pervades contemporary metaphysics, and moreover attempts to do so on broadly naturalistic grounds. Nonetheless, I will argue that this argument falls short in its ambitions, and also that it fails to exemplify the naturalistic approach to fundamentality questions that I seek here. Schaffer's argument can, however, be regarded as a useful starting point and a good example of what *not* to do, and as such it offers valuable lessons on how we might tackle the question I am concerned with in this part. In the wake of these insights, I will propose that the best way to attempt to deny the existence of a fundamental level is by adopting (what I will call) the 'internal' approach, in which fundamentality questions are viewed through the lens of an *extant physical theory*. Then, in Chapters 5 and 6, I will demonstrate through two case studies that it *is* indeed possible to use the internal approach in the service of arguing against fundamentality, and thus that we can have good naturalistic grounds to deny a fundamental level. The first of these case studies concerns the so-called Analytic S-matrix theory of the strong interactions, which will be enlisted to show that we can use physical theory to argue against the existence of a level of mereologically fundamental particles. The second case study will concern quantum field theory and the 'effective' interpretation of it, and will argue that this theory provides us – *modulo* certain assumptions – with grounds to deny the existence of a fundamental level of laws.

It will be a significant consequence of the internal strategy adopted in Chapters 5 and 6 that naturalistic arguments against fundamentality are limited in a very important sense, in that certain physical *principles* must always be at least *treated* as fundamental in order for their conclusions to go through. In Part 2 of this thesis, 'Arguing for the Fundamentality of Structure', I will reflect upon some of the fundamentality issues that are raised by this implication. In particular, I will meditate in Chapter 7 on the question of whether we can, or should, regard the sorts of principles that were used to deduce an infinitely descending hierarchy of levels as being, in some sense, more ontologically fundamental than any of the inhabitants of those levels. This discussion will forge rich connections with the ontic structuralist tradition in the philosophy of physics – a tradition that claims to recommend a re-think of other entrenched fundamentality assumptions, complementary to those that were the focus of Part 1. In particular, a version of ontic structuralism demands that we reject the idea that objects comprise a fundamental category and that we introduce *structure* as a category of greater ontological standing. I will therefore consider, in Chapters 8 and 9, whether any of structuralism's revisionary fundamentality claims may be regarded as justified. While structuralism is arguably not concerned with denying that there is a fundamental *level* of particles, versions of it can nevertheless be construed as claiming that *there are no fundamental particles*. As such, the discussion of structuralism in Part 2 will not only help us gain a deeper understanding of the implications of the internal arguments that were the focus of Part 1, but will also offer an interesting twist on the question of anti-fundamentality that was raised in that first part. Chapter 10 is the conclusion.

Before I embark on any of that, however, I would like make a few points of clarification. Firstly, and as already stated, I favour a broadly *naturalistic* approach to metaphysics, and as such I view the latter as largely continuous with physics. While there is, of course, a great deal that one could say about what precisely that means, I hope it will nonetheless be agreed that this thesis maintains a recognisably naturalistic tenor throughout.[8] Secondly, and in accord with the naturalistic approach, I want questions about the structure of priority relations to be settled, as far as they can be, by the relevant phyics, and thus I will not begin with prior assumptions about the logical form of ontological priority relations. In particular, I will not assume at the outset that priority relations form partial orders. I therefore apologize that the term 'priority' is awkward if we do not, as *I* do not, simply presume that it is an asymmetric relation, given that the very word itself connotes asymmetry. However, rather than invent a term that is somehow neutral between the various ways of spelling out priority and that does not carry such asymmetric connotations, I think it is best to use the terminology already in use *even though* that terminology inconveniently betrays the presumption that the levels are asymmetrically ordered – an assumption I do not myself make here. Therefore, to clarify my terminology at the outset, I will not take x's being

[8]Something by way of a statement of my view regarding the relationship between physics and metaphysics may be found in French and McKenzie [2012].

prior to y to be sufficient for x's being *more fundamental than* y; I will take x to be more fundamental only if, in addition, y is not also prior to x. Where y *is* also prior to x, I will take x to be *as fundamental as* y. Finally – and again perhaps relatedly – unlike much contemporary metaphysics, including fundamentality metaphysics, the focus will be on *actuality* throughout. Thus, in stating that I am interested in the question of whether physics can give us grounds to deny the existence of a fundamental level, I am asking a question about physics' capacity to ground denials of a fundamental level to *this* world in particular. And since I intend to use physics to engage with the metaphysics of fundamentality, the physics theories I shall be primarily concerned with are those that are regarded as the most fundamental working theories that we have managed to produce to date – namely, our theories of high-energy particle physics.[9] Therefore another, more pithy way of stating my project in this thesis is to say that I am investigating fundamentality metaphysics through the lens of fundamental physics.

In sum, then, Part 1 this thesis will examine whether we can use physics to deny the existence of a fundamental level to the actual world, and Part 2 will ask whether, and how, the approach taken to answering that question suggests further changes in our fundamentality metaphysics. With the terrain we are entering into mapped out, I will embark on my discussion by expanding on some of the assumptions implicit in the question set in Part 1. In particular, I will begin by clarifying what it means to consider reality to be structured into a levels hierarchy, and whether one may legitimately regard it as so structured.

[9] Of course, the term 'fundamental' has been a disputed term in physics; see, e.g. Martin [forthcoming].

Part I

Denying the Existence of a Fundamental Level

Chapter 2

Defining the Terms of the 'Levels' Picture

The 'layers of reality' picture that I alluded to in the last chapter depicts the world as being stratified into levels, with the most fundamental level lying at the bottom of it. What sort of priority relation it will be appropriate to cite in defining the levels structure will in part be a function of how the levels are conceived – that is, whether they are regarded as being constituted by, for example, objects, properties or laws. It is therefore important to articulate at the beginning some aspects of what these priority relations may be taken to involve.

2.1 Defining 'Priority'

Before I get to the specific relations that have been cited for this purpose, it may be helpful to note at the outset that priority relations may be sorted into two broad classes depending on what we take the role of the fundamental to be. On the one hand, we may view the fundamental as that which is in some sense *sufficient* for the existence of the non-fundamental. We might thus consider the fundamental to be that whose existence *implies* the non-fundamental: to be such that, given it, nothing else need be supplied for the non-fundamental to come into existence. This is arguably the concept of fundamentality that is presented to us in the work of Armstrong and Lewis, in which the fundamental level of a given world is regarded as a *supervenience base* for that world.[1] According to this conception, then, we may say that the fundamental is that which *determines* the non-fundamental – both the way that it is, and the fact that it exists at all.[2] On the other hand, we may regard the fundamental as a *necessary* condition on the existence of the non-fundamental; that is, we may choose to conceive of the fundamental as that without which the non-fundamental could not exist. It it thus intuitively correct to say on this conception that the non-fundamental is *ontologically dependent* on the fundamental. Such a dependence-based conception of relative fundamentality is to be found in the works of, amongst others, Schaffer and Fine.[3]

Each of these ways of conceiving of the fundamental – that is, as a *sufficient* condition for the existence of the non-fundamental, or as a *necessary* condition on it – may thus be taken to present us with a different class of priority relation. I will designate these classes as *determination* and *dependence* relations respectively. Indeed, one may argue that these two ways of conceiving of relative fundamentality are not only conceptually quite distinct, but also that are they are not co-extensive.[4] Thus, although the two classes of relation

[1]See for Armstrong [1997]; Lewis [1999]. Actually, there are slight differences in Armstrong and Lewis' concepts of supervenience, such that Armstrong's implies Lewis' but not (necessarily) *vice versa*; see Johansson [2002]. I will put forward a definition of supervenience below that is intuitive and implies both of their versions.

[2]On how this concept of supervenience amounts to a notion of determination, see Yoshimi [2007], Section 2.

[3]See e.g. Schaffer [2009] and Fine [1995a]. Note that I am not saying that dependence consists *in no more than* a necessary condition. This will become clearer in Chapter 9, where I will side with Fine in holding that purely modal analyses of dependence are too coarse-grained to be of use, so that, while dependence attributions imply statements of necessity, the converse may not be true.

[4]As Yoshimi argues, the supervenience of the mental on the physical permits free-floating Cartesian minds, which cannot be said to depend upon physical properties (*op. cit.*, Section 3). (Additional support for this claim comes from consideration of the supervenience of necessary properties, and will be discussed below.) Conversely, it may be argued that there are cases of dependence without supervenience. To take an example from the history of philosophy, Descartes held that the attributes of objects depend on those objects having extension, but he would presumably not have held that, for example, the colour of a table supervenes on its extension (see Yoshimi [2007], Section 5). For a more contemporary example, consider how the spin state of a pair of entangled electrons is dependent on its relata having some value of absolute spin – that is, dependent on its relata being spin-1/2 particles, spin-1 particles, or any kind of particle *that has spin* – but cannot be said to *supervene* on such properties (cf. Teller [1986],; Maudlin [1998].

are often conflated in the literature, we should be clear in our heads that these two broad approaches to characterizing relative fundamentality may each pick out different relations.[5] With that in mind, I will move on now to consider some of the more specific relations that are taken to populate each class.

A variety of priority relations are discussed in the literature, each tailored to the entities in terms of which we might choose to define the levels. According to Schaffer, the 'central connotation' of the levels structure is in mereological terms – that is, in terms of 'the part-whole relation'.[6] And it certainly is the case that part-whole relations are frequently alluded to when articulating the levels structure. For example, the above quote from Kim concerning the fact that the bottom level is thought to consist 'of elementary particles, or whatever our best physics is going to tell us are the basic bits of matter' continues:

> As we go up the ladder, we successively encounter atoms, molecules, cells, larger living organisms, and so on. The ordering relation that generates the hierarchical structure is the mereological (part-whole) relation: entities belonging to a given level, except those at the very bottom, have an exhaustive decomposition, without remainder, into entities belonging to the lower levels.[7]

The same sort of sentiment is sometimes echoed in the physics community as well; think, for example, of particle physicists' frequent claims that they are 'in search of the ultimate building blocks' of nature and that this search constitues the principal business of physics.[8] Such a rendering of the level-structuring relation will clearly be appropriate only if the relata at hand can meaningfully be conceived of as admitting a part-whole structure, and thus levels related through this relation will typically, though perhaps not always, be defined in terms of objects.[9] Since by definition a composite object requires

[5]Thus Schaffer [2003] refers to supervenience as 'dependence'; Kim likewise frequently speaks of 'dependence or determination' as if they were one and the same thing (see e.g. Kim [1993], p143). See again Yoshimi [2007] for discussion.

[6]Schaffer [2003], p500.

[7]Kim [1998], p15.

[8]This is, for example, the title of t'Hooft's book on the development of the Standard Model (t'Hooft [1996]).

[9]That is, though we may in some sense say that laws have 'parts', such as the terms that are summed to form the relevant equation, we do not use sharing of

for its existence the existence of parts (at least *some* parts, and typically at least sufficiently similar to those it as a matter of fact has), the part-whole relation is taken as the paradigmatic *ontological dependence* relation. Other paradigmatic species of dependence relations besides mereological relations include the set-theoretic relation of *having as a member* and – more generally – *being defined in terms of*.[10] Indeed, there are a number of approaches to dependence that are completely neutral on the category of their relata, and thus can apply equally well to physical objects and sets as to properties and laws.[11] Such category-independent approaches to priority will be useful to us in Part 2.

In addition to this mereological construal of priority, Schaffer cites (i) 'a *supervenience* structure, ordered by asymmetric dependencies', and (ii) 'a *nomological* structure, ordered by one-way bridge principles between families of lawfully inter-related properties' (though he states that these are merely 'peripheral' connotations relative to the mereological relation).[12] Of these two relations, the most prominent is doubtlessly that of supervenience (and probably in large part owing to the influence of Armstrong and Lewis). Supervenience paradigmatically obtains between properties, and so it could be an appropriate priority relation if we took the levels hierarchy to be defined in terms of more and less fundamental properties. However, since presumably

these 'parts' to make for more and less fundamental laws. Similarly, Armstrong [1978] tried to articulate the relationship between structural *universals* – such as those instantiated by molecules – and those universals instantiated by their atomic components in mereological terms, but seemingly with limited success; see Lewis [1986].)

[10] Indeed, for Fine 'being defined in terms of' is what ontological dependence is, in general, ultimately all about, and I will expand on this notion later on in Chapter 9.

[11] See for example the survey in Correia [2008], but to take just one example for the purposes of illustration: Simons has proposed that ontological dependence can be analyzed as nothing more than a necessary condition on existence, so that x will depend upon y if necessarily, x exists only if y exists (Simons [1991]). Clearly, such an understanding of priority does not require its relata to be of any specific category. (I note that although the approach to dependence that we will look at in detail in Chapter 9 rejects purely modal approaches such as this, that is not really to the present point.)

[12] Schaffer *op. cit.* Note that asymmetry is explicitly being assumed *a priori* here. Once again, *I* will not do this; in my case studies I will try to derive the appropriate structure of the priority relations in play. I should point out too that Schaffer also cites 'a realization structure, ordered by functional relations' as an example of a priority relation, but this relation will not be discussed here.

everything, of any category, has to have at least some properties in order to have any existence at all, we may take supervenience to be a relation that can hold between entities of whatever category, so long as it is remembered that it is the *properties* of those entities that are directly related.[13] While there are a number of (inequivalent) formulations of supervenience in the literature, I will hold that A-type properties *supervene on* B-type properties iff indiscernability with respect to B-properties entails indiscernability with respect to A-properties.[14] This formulation entails that variation between some specific A-properties implies variation in the specific B-properties, and hence that differences in the supervenient A-type properties entail differences in the subvenient B-type properties – which is Lewis' formulation of supervenience.[15] And since B-indiscernibility entails A-indiscernibility, the formulation also implies that instantiation of the specific subvenient properties *suffices* for the instantiation of the specific supervenient properties – which is Armstrong's formulation of supervenience.[16] This latter implication makes it especially clear that supervenience, as I have defined it, is a natural candidate for a *determination* relation, since it implies that specific subvenient properties *determine* that specific supervenient properties are likewise instantiated.[17] However, since it may be easily seen that properties that anything necessarily has will supervene on any properties whatsoever, supervenience is not in general recognized as a dependence relation.[18] This is (in part) because

[13]Thus, as Jantzen reminds us, when we say that objects supervene upon some other entities, we usually mean that the *properties* of those objects supervene upon the properties of those other entities (Jantzen [2011], p434).

[14]See e.g. Yoshimi [2007], p116. Various modal modifications of this are of course possible. We will say that the supervenience is mere *nomological* supervenience, for example, if this is only required to hold in worlds in which the actual laws of nature operate: and so on. But this will do for present purposes.

[15]For Lewis, 'To say that so-and-so supervenes on such-and-such is to say that there can be no difference in respect of so-and-so without difference in respect of such-and-such' (Lewis [1999], p29.)

[16]According to Armstrong, 'We shall say that entity Q supervenes upon entity P if and only if it is impossible that P should exist and Q not exist, where P is possible', so that 'supervenience in my sense amounts to entity P entailing the existence of entity Q' (Armstrong [1997], p11).

[17]Indeed, Yoshimi points out that 'Most varieties of supervenience can be thought of as varieties of determination relation' (*op. cit.*, p117).

[18]See McLaughlin and Bennett [2011], Section 3.5. The reason is simple: two things cannot differ with respect to necessary properties, hence cannot so differ without differing in other respects too. (Note that the grade of necessity of the

necessary properties are not conditional for their instantiation upon any other property, and thus – assuming either a modal analysis of dependence or any stronger analysis that implies it – necessary properties will fail to be dependent upon any other property.[19] Since such properties may nonetheless be shown to supervene upon every other property, it is held that supervenience should not be construed as a dependence relation. I therefore place it in the determination category here.[20]

The nomological relations that Schaffer cites also seem to be good candidates for determination relations, albeit of a less general sort (since they obviously obtain only between laws of nature). The reason, of course, is that such relations are paradigmatically conceived of in deductive terms, hence in terms that make it the case that the more fundamental law *implies*, hence *determines*, the less fundamental law.[21] As the above quote from Schaffer indicates, where such derivations are possible it will typically be *modulo* the use of 'bridge principles' relating the two theories' vocabularies: this is of course the outline of the Nagelian model of inter-theoretic reduction.[22] Where such derivations are possible, we will want to say that the derived laws are *less fundamental* than those they are derived from. This will have as a result that, to take Nagel's own example, the laws of thermodynamics will be less fundamental than those of statistical mechanics on account of the fact that the former – or, better, 'corrected approximations' of them – can be derived from the latter.[23] Let me therefore designate these priority relations as 'relations of

property (i.e. nomologically, metaphysically etc.) involved here is matched by the grade of the supervenience.)

[19]Fine's non-modal analysis, to be discussed in Chapter 9, entails the modal account (but not vice versa), and so the fact that the instantiation of necessary properties does not entail the instantiation of any other properties implies that they do not depend upon them in Fine's account as well.

[20]Again, in agreement with Yoshimi [2007].

[21]See e.g. Schaffer [2003], p 500.

[22]There is thus a sense in which these relations may be taken to involve elements of determination *and* dependence, if we take the bridge principles to express *definitions* of the terms in the reduced theory (and hence indicative of dependence relations if conceived of in definitional terms *a la* Fine). However, as Dizadji-Bahmani *et al.* [2010], Section 4, point out, such a strong reading of bridge principles is in no way forced upon us; we may take the bridge laws to express, for example, mere *de facto* correlations.

[23]For a brief discussion of this notion, and references to extended discussion, see Dizadji-Bahmani *et al.*, *op. cit.*, Section 3.1.

nomic derivation'. Invoking such relations is of course likely to be appropriate if we take the world's levels structure to be carved out in terms of the *laws of nature* that operate in it.

The levels hierarchy is typically articulated in terms of these specific dependence or determination relations – namely parthood, or supervenience and nomic derivation, respectively. But some philosophers have argued that not all of these relations are appropriate for defining the levels structure, given either (i) what we intuitively want ascriptions of relative fundamentality to mean, or (ii) the naturalistic demands that we should place on our metaphysics. It has been argued, for example, that supervenience is not appropriate for defining a levels hierarchy given what we want ascriptions of relative fundamentality to do. Others have argued that, on the contrary, we *ought* to approach the issue in terms of supervenience, since the obvious alternative, the notion of composition, has outlived its usefulness in physics. Furthermore, the very idea that there is anything that is both useful and sufficiently general that can be said about inter-theoretic derivations of laws has been subject to a great deal of scrutiny. But, of course, if turns out that *none* of these relations are suitable for defining a levels hierarchy, then the idea that we can even meaningfully assume the existence of such a thing – and thus meaningfully address questions of whether there is a fundamental level to it – will come under a great deal of pressure. It is therefore important to consider the reasons why these claims have been made and the right response in the face of them, and it is to these issues that I now turn.

2.2 Criticisms of Supervenience as a Level-Structuring Relation

Perhaps the best known of the above criticisms is that the notion of supervenience is not appropriate for expressing relative fundamentality, on the grounds that the notion is simply too weak to capture what we intend by ontological priority. As Kim points out, for example, one can easily construct 'common cause'-type scenarios in which one can say that A supervenes upon B, and that both supervene upon C, while by hypothesis both A and B are as fundamental as one another and only C is more fundamental. (This would be the case if, for example, B made finer discriminations than A.)[24] Similarly, and as Maudlin points out, in any deterministic theory of physics the later state of the world may be said to (nomically) supervene on the earlier states.[25] But we do not thereby want to count the latter as somehow less ontologically fundamental than the former, for intuitively both time-slices of the world are on a par with one another from an ontological point of view. As these examples make clear, supervenience relations can relate what seem to be (and what we might call) 'horizontally' related states of affairs, and as such it seems that the notion is insufficiently fine-grained to express the sought-for notion of hierarchy.

There is in fact now 'a growing consensus that modal notions in general are too coarse for metaphysics, and that notions in the vicinity of "fundamentality", "in virtue of", and the like, should not be understood in modal terms.'[26] Nonetheless, it remains that the view that relative fundamentality is to be cashed out in supervenience terms still pervades the recent literature, and my response to these criticisms of supervenience as a priority relation will be somewhat glib in consequence.[27] In particular, given that supervenience remains fairly ubiquitous as a means of expressing relative fundamentality, I will simply bracket the problems associated with its ability to adequately do so in the one case study in which I invoke it.[28] After all, the criticism of supervenience made by Kim and others is that supervenience is too *weak*, too *coarse-grained*, to capture relative fundamentality. That of course does not

[24]Kim [1993], p146.

[25]Maudlin [2007], p3153.

[26]Sider [2011a], pviii.

[27]This is no doubt due to the prevailing Humean stance in contemporary metaphysics, which motivates the attempted analysis of priority in purely modal terms. As Wilson discusses puts it, for Humeans priority is not *sui generis* but rather understood in terms of 'asymmetric existential necessitation or metaphysical supervenience, with the rough idea being that if some entity *a* asymmetrically existentially necessitates (provides a supervenience base for) some entity *b*, but not vice versa, then a is less fundamental than *b*' (Wilson [forthcoming], p4).

[28]That is, when looking at *structuralist* proposals regrading the priority of symmetry structures over objects in Chapter 8 I will assume that supervenience ascriptions can be used to express priority – primarily because structuralists themselves often phrase priority in precisely these terms. Note, however, that I will also argue in Chapter 9 that supervenience is *not* the most appropriate way for structuralists to capture priority, and I will reconsider their claims when interpreted through a dependence-based account of priority.

preclude that supervenience may nonetheless be regarded as *an important part of the story* about how priority ought to be cashed out, at least in some cases, and in fact Kim himself suggests exactly this.[29] If supervenience can be so regarded, then establishing a supervenience claim can be seen as an important step towards establishing a priority claim, even if it may not ultimately be sufficient for it. For the moment, then, I will simply note that, although arguments abound that it is problematic to employ supervenience (or supervenience *alone*) to cash out relative fundamentality, many commentators nonetheless do exactly that. As such, any interesting results about the levels structure defined in these terms that are deduced in what follows will represent valuable contributions to this extensive literature, flawed though that literature may ultimately have to be taken to be.

2.3 Criticisms of 'The Part-Whole Relation' as a Level-Structuring Relation

Besides supervenience, another relation that philosophers have taken issue with as a legitimate candidate for a level structuring relation is 'the part-whole relation'. Now, in choosing to adopt Schaffer's term here I am arguably stacking the deck in favour of the sceptic, since naturalistic philosophers have argued that there simply is no such thing as 'the' composition relation. As Ladyman and Ross write, for example,

> A good part of most of the special sciences concerns the particular kinds of composition relevant to their respective domains... Metaphysicians do not dirty their hands with such details but seek instead to understand something more fundamental, namely the general composition relation itself. But why suppose that there is any such thing? It is supposed to be the relation that obtains between parts of any whole, but the wholes [concerned can be] hugely disparate and the composition relations studied by the special sciences are *sui generis*. [Footnote: Cf. Paul's mention of the 'primitive relation of fusing, already a part of standard ontology'...

[29] Kim [1993], p148.

> Again 'fusion' in the metaphysician's sense has nothing to do with real composition, and the 'standard' ontology appealed to here is standard, if at all, only among metaphysicians.][30]

Ladyman and Ross therefore claim that there is no such thing as 'the part-whole relation'; rather, composition is resolved into a class of different relations, each of which is suited to the domain at hand. More strongly still, however, Brown and Ladyman seem to claim that appeals to *any* sort of mereological relation are illegitimate. According to them, 'like materialism, mereological structures are obsolete philosophical conceptions in the face of modern physics, and have lost credibility and utility in the effort to describe reality'.[31] As such, they hold that a levels structure should not be phrased in compositional terms at all, but rather in terms of a supervenience structure.

There are a number of different strands of thought in play here, and I will address these two objections to the idea that compositional relations are relevant to defining a levels structure in turn. First of all, the fact that there are *sui generis* notions of composition in the sciences – which there doubtlessly are – does not in itself seem to undermine the claim that part-whole relations can be used to define a levels structure. To my mind, all that Ladyman and Ross' observation that compositional relations are *sui generis* shows is that the relevant part-whole relations should, if necessary, be understood differently as we traverse the domains of the various sciences (and indeed perhaps the domains of the various theories within each). Thus, while we can take issue with the idea that the dominant *a priori* theory of mereology – a theory in which parthood (i) is typically understood spatially (in terms of 'overlap' etc.), (ii) has a partially ordered structure imposed upon it *a priori* and (iii) is such that wholes are represented simply as the 'fusion', or 'mereological sum', of their parts – has any claim to describing the 'most general' form of composition, we can still claim that there is a *recognizable notion* of composition that may take on different forms, including different *logical* forms, in play across the sciences.[32] Indeed, were we not even able to

[30]Ladyman and Ross [2007], p21.

[31]Brown and Ladyman [2009], p.28

[32]Healey [2011], Section 1 presses that philosophers often conceive of composition in spatio-temporal terms, but that such conceptions are increasingly redundant in post-classical physics. Note, however, that while mereology often *is* conceived of

recognize that the various *sui generis* composition relations have something in common – namely, the fact that they *are* composition relations – it seems that Ladyman and Ross' claim above would not even make sense. And if it *is* the case that the existence of a multitude of *sui generis* composition relations does not undermine the idea that it makes sense to speak of part-whole relations, we can still hold on to the idea that the levels structure may be delineated with respect to part-whole relations, with the qualification that those relations should be understood in the specific sense appropriate to the levels in question.

The claim of Brown and Ladyman that the very notion of composition is obsolete in modern physics, on the other hand, is a very different, and at first sight much stronger, claim than that of Ladyman and Ross. But even if it is true (on which more in a moment), it first of all does not clearly follow that compositional relations are thereby useless for defining a levels hierarchy. All this obsolescence would mean, after all, is that compositional relations are of no use when defining the levels structure within that portion of reality *that is described by modern physics*. In regimes in which concepts of classical physics are applicable (to a good approximation), or in regimes best described by sciences other than physics, it may still be the case that compositional relationships play at least some role in defining the levels hierarchy, even if they cannot be employed across the board. More strongly still, I think that the very fact that there is a point in the hierarchy at which such relationships break down – so that the levels must be conceived of in wholly different terms from that point on – should strike us as a very *interesting feature* of the hierarchy, not one that means that mereology should be dispensed with *tout court* in defining priority structure. Still, *if* Brown and Ladyman are right that mereological structures are obsolete in modern physics, it follows that composition cannot be the whole story when it comes to relative fundamentality, and as such that any mereological construal of it must be supplemented with another priority relation.

But it is not at all clear that they *are* right. To my mind, it seems more representative to say that across modern physics we find a rich array of

spatially – Decock for example states that 'mereology is the study of the relation of part-whole in the spatial sense' [2002], p227 – others hold that mereology can be divorced from spatial concepts (see e.g. Paul [forthcoming]).

compositional relations – as has been described by, for example, Shimony and Healey.[33] In particular, it is certainly not the case that quantum mechanics lacks any principle of composition, for that principle is furnished by the tensor product of the Hilbert spaces associated with each component system.[34] So it cannot simply be true to say that quantum mechanics recognizes no notion of composition at all. That is not to say, of course, that composition in quantum mechanics is not radically different in many respects from the quasi-Democritean concepts of composition that typically govern philosophical mereology.[35] As Healey points out, for example, there are contexts in quantum physics (and indeed classical field theories) in which *linear superposition* arguably plays the role of a composition relation.[36] But since superpositions are always invertible, if superposition defines a composition relation then anything that is regarded as a constituent of a whole may just as well be regarded as having that whole as *its* constituent. And since parthood is standardly taken to be asymmetric, this represents a radical departure from mereological orthodoxy.[37] Now, of course, whether we say that this symmetric relation represents a different (and perhaps more general) sort of composition relation than those considered hitherto, or rather, as Brown and Ladyman suggest, that compositional relations are simply obsolete in this context, is to some extent a purely semantic matter. But, in light of the fact that composition relations in contemporary physics can be *sui generis*, various, and even highly counter-intuitive, it seems that the fairest thing to say is that questions of composition are subtle, delicate, necessarily qualified and context-dependent, not that there is simply no place for the notion of composition in contemporary physics at all. (Such a claim would be far too hasty.) While there is of course much more that one could say about composition in modern physics, I will take it that *sui generis* composition relations remain at work in that context and as such that mereological relations, of some suitable sort at least, remain plausible candidates for defining the levels structure.

[33]Shimony [1987]; Healey *op. cit.*.

[34]This is discussed in Shimony *op. cit.*, p194; Healey [2010], Section 7; and Butterfield [2010], Section 3.1.2.

[35]I take impenetrability, indivisibility and immortality to be features of Democritean atoms (cf. Shimony *op. cit.*).

[36]Healey *op. cit.*, Section 3.

[37]Note that some 'non-standard' formal mereologies have been developed in which asymmetry need not be respected; see e.g. Healey *op. cit.*, p11 for references.

Nonetheless, I will concede that it is possible that there could be a regime in high-energy physics in which notions of composition are either too far removed from other, antecedently familiar forms to be usefully described as such, or even that they are simply nowhere to be found.[38] If composition does indeed break down in some physical regime then I grant that another structuring relation will be needed. Once again, I take it that that itself would constitute an interesting feature of the levels structure, and not one that renders composition simply useless to the project of defining it.

2.4 Criticisms of Nomic Derivation as a Level-Structuring Relation

Given the possibilities raised above, perhaps a safer – that is, more likely to be universally applicable – structuring relation would be one that relates more and less fundamental *laws*. It is difficult, after all, to imagine physics ever doing without laws.[39] As indicated in Schaffer's list above, the relation standardly taken to perform the role of relating laws is a *deductive* relation – or, as I am designating it, a 'relation of nomic derivation' – where this deduction will typically involve the use of bridge principles relating their various terms

[38]I should note that it is not just Brown and Ladyman who have mooted this view, for Steven Weinberg has foreshadowed it. As I will have cause to mention again in Chapter 7, Weinberg likewise believes that the concept has outgrown its usefulness in defining reductionism, and as such he holds that fundamentality ought to be conceived of in wholly different terms – namely, in terms of fundamental *principles* (on which much more in Part 2). As he writes, 'it is not possible to give a precise meaning to statements about particles being composed of other particles', giving as an example how the constituent quark model is only a part of the story when it comes to hadron structure (Weinberg [1995a]). He then goes on to claim that, because of this, fundamentality ought to be construed not in terms of constituents, but in terms of fundamental physical *principles*, since it is only the latter that have a clear meaning. But, once again, to say the fact that the notion of composition is bewildering in *that* context is not to say that there is no useful notion of composition to be had *at all* (as the fact that the constituent quark model is at least *part* of the picture suggests). Nonetheless, if Weinberg is right, it does mean that compositional relations are of limited applicability and thus cannot by themselves tell the whole story about the levels structure.

[39]See, however, Wheeler [1983].

à la Nagel.[40]

However, the idea that Nagelian relations between laws could be used to define a levels hierarchy might seem naïve for a number of reasons. First, the physicalist assumption that all special science laws ultimately reduce to the laws of physics is regarded as contentious, at least in some quarters.[41] Indeed, even the question of whether theories *within* physics – such as Newtonian mechanics or thermodynamics – stand in well-defined deductive relations to other, *prima facie* more fundamental theories of physics, such as quantum mechanics, can be very difficult to argue.[42] Second, the idea that (something like) Nagel's model of inter-theory reduction constitutes an adequate representation of relations between realistic theories has come in for a good deal of criticism.[43] One of the chief criticisms of Nagel's model is that it is typically not the putatively 'less fundamental theory' that is derived (via the bridge principles), but only some *approximation* of it. Yet no independent criterion of *how* these approximations are to be conceived, or what their acceptable quantitative limits are, can be antecedently provided.[44] This has been argued to render Nagel's account vacuous.[45]

Instead of trying to survey all the relevant literature on this issue, I will settle with saying the following on each of these points. The point regarding special science laws I will simply reject by fiat; I adopt a reductive world-view in which the laws of chemistry and biology ultimately, and irrespective of our grasp of how they do so, reduce to the laws of physics. I will not argue for that view here.[46] Regarding the point concerning the adequacy of (something like) Nagel's model to capture inter-theory relations, I will here refer to recent and

[40]The original statement of this is in Nagel [1979].

[41]One need not even go to biology to find examples of this view, for some even deny that the laws of chemistry reduce to those of physics. See e.g. Hendry [2010], Section 3.

[42]See e.g. Bokulich [2008]

[43]These last two points are clearly not independent of one another, for in the absence of at least some defensible model of reduction the claim that these theories do or do not stand in well-defined relations of relative fundamenality cannot even be asserted.

[44]See for example Problem 7 of Dizadji-Bahmani *et al.*, *op. cit.*

[45]Of course, other points have been waged against the theory, such as Feyerabend's that bridge principles are incoherent on semantic grounds. See *ibid.* for replies.

[46]A critical survey of the anti-reductionist arguments in chemistry, which ultimately places the problems motivating that world-view squarely within problems in the interpretation of physics, may be found in McKenzie [2008].

persuasive work by Butterfield and by Dizadji-Bahmani, Frigg and Hartmann defending the Nagelian account from the objections that have been levelled at it.[47] What I *will* say, however, regarding the criticism of Nagel's model mooted above concerning the fact that no general prescription for approximation has been given is more or less just what I said in the mereological case – namely, that the fact that the question of inter-theory derivations is subtle, complex, and not amenable to blanket generalization does not in itself mean that such an approach to structuring the levels hierarchy is ruled out.[48] Given that (something like) Nagel's approach arguably *is* the right way to conceive of the relative fundamentality of theories, the question of whether the known laws of physics can thus be taken to stand in well-defined relations of relative fundamentality becomes an *a posteriori* question to be settled by examination of the theories themselves, such that *if* well-defined relations of nomic derivation may be shown to exist between certain laws, then we will be justified in taking those laws to be aligned in well-defined relations of relative fundamentality. That, in any case, is all I will need for my purposes.[49]

2.5 The World as Possessing a Levels Structure

In the wake of that (brief) discussion, I believe that we have grounds to assert that there exist a variety of priority relations, each of which is *prima facie* feasible for defining (at least some portion of) the levels hierarchy. But this embarrassment of riches raises another potentially problematic issue – namely, whether each of these levels 'comport' with one another.[50] To ask whether the levels comport is to ask whether the relations align with one another such that, if two entities (such as two objects) are related by one priority relation (say a part-whole relation), then either they or some feature of them (such as their

[47]Butterfield [2011], Section 3 (especially 3.2.2); Dizadji-Bahmani *et al.*, *op. cit.*.

[48]This is echoed in Dizadji-Bahmani *et al.*, Section 4, reply to Problem 7.

[49]Indeed, in the case study I will look at in which laws define the levels hierarchy, namely the 'effective' approach to quantum field theory, laws may indeed be taken – at least at the level of detail at which I will study the issue – to stand in such relationships. (As Castellani has already pointed out, assessing questions of relative fundamentality pertaining to laws proves to be much more transparent in this context than in the general case (Castellani [2002], p253-4); much more on this in Chapter 6 below.)

[50]Cf. Schaffer [2003], p500.

properties or the laws governing them) will likewise be so related by another priority relation. However, the various priority relations may be expected to comport in this way appears *prima facie* unlikely. As Schaffer notes, Kim has argued that one and the same object can instantiate both the sub- and supervenient properties associated with some phenomenon.[51] If so, then it follows that supervenience structure may come apart from mereological structure, in the sense that, where a mereological relation discerns one level in that object, a supervenience relation discerns two. Another example may be gleaned from elementary particle physics. Few would regard quantum electrodynamics as a fundamental theory (on account of the fact that it is derivable from the more unified Weinberg-Salam model), but nonetheless the electrons and photons that QED relates are currently regarded as fundamental particles, in the sense that they are regarded to lack any constituents.[52] Thus it appears possible that the hierarchy of laws has structure extending beneath the fundamental mereological level (if such a level there be). According to Schaffer, if it were to be the case that the levels did not align, it may be that 'the entire "levels" metaphor is best abandoned'; but this seems wholly unwarranted to me.[53] After all, just because there may be as many foliations to make of the structure as there are priority relations does not itself mean that reality is not objectively structured; given that it contains structure with respect to each relation, it seems better to say that it is *more richly* structured than we may at first have assumed. To my mind, all any lack of 'comporting' would imply is that, when speaking of relative fundamentality, we need to take care to specify which particular relation it is that we have in mind. In consequence, it seems that the notion of 'relative fundamentality' should itself be understood as relative to a particular priority relation. And that need not, of course, undermine the objectivity of any relative fundamentality

[51] *Ibid.*, footnote 2.

[52] Of course, whether this is true depends on how exactly we conceptualize what it is for a particle of contemporary physics to have constituents! (In case the reader's mind is wandering in this direction, I note Shimony's point that the fact that an 'elementary particle' can undergo decay into a superposition of other particles need not compromise the view that it lacks constituents; it only requires that we abandon 'the Democritean equating of noncompositeness with immortality' (*op. cit.*, p209). A very nice discussion of some of the subtleties involved in concepts of composition as they appear in high-energy physics may be found in Heisenberg [1975]; I too will have more to say about superposed particles and compositeness in Chapter 5.

[53] Schaffer *op. cit.*, p500.

attribution.

With all that now in place, let me take stock of where the discussion is at this point. The task I set myself in Part 1 of this work is to adjudicate on whether physics can deny the existence of a fundamental level. Such a task clearly presupposes that it makes sense to speak of levels in the first place, and we have just seen that such talk is enmeshed in subtleties and controversies of various sorts. Still, I do not think that anything I have said so far drastically undermines the thought that such talk can be meaningfully engaged in, or that it is appropriate to conceive of the world in these terms. The assumption that nature may be fractioned into more and less fundamental levels is thus one that I will take seriously in this essay, though I happily concede that which priority relation I choose to use to express that relative fundamentality will have to be a function of the relevant physical or philosophical context. (Indeed, if all goes well, the justification for assuming that there *is* a well-defined levels structure will become clearer in the course of the two case studies I will look at in Part 1 of this thesis.)

What I will, however, subject to much closer scrutiny than the question of whether the levels hierarchy exists is the assumption that priority relations must be well-founded, and hence that there must be a fundamental level to the hierarchy that I take to exist in nature. As already noted, there seems to be a widespread presupposition in metaphysics in favour of the existence of such a level. It is therefore surprising that, at least until very lately, the justification for this assumption has been almost non-existent. As Schaffer puts it, 'the proposition that there is a fundamental level is widely accepted but seldom defended.'[54] This situation is changing, however, and it is the quality of the justifications for the fundamentalist assumption that I want to examine now. After all, only if I can show that we are not committed *a priori* to the existence of a fundamental level will there be any point in asking whether *physics* in particular is in any position to contradict that assumption. Let me therefore now consider what good reasons, if any, there are for believing in a fundamental level.

[54]*Ibid.*, p498; p499.

Chapter 3

The View from Metaphysics: Contemporary Fundamentalist Arguments

A particularly expedient way of arguing for the existence of a fundamental level to the actual world would be to argue that such a level is metaphysically necessary, and hence a feature of *all* possible worlds. Now, arguments for the metaphysical necessity of some state of affairs admittedly have an unfortunate tendency to wither in the face of developments in physics.[1] Nonetheless, *if* a good argument for the neessity of a fundamental level could be constructed – and one that is consistent with the physics that we currently possess – it seems that this would constitute about as compelling a justification for the belief in a fundamental level as one could reasonably hope for. After all, it seems unfair to hold speculations about what future physics might produce against a contemporary metaphysical hypothesis – not least since essentially the same thing could be said of our best current *physical* hypotheses. And of course, if we did succeed in convincing ourselves that such a level is metaphysically necessary, then we would simultaneously convince ourselves that physics too was necessarily committed to the existence of such a level, and hence also that physics could not be used to argue against it.

In spite of the prevalence of the fundamentality assumption within con-

[1] A classic statement of this is Putnam [1963].

temporary metaphysics, however, the current consensus in fundamentalist metaphysics seems to be that the existence of a fundamental level is *not* in fact metaphysically necessary. Cameron, for example, reviews a series of *a priori* arguments – some of which have been prominent in the history of philosophy and some of which are of his own making – in support of the conclusion that 'there must exist a realm of ontologically independent objects which provide the ultimate ontological basis for all the ontologically dependent entities', where the 'must' is understood as having metaphysical force.[2] But he argues, correctly I think, that none of these arguments fare any better than the cosmological argument for the existence of God as the ultimate locus of causal relations – an argument which is well-known to have come under withering attack from, amongst others, Russell and Hume.[3] In response to his own criticisms of arguments for the necessity of fundamentality, Cameron suggests that we 'should abandon the attempt to give a metaphysical argument for the intuition under discussion and instead justify it on broadly theoretical grounds', and in particular, in terms of the *virtues* that theories that posit fundamentality are claimed to possess.[4] Such grounds, he claims, would constitute 'a reason to believe in the *truth* of the intuition against infinitely descending chains of ontological priority', and hence to believe in the *actuality* of a fundamental level, but they provide 'no justification for the claim that the intuition is *necessarily* true'.[5] But since it is the more restricted question of whether the actual world possesses a fundamental basis that I am interested in here, were theoretical considerations to furnish us only with a justification for endorsing the more limited conclusion that the actual world does, as a matter of fact, possess a fundamental level, then that need not be seen as any shortcoming of an argument based on them from my point of view. Rather, all

[2]Cameron [2008], p8. Cameron assesses these arguments in Section V of his paper.

[3]The most compelling objection one can make against the cosmological argument, in my view, is that which Bertrand Russell waged against Father Copelston. In this famous exchange, Russell argued that the supposed fact that everything needs a cause no more implies the conclusion that there is a cause of everything than the fact that every human requires a mother implies that there must be a mother of everyone. (A transcript of the discussion is available here: http://www.philvaz.com/apologetics/p20.htm.) Nowadays such arguments are of course known as 'quantifier shift' arguments.

[4]*Ibid.*, p13.

[5]*Ibid.*

that our failure to come up with an argument for the necessity of fundamentality need signify is that one particularly *expedient* way of arguing for the actuality of fundamentality is precluded. Cameron's argument is therefore one that should be considered here.

As noted, Cameron employs the notion of theoretical virtues to argue for the actuality of fundamentality. Cameron is not alone in using theoretical considerations to this end, however, as Sider has recently offered a related, though different, argument with fundamentalist implications that draws on similar concepts. I will therefore consider in this chapter whether these contemporary arguments, arguments predicated on the features that fundamentalist theories supposedly enjoy, are arguments for the well-foundedness of priority relations that we ought to take seriously. I begin with that of Cameron.

3.1 Fundamentality through Virtue 1: Cameron's Argument from 'Theoretical Utility'

As mentioned, Cameron seeks to justify the fundamentalist intuition on 'broadly theoretical' grounds – namely, on the basis of the *virtues* that fundamentalist theories are supposed to possess in comparison with their anti-fundamentalist rivals. The virtue that Cameron primarily appeals to to this end is that of unification.[6] As he writes,

> If we seek to explain some phenomena, then, other things being equal, it is better to give the same explanation of each phenomenon than to give separate explanations of each phenomenon. A unified explanation of the phenomena is a theoretical benefit.[7]

It is therefore clear that Cameron views more unified explanations, in the sense just given, to be in some sense 'better'. The key point, for Cameron, is that if chains of priority do not terminate, then while everything that exists has an explanation, 'there is no explanation of everything that needs explaining' – that is, we cannot provide *a* [i.e. *the same*] explanation for everything

[6] Instead of using the term 'theoretical virtues' directly, Cameron speaks of theoretical 'costs and benefits' (see e.g. *ibid.*, p12).

[7] *Op. cit.*, p12.

non-fundamental'.[8] He moreover takes this to mean that anti-fundamentalist explanations are *less unified* than their fundamentalist counterparts. Putting everything together, then, fundamentalists *give better explanations* of whatever needs explaining than anti-fundamentalists do. But better explanations are, claims Cameron, more likely to be true. As such,

> the status [he] attribute[s] to the intuition is like that enjoyed by Ockham's razor: we should accept it because if it is true the theories we arrive at give a better explanation of the phenomena to be explained, and hence are more likely to be true.[9]

This is taken to 'provide some evidence for the intuition' that priority relations amongst actual entities do indeed bottom out somewhere. Now, as already mentioned, Cameron concedes that such virtues do not secure the *necessity* of fundamentality, but claims that they nonetheless provide grounds for thinking that this, the actual world does in fact possess a fundamental basis. In his words,

> [S]uch principles of theory-choice do not appear necessary; it is not as if the world is necessarily such that the simplest explanation is the right one – we just hope that our world is like this. Relying on these principles could have taken us badly wrong, but we live in hope that they do not in fact do so. I have offered a reason to believe in the truth of the intuition against infinitely descending chains of ontological priority, but I can think of no reason to believe in its necessity.[10]

That, in a nutshell, is Cameron's argument.

How seriously ought we to take it? In sum, the premises of the argument seem to be that (i) fundamentality gives the best explanation of what needs to be explained, on grounds of superior unification, and that (ii) it therefore provides the more *likely* explanation than its anti-fundamentalist rival. From these it is concluded that it is *more likely than not* that the world possesses a fundamental basis (even though we cannot claim that the world is necessarily that way). However, it is doubtful that even this modally weakened conclusion

[8] *Ibid.*; italics added.
[9] *Ibid.*, p13.
[10] *Ibid.*

is sanctioned by Cameron's argument. I contend that one can take issue with each of Cameron's premises, and moreover do so from a number of angles.

Criticism of the first premise has already been lodged by Orilia, who argues that it is, *contra* Cameron, entirely unclear that fundamentalist explanations do in fact fare better with regard the virtue cited – namely, that of unity.[11] As Orilia puts it, saying that there is a unified explanation of two non-fundamental existents on the grounds that they both have explanations in the fundamental basis

> is like saying that there is a uniform causal explanation of two disparate phenomena, the breaking of the glass and John's recovery from pneumonia, because there is a collection of two events, namely {Tom's hurling a stone, John's taking antibiotics}, such that one of these two events caused the glass to break and the other John to recover. Clearly this kind of 'uniformity' is too gerrymandered for it to confer any advantage over rival explanations that do not enjoy a similar uniformity[.][12]

This seems hard to deny. Nonetheless, Cameron might have a way out in shifting the emphasis from unity to *simplicity*: while a fundamentalist explanation may have no clear claim to unity, it seems that he might be entitled to claim that such an explanation would be simpler, since any full and satisfactory anti-fundamentalist explanation would necessarily fail to terminate (at least if it was to be non-circular). And it seems at least *plausible* that a form of explanation that terminates has a claim to being 'simpler' than one that in principle does not – indeed simpler than one for which there is not even any principled place for it to begin.[13] So given that Cameron's general strategy is to defend the actuality of a fundamental level by appealing to

[11] See Orilia [2009].

[12] *Ibid.*, p.338.

[13] As Callender writes in the context of discussing Schaffer's anti-fundamentalist argument, to be discussed below: 'The obvious point to make is that a theory appealing to only a finite descent is far simpler than an infinite descent model. Simplicity is perhaps the cardinal theoretical virtue of scientific theories. [...] What is simplicity? Who knows? On any discussion of simplicity I have ever seen, an infinite hierarchy of entities and theories of those entities doesn't count as simple.' (Callender [2001], p6.)

theoretical virtues, it seems he could meet (or at least avoid) Orilia's objection by placing the emphasis on simplicity as opposed to unity. Doing so would likely prompt him to make an explicit appeal to some version of Ockham's razor to justify his conclusion, as opposed to merely citing it to communicate the structure of the argument that he does adopt concerning unity.[14]

Let us therefore – at least for argument's sake – concede that fundamentalist explanations do indeed fare better with respect to *some* virtue, such as the virtue of simplicity, even if they fail to exemplify a high degree of unification. However, even if we do grant this, we may still take issue with the idea that the possession of such virtues by the fundamentalist theory gives grounds for ascribing greater relative likelihood to it. Suppose we begin by thinking about the general issue in the context of scientific theories. As is well known, the optimistic idea that virtues such as simplicity enjoy any obvious or general connection with truth has come under sustained attack in the philosophy of science. In that context, anti-realists (and perhaps most notably van Fraassen) might well agree that simpler theories are 'better', but will hold that this 'betterness' is ultimately pragmatic in character and of no epistemic significance. Thus, they would reject the slide from the idea that a given explanation is 'better' insofar as it is simpler (or indeed more unified) to the idea that it is therefore more likely to be true, since the 'betterness' of the explanation has *nothing whatsoever* it do with its truth (they will hold). As van Fraassen writes,

> Simplicity... is obviously a criterion in theory choice, or at least a term in theory appraisal. For that reason, some... suggest that simple theories are more likely to be true. But it is surely absurd to think that the world is more likely to be simple than complicated (unless one has certain metaphysical or theological views not usually accepted as factors in scientific inference). The point is that the virtue, or patchwork of virtues, indicated by the term is a factor in theory appraisal, but does not... make a theory more likely to be true.[15]

[14]Indeed, Sider uses simplicity considerations in support of fundamentalist conclusions, as I will discuss in the next section.

[15]van Fraassen, [1980], p90; quoted in Musgrave [1985], p202.

As is equally well known, however, scientific realists have worked hard to circumvent this objection and have typically done so by holding that simplicity and other virtues need not be seen as purely pragmatic in character, nor as purely metaphysical (or indeed 'theological'). Musgrave, for example, has claimed that if one can show that 'theories constructed under [the] aegis [of simplicity] are empirically successful, while theories which violate it are not', then we could 'point to the empirical success of science in vindication of our belief' that 'Nature is simple (in some carefully specified sense or senses)'.[16] If we could indeed show that, we would then be warranted in choosing between the underdetermined theories accordingly. Defenders of the realist import of theoretical virtues will therefore urge that since appeals to simplicity or unity have repeatedly led us to well-confirmed theories, the idea that nature is accurately described in particularly simple or unified terms is one that has received *indirect confirmation* along with those theories themselves – no doubt noting that Einstein himself famously held such a view.[17] There is thus a tradition in the philosophy of science that holds, in the face of these sorts of empiricist objections, that virtues such as simplicity *can* be taken to have genuine truth-tracking import.

Whatever can be said for such moves, any such appeal is obviously going to be parasitic on the track record of science, and in particular on the case that one can make for the claim that appeals to the relevant virtues have indeed led to empirically successful theories. But suppose we now consider theories that do *not* receive any empirical confirmation. *Prima facie* at least, we might be tempted to deny of such theories that any feature of them has been confirmed, directly or indirectly, since by hypothesis these theories do not enjoy *any* confirmation; hence, it seems, nor do their features either. So while it may be that historico-empirical facts can be cited to warrant appeals to virtues in the case of *scientific* theories, it is entirely unclear that such warrants transfer to non-empirical theories, to which the concept of confirmation of course does not apply. But it is clear that the rival (fundamentalist and anti-

[16]Musgrave [1985], p203-4.

[17]See e.g. Musgrave *op. cit.*, p204; Einstein [1934]. Of course, such a claim has no bite without some working *criterion* of simplicity. But both scientists and philosophers of science have tried to articulate what sort of thing they intend by it, and the motivation for putting in this work is largely because emminent scientists are 'always appealing' to such virtues (Musgrave *op. cit.*).

fundamentalist) theories of 'metaphysical explanation' that Cameron has in mind are examples of just such theories. What the theories he discusses are to 'explain', after all, is just *the existence of the non-fundamental*. It is of no relevance whatsoever to either theory what these entities are like – what properties they have, or how they behave – since *any* non-fundamental entity, *whatever* it is like, is to be accounted for by each of these theories. By contrast, of course, empirical theories cannot be expected to account for everything non-fundamental, regardless of its properties. For one thing, unless there exists some phenomena that they are incompatible with, and hence cannot hope to explain, they will never qualify as falsifiable. As such, whatever support appeals to theoretical virtues may have gained from historical episodes of successful confirmation, that support seemingly cannot be appealed to here, on the grounds that the theories Cameron considers are simply not the sort of thing to which confirmation can apply. If appeals to simplicity are to be warranted in this context, then, that warrant must issue from another source. And until we discover what that warrant is in the case of metaphysical theories, we remain threatened by the sort of predicament that van Fraassen alludes to – a predicament in which virtues may make for 'lovely' features but nonetheless none that have any demonstrable connection to truth.

However, one might object that the argument just lodged against Cameron is too hasty. In particular, one might object to the idea that the fact that his metaphysical theories are not subject to confirmation implies that their features are not either. For suppose that the realist defences as applied to empirical theories are warranted, and the virtue of 'simplicity', however we may choose to cash it out, has received confirmation in the case of such theories. One may then be tempted to claim that it has therefore received confirmation *simpliciter* – for would not the fact that nature favours simplicity when it comes to its physical structure give grounds for thinking that it favour simplicity regarding its metaphysical structure as well? Indeed, one might be tempted to argue that if, as I do, we want to keep our metaphysics naturalistic, we must presume that the same virtues that we take to govern our physical theories govern our metaphysical theories as well.

Compelling as it may at first seem, however, the slide from the idea that the virtues can be epistemically backed up as features of scientific theories to the idea that they can be supported as features of metaphysical theories is not as innocuous as it looks.[18] This is because the notion of simplicity that is appealed to in the sciences involves a constraint that renders it inapplicable to the sort of metaphysical theories that Cameron discusses. Moreover, it is unclear that there is any notion of simplicity that *is* applicable to metaphysical theories and that can be appealed to without begging the question at issue. To see this, let us recall what Cameron has to say about simplicity, and in particular about Ockham's razor:

> Such principles of theory choice do not appear necessary: it is not as if the world is necessarily such that the simplest explanation is the right one – we just hope that our world is like this.[19]

This conveys that the understanding of Ockham's razor that Cameron is working with is something like 'the simplest explanation is the best explanation'. But such a rendering concedes far too much to simplicity, for it is of course *not* the case that we regard the simplest explanation as the most preferable one in general. The simpler of two putative explanations need not be regarded as the better one if it is already acknowledged, for example, that the phenomena to be explained are at root very complicated, and hence not the sort of thing that could be be adequately explained in simple terms. (Indeed, in such a case the 'simplest explanation' is unlikely to be regarded as an explanation at all.) For although the explanation of, for example, the solidity of matter in terms of it being composed of interlocking impenetrable atoms may well be 'simpler' than the explanations offered by contemporary atomic physics, we obviously regard the latter as 'better' – presumably in part because we are now well aware that the world is a far more complicated place than we used to think it was. Thus, a much better statement of the sentiment that is – or that ought to be – expressed by Ockham's razor often goes by the name of 'Einstein's razor', and is pithily presented as the maxim that '*everything should be made as simple as possible, but not simpler*'. Holding that things should be made only *as simple as possible* to satisfactorily explain is clearly not the same thing as valuing simplicity of explanation in some unqualified sense, and – as the above example makes clear – the qualification is surely necessary.

[18] Saatsi has also discussed the dangers of assuming that the non-empirical features we cite in warranting our beliefs in scientific theories will automatically be citable in justifying choices between metaphysical theories; see Saatsi [2011]. Similar issues are also discussed in Ladyman [2012].

[19] *Op. cit.*, p13.

But how should we understand the qualification 'as possible' that distinguishes Einstein's from Ockham's razor? Consider first of all how the maxim is understood in the context of scientific theories. In this context, 'as simple as possible' has a clear meaning, and one that is expressed in Einstein's original statement of the principle that bears his name:

> It can scarcely be denied that the supreme goal of all theory is to make the irreducible basic elements as simple and as few as possible without having to surrender the adequate representation of a single datum of experience.[20]

In this context, then, to say that a theory should be made 'as simple as possible' means that it should be made 'as simple as it can be *without compromising its empirical power*'. In other words, the maxim counsels us to construct theories of the unobservable in such a way that they are able to recover all of a relevant class of phenomena whilst being kept as free as they can be of 'loose wheels' that do not support discernible differences in that phenomena. Now, the reason that such a feature is regarded as so useful in underdetermination disputes is because, although an empirically equivalent theory can (it is claimed) always be 'cooked up' from a pre-existing theory, it is perceived as highly likely that the cooked-up theory will contain more in the way of empirically superfluous elements – contain more in the way of 'loose wheels' – than the original theory does.[21] Einstein's maxim would then council us to choose the simpler theory in such cases, and thus help us to ward off this particular realist bogeyman (provided, of course, that the maxim can indeed be given realist support). But what is absolutely key to the utility of appeals to simplicity in such disputes is that the criterion for how simple would be *too* simple may be stated *without reference to what is in dispute between the two underdetermined theories*, namely, the nature of the unobservable world. How simple things are allowed to get is settled with reference only to a domain of empirical phenomena, which by definition is not an issue that divides underdetermined alternatives.

But now the problem that Cameron faces in appealing to simplicity is clear. We know that explanations should not aim to be simple in some unqualified sense, but that they should only be as simple as possible; but how are we to

[20]Einstein [1934], p165.
[21]Musgrave *op. cit.*

specify how simple is *too* simple in the case of two rival, purely metaphysical theories? Since there *are* no relevant empirical phenomena to constrain such theories, any qualified notion of simplicity that is applicable in metaphysics must therefore be different to that which applies in the sciences. And in the absence of any obvious criterion of how simple is too simple that is theory-independent in the way that empirical data is and that is applicable in the case of non-empirical theories, it seems that any appeal to simplicity must beg the question against the anti-fundamentalist. The reason for this is as follows. Although I have granted that it may well be 'simpler' in some unqualified sense to explain the non-fundamental in terms of the fundamental (perhaps, I hazarded, because explanations that can in principle terminate are simpler than those that necessarily do not), any explanation that is simple in this sense would clearly be *too* simple an explanation for cases in which the non-fundamental *cannot* be explained in this nice and simple way – namely, in worlds that lack fundamentality. In such worlds, it would simply be *wrong* to cite a fundamental basis in such explanations, and thus wrong to adopt such a simple explanation. Thus in defending the existence of a fundamental basis, the fundamentalist cannot appeal to a feature of simplicity as a virtue of explanations if possession of that feature would make an explanation *too* simple than would be appropriate in worlds that lack such a basis, since doing so would simply beg the question against the anti-fundamentalist. And nor does there appear to be any reason why exactly the same thing cannot be said, *mutatis mutandis*, for unity as a feature of explanations – even if Cameron's claim that fundamentalist explanations are more unified went through.

All things considered, then, it appeals that Cameron's appeal to theoretical virtues in the effort to secure fundamentalist conclusions simply does not work. Let me summarize what has been shown. As is well known, there is no *a priori* and general connection between truth and theoretical virtues. In the case of empirical theories, however, there is at least the possibility of making an appeal to the history of science to say that some virtues of successful theories have received indirect confirmation through the confirmation of those theories themselves. Unfortunately for Cameron's theories of 'metaphysical explanation', however, no such appeal can be made. And even if one were to claim, perhaps on some sort of putative naturalistic grounds, that if simplicity has been confirmed as a truth-tracking feature of theories of natural science then we should be able to appeal to it in the case of theories of metaphysics, we should be clear that the notion of simplicity that is of interest in the sciences is qualified, and qualified in such a way as to make it inapplicable to non-empirical theories such as Cameron's theories. Furthermore, in the absence of some theory-independent criterion of how simple is too simple that *is* applicable in the metaphysical case, it seems that simplicity cannot be appealed to without begging the question at hand. In sum, then, if the supposed possession of virtues by certain metaphysical theories is all we have to go on regarding the existence of a fundamental level, then it seems that the best thing to say at this point is that we simply *do not know* whether there is one or not – nor, *contra* Cameron, do we have any inkling of the relative likelihoods of the fundamentalist and anti-fundamentalist theories. As such, Cameron's argument furnishes us with no good grounds to believe in a fundamental level – whether of this world or of any other.

3.2 Fundamentality through Virtue 2: Sider's Argument from 'Ideological Parsimony'

In the last section, I noted that there is no obvious and general justification for the claim that the world is more likely to be simple than complicated, and that, even if some support for this claim can be garnered through the empirical success of suitably 'simple' theories historically, it is not a kind of success that theories of metaphysics have any obvious claim to. But that does not detract from the fact that there may be specific *forms* of simplicity for which there is a clear connection with truth-likeness. Thus if theories with fundamentalist implications can be shown to possess such a feature, then we will have rational grounds for holding that the world admits a fundamental level (or at least, *à la* Cameron, that it *probably* does so). It seems to be such a specific notion of simplicity that Sider exploits in his argument for 'mereological nihilism'. The latter is the view that nothing has proper parts, so that there are (despite appearances) no composite objects – a view that is of course committed to the idea that whatever objects exist are mereological simples. Mereological nihilism is therefore a variant of the view that all objects bottom out into fundamental particles – even if in this case the 'bottoming out' is entirely trivial, since there *are* no objects but the fundamental ones. Since it is incompatible with the existence of 'gunk', we can construe Sider's argument for mereological nihilism as an argument with fundamentalist implications.[22] Sider's is perhaps the most well-known of the contemporary arguments in support of such conclusions, and it is this that I will examine now.

It is the idea that theories that exhibit, in particular, greater *ideological parsimony* that are more likely to be true that forms the basis of Sider's fundamentalist argument. As such, a crucial element of the backdrop to his proposal is a realism about 'theoretical ideology', and to communicate what this notion involves it may be helpful to begin by explaining where this notion came from. The notion of ideology (in this context) traces back to Quine, in an essay aimed at Bergmann's claim that the structural properties of the world reflect themselves in an ideal language. According to Bergmann, the primitive predicates of such a language are ontologically significant and as such demand the existence of properties. Predictably, however, Quine saw this conclusion as one that ought to be resisted, and he went on to argue that the primitive predicates of a theory correspond merely to what *ideas* could be expressed in it. These primitive predicates were thus said to belong to the *ideology* of a theory.

> The ideology of a theory is a question of what the symbols mean; the ontology of a theory is a question of what the assertions say or imply there is.[23]

By taking 'ideology' to pertain within the intensional realm of ideas and meaning, while keeping ontology in the more respectable, extensional realm of reference and quantification, Quine presented ideology as a psychological quirk with a bleak scientific future. As he put how he saw matters,

> The theory of reference treats of naming, denotation, extension, coextensiveness, values of variables, truth; the theory of meaning treats of synonymy, analyticity, syntheticity, entailment, intension. The question of the ideology of a theory... obviously tends to fall

[22]Indeed, as I will argue the parsimony argument in favour of a nihilist theory, containing only mereological simples, over a theory which also contains composites of those simples, and the parsimony argument for a nihilist theory over a gunky theory are perfectly analogous: if one works, so does the other.

[23]Quine [1951], p14.

> within the theory of meaning; and, insofar, it is heir to the miserable conditions, the virtual lack of scientific conceptualization, which characterize the theory of meaning.[24]

As such, the ideology of a theory was regarded by Quine as of no objective or worldly significance.

But this is not the view of ideology that is adopted in contemporary fundamentality metaphysics. Indeed, it is not even the view of ideology that Quine himself was to endorse in his later work – work which moved away from the hardline nominalism and extensionalism that was so characteristic of the earlier doctrines. The motivation for Quine taking ideology seriously, and hence also its attendant notion of the *meaning* of physical predicates, issued from his famous study of proxy functions.[25] His construction and use of such functions showed that sentences about physical objects in spacetime could in principle be reduced to the language of set theory, which in turn showed that, if we continue to retain an extensional approach to predicates, the ontology of physics can be reduced to an ontology of pure sets only.[26] But Quine (rightly) saw the 'hyper-Pythagorean' idea that the ontology of physics (or chemistry, or zoology, or any other science) could be reduced to an ontology of sets as disastrous.[27] If this state of affairs is to be avoided, there is therefore no alternative but to relax the thesis of extensionality and concede that predicates carry further significance – significance that can only be understood in intensional terms. As he wrote:

> We must note that this triumph of hyper-Pythagoreanism has to do with the values of the variables of quantification, and not with what we say about them. It has to do with ontology and not with ideology. The things that a theory deems there to be are the values of a theory's variables, and it is these that have been resolving themselves into numbers and kindred objects – ultimately into

[24]*Ibid.*, p15.

[25]See Decock [2002], Chapter 5 for a full discussion of this transition.

[26]*Ibid.*, p158.

[27]He referred to it as an 'ontolological debacle;' see *ibid.*, p157. Note too that retaining a commitment to extensionalism, and hence downplaying the significance of the domestic interpretation, entails a parallel 'ideological debacle', in which the only predicates required to construct theories of physics are set-theoretic. See *ibid.*, p159.

> pure sets. The ontology of our system of the world reduces thus to the ontology of set theory, but our system of the world does not reduce to set theory; for our lexicon of predicates and functors still stands stubbornly apart... We might most naturally react to this state of affairs by attaching less importance to mere ontological considerations than we used to do. We might come to look to pure mathematics as the locus of ontology as a matter of course, and consider that the lexicon of natural science, not the ontology, is where the metaphysical action is.[28]

The work on proxy functions thus led Quine to view the *way that we describe* our ontology as of equal – or even greater – significance than the ontology itself.

It is hard to see how a more complete retreat from the austere doctrines characteristic of Quine's earlier work – work which today remains synonymous with him – would be possible, and Quine never developed a satisfactory statement of the shape of his programme in the wake of the grave problems he himself ultimately raised against it.[29] But irrespective of the difficulties the seeming fact that ideology 'is where the metaphysical action is' caused for the pillars of Quine's programme, it is this realistic conception of ideology that holds sway in contemporary fundamentality metaphysics. Sider in particular has deployed the notion of ideology to articulate his notion of 'realism about structure,' which is now constitutive of his own metaphysical programme.[30]

> The term 'ideology', in its present sense, comes from Quine (1951a; 1953). It is a bad word for a great concept. It misleadingly suggests that ideology is about ideas – about *us*. This in turn obscures the fact that the confirmation of a theory confirms its ideological

[28]Quine [1976], pp503-4; quoted in Decock *op. cit.*, p157.

[29]*Ibid.* pp45-46. According to Decock, the proxy function argument shows that 'logical regimentation of physics is possible, but that the transparency that Quine hopes to gain is entirely lost because no austere explanation of our physical lexicon, our physical ideology, is feasible' (*op. cit.*, p161). (Note that the claim is not that predicates must be taken to *individuate* a meaning, but that they nevertheless must *have* meaning, *viz.* that which they have according to the domestic interpretation of our theories (*ibid.*)).

[30]This is developed in Sider [2011a].

> choices and hence supports beliefs about structure. *A theory's ideology is as much a part of its worldly content as its ontology.*[31]

Amongst a theory's ideology, Sider, like Quine, counts its primitive predicates. Thus a theory's fixing on the right ideology to describe the world is it fixing on the *ways* that the world's ontology is, which includes getting its *structure* right. As examples of theoretical ideology, Sider cites the non-Euclidean structure of spacetime postulated by general relativity. While an anti-realist could adopt a Reichenbachian conventionalism, according to which the points of spacetime may legitimately be carved up in any of a number of ways, the realist about GR's ideology will say that (i) there *is* a non-Euclidean structure out in the world for a theory to get right, and that (ii) GR *gets* it right.[32] There is more to ideology for Sider than predicates, however: for him, the ideology of a theory 'corresponds to its primitive notions... which includes its logical notions as well as its predicates'.[33] Sider admits this extension of ideology beyond predicates is 'vague', and the idea that, for example, the logical quantifiers could count as reflective of 'the way the world is' is a difficult one to come to grips with.[34] But the only ideology that is relevant to the fundamentalist argument about to be discussed will be that of primitive predicates, and this portion of ideology is, I take it, sufficiently clear to be getting along with.

So with all that in hand, let me now turn to that argument. As already noted, Sider uses the notion of *ideological parsimony* to argue for 'mereological nihilism' – namely, the view that nothing is a proper part of anything, and thus that there are no composite objects. All there are are simples arranged in the requisite fashion. As he puts it,

> ...the situation is this: i) ordinary evidence apparently leaves open whether composites exist or whether there exist only appropriately

[31]Sider [2011a], p13; italics added.

[32]See *ibid.*, especially Section 3.4. Just to be clear – as there may be some confusion in this context – here 'ideology' is not to be confused with the notion of a 'stance'. The realist *stance* is that knowledge of the structure of e.g. spacetime is possible; the realist's *ideology* is the predicates (and perhaps some other elements) that are needed to describe that structure itself.

[33]Sider [2009], p417.

[34]*Ibid.* Note that Quine himself initially suggested that the quantifiers of a theory could count amongst its ideology, but later he was unequivocal that it was the predicates alone that did so. See Decock *op. cit.*, Chapter 1, notes 52 and 54.

arranged particles; and ii) ideological parsimony gives us a positive reason to reject parthood, and thus composites.[35]

He then goes on to say that 'the opposing case for composite objects is surprisingly weak', and hence that, as far as he can see, nothing 'counterbalances' his case that all existent objects are mereological simples. As pointed out above, nihilism is inconsistent with the existence of gunk, and is thus committed to the existence of a (mereologically) fundamental level. Sider's argument may thus be viewed as an argument for a fundamentalist conclusion.[36] I will now unpack exactly how it is that Sider takes the notion of ideological parsimony to give us 'positive reason' to endorse mereological nihilism, and thus embrace fundamentalism.

The parsimony argument that Sider employs is in essence very simple, and it may be presented as follows.[37]

1. A world in which no object is composite requires, *ceteris paribus*, less ideology to describe it than would be required in worlds in which some composite objects exist, since the notion of 'is a part of' is not required to give a full description of the former.[38]

2. Theories containing less ideology are more likely to be true.[39]

∴. A theory in which there are no composite objects is, *ceteris paribus*, more likely to be true than a theory in which there are such objects.

Laying the argument out in this way makes it clear that, if the notion of parsimony can be used to argue against the existence of composite objects in fundamentalist worlds, it can be used to argue in favour of fundamentalist over gunky worlds. After all,'ordinary evidence' presumably leaves the question of whether the world is gunky or not at least as open as it leaves the question of whether my table is a *bona fide* object or rather just 'simples arranged tablewise' (assuming that such simples exist), so that the argument for the existence of gunk, and that for the existence of composites of simples, seem to be on a par with one another in this respect. Furthermore, the '*ceteris*

[35]Sider [2011b], p4.
[36]*Ibid.*, p20.
[37]Adapted from Sider [2011b].
[38]*Ibid.*, p2.
[39]*Ibid.*, p3.

paribus' clause could be satisfied, for present purposes, between theories containing composites of simples and theories containing simples just as well as it can between gunky and fundamentalist theories.[40] All that is required is that in each case the theory containing composites contains no extra ideology than is required in fundamentalist worlds (other than parthood, of course). Such would be the case between gunky and fundamentalist worlds if the same qualitative predicates are required repeadedly *ad infinitum* as we descend down levels in the gunky worlds.[41] Thus, while as presented Sider's argument is aimed at the conclusion that we should prefer theories containing only simples over theories containing composites of simples, it seems that the very same argument could equally well be used to argue for mereologically fundamentalist over gunky worlds. I will therefore assess his argument now.

As the argument is clearly valid, I will proceed directly to assessing the premises. Since we are counting primitive predicates as ideology, and 'is a part of' is standardly taken to be just such a predicate, we may regard the first premise as true by definition.[42] I in any case will be happy to grant it here. What I *do* want to scrutinize, however, is the much more contentious-looking second premise. The problem here, of course, is just the familiar one that it is far from obvious what the epistemological significance of ideological parsimony is. Indeed, in places Sider replaces 'ideologically more parsimonious' with the less polysyllabic 'simpler', and I have stressed already how there is in general no straightforward connection between a theory's perceived simplicity and its truth.[43]

That point notwithstanding, it is nevertheless easy to construct *cases* in which an ideologically more parsimonious theory will indeed be more likely to be true. For suppose that we construct a (let's assume finitely axiomatized) theory $T(e)$ of some entity e, perhaps a world, by means of a finite stock of predicates $\{p_i\}$. Now suppose we construct a new theory $T'(e) = T(e) \wedge s(e)$, where s

[40]I will argue below that the *ceteris paribus* clause cannot be appropriately satisfied in either case.

[41]Schaffer [2003] refers to such worlds as 'boring worlds'. The worlds we will have cause to look at in Chapter 5 are arguably just such worlds.

[42]See however Barnes [ms] for a discussion of the notion that parthood may be reduced to other notions.

[43]See e.g. Sider *op. cit.*, p3.

is a contingent sentence formulated in terms of a new primitive predicate p not in $\{p_i\}$.[44] Since primitive predicates count amongst – indeed are the clearest representatives of – a theory's ideology, it is clear that $T(e)$ is *more ideologically parsimonious* than $T'(e)$. But we can also say with confidence that $T(e)$ is *more likely to be true* than $T'(e)$, since it is a consequence of the basic axioms of probability theory that a conjunction cannot be more likely to be true than any of its conjuncts, and will be determinately less likely if any of the other conjunct(s) are contingent. To quote a famous example, it cannot be more likely that Susie is a banker than that Susie is a banker and a feminist: since *more* is predicated of Susie in the latter case, there are more conditions for her to fulfill than in the former case and thus there is a lower probability of her fulfilling the latter description. It follows that we can indeed say, *in cases in which theories are structurally related in this way*, that the more ideologically parsimonious theory will be more likely to be true than the less parsimonious one.

Indeed, the example that Sider gives in illustrating the general strategy – and uses to butter us up to the idea that more ideologically parsimonious theories are to be preferred in general – could be construed as having precisely this form.[45] The example concerns the debate over pre-relativistic spacetime to be found in the philosophy of physics.

> My argument presupposes a sort of realism about ideology. Ideologically simpler theories aren't just more convenient for us. They're more likely to be true, since the worlds they posit are simpler, contain less structure. (Ideology is a worldly matter, not about ideas at all.) Compare the common belief amongst philosophers of physics that neo-Newtonian spacetime is simpler and hence more choiceworthy than Newtonian spacetime. The difference in simplicity has nothing to do with ontology. The same points of spacetime exist according to the two theories (and neither needs to reify relations over points of space-time). Instead, the difference concerns ideology. Describing neo-Newtonian spacetime requires a certain ideology, such as the notion of three points being on a

[44]The sentence s can be presumed to be contigent if it is to constitute a non-redundant component of a theory of a world.

[45]Whether this *is* the best construal is something I will return to parenthetically below.

> straight line through spacetime; describing Newtonian spacetime requires this ideology and then some further ideology as well: the notion of two points of spacetime being at the same absolute position. Given the added ideology of the Newtonian theory, the spacetime that it describes has more structure, is more complex.[46]

In the case just discussed, then, the two rival theories do not differ in their ontologies: both of them describe the same set of spacetime points. But the Newtonian theory makes a demand on the ontology of spacetime points that the latter does not – namely, that the points of spacetime resolves themselves into relations of *being at the same place*. Since there is nothing in the neo-Newtonian theory corresponding to absolute position, the Newtonian theory contains an additional predicate relative to its rival. Supposing that we conceive of the latter theory as different from the former *only* in that the former makes an additional demand relative to the former, we may then represent the relation between the two theories just as was done a moment ago, with e being the set of spacetime points, $T(e)$ being the neo-Newtonian theory, and $s(e)$ representing this extra demand concerning the classes that the points must resolve themselves into according to the Newtonian theory. In accordance with our argument above, then, there is here a clear sense in which Sider's claim that the more ideologically parsimonious theory, namely neo-Newtonian theory, is more likely to be true goes through – at least assuming that the theories are indeed taken to be related in this way.[47]

But it is not the issue of whether pre-relativistic spacetime should be taken to admit of absolute position that I am interested in here, but rather that of

[46]Sider *op. cit.*, p3.

[47]Of course, one could well object to the idea that the two theories *are* related in this way. If one considers the mathematical models of these theories, it is indeed the case that Newtonian theory differs from neo-Newtonian only in that the former contains a vector field – a 'rigging' – that is absent from the latter; see for example Friedman [1986], Chapter 3, especially Section 3.2. (There he talks of 'Galilean' instead of 'neo-Newtonian' spacetime.) However, one may consider the *theory* of neo-Newtonian spacetime to consist of the proposition that the spacetime contains the structure contained in the corresponding model, *and no other structure*. Were that the case, then instead of the Newtonian theory simply saying *more* than the former, the two theories will positively *exclude* one another. And if that *were* to be the case, then we are back to square one regarding why it is that we should take the greater relative likelihood to accrue to the more ideologically parsimonious theory, and I must simply refer back to my discussion of Cameron in Section 1.

whether we ought to believe in fundamental objects. It is of course implicit in the act of using the neo-Newtonian and Newtonian theories to communicate how ideological parsimony maps onto relative likelihoods that Sider believes the same sort of conclusions will apply to the theories in which there do and do not exist (only) fundamental objects as applied to those theories.[48] It is therefore crucial to consider whether the latter pair of theories stand in relevantly similar relations to one another as the former pair do.

However, it is clear on a moment's reflection that there is a significant disanalogy between the two pairs of theories that prevent the conclusion regarding likelihood that was reached in the case of the spacetime theories – *modulo* our assumptions about how they are to be represented and thus related to one another – from going through in the case of the theories in question. The reason for this is that parthood is a predicate *with ontological implications*, and as such, two theories cannot differ over whether or not they require a notion of parthood to give a complete description of those ontologies *without also differing in their ontologies*. Thus suppose for example that our world is fundamentalist, bottoming out at the level of quarks and electrons (say), and suppose that we take there to be *only* such fundamental particles in that world (so that all putative composite objects, such as tables for example, are analyzed merely as 'fundamental particles arranged tablewise'). Now consider a gunky counterpart of this world, in which those particles resolve themselves into more and more fundamental objects *ad infinitum* and thus is such that the parthood predicate is required to fully describe them. Clearly there are 'additional' objects in that latter world compared to this one, in that there will be objects in that world that lack counterparts in this. It follows from *that*, however, that the fundamentalist theory and the gunky theory each describe worlds that necessarily differ with respect to their ontology; and it follows from that in turn that the theory of a gunky world cannot be represented as that of a fundamentalist world *conjoined with* some extra constraints, for the two theories are about *different things*. Thus while we can concede that the gunky theory contains more in the way of ideology than the fundamentalist theory, and is therefore less parsimonious, there is no obvious way to establish

[48]The 'only' here serves to differentiate the 'anti-nihilistic' theory in which there exist composites of simples in addition to simples and the 'gunky' theory in which there exist no simples at all. As argued above, the same argument affects one and other equally. As already mentioned, the same conclusions will apply in either case.

that the less parsimonious theory is less likely to be true than its rival, since the two theories in this case are not related 'by conjunction' as the Newtonian and neo-Newtonian theories may (arguably) be argued as being. Rather, they are simply *two different theories* of *two different things*. Thus saying that it is somehow obvious that the more ideologically parsimonious theory is more likely to be true in this case is like saying that it is *a priori* more likely that Susie is a banker than that Sophie is a banker and a feminist. But since these theories of Susie and Sophie concern *two different things*, the latter is not the conjunction of the former with another proposition and thus there is simply no way that we can assess their relative likelihoods in the absence of further information. It certainly does not follow *a priori* from the axioms of probability theory in any case.[49]

In summary, then, it is not at all obvious that the notion of ideological parsimony in itself has any bearing on relative likelihood – at least not unless it is accompanied by special constraints on the logical forms of the theories that are being compared. But while the spacetime theories that were discussed for the purpose of illustration may perhaps be regarded as satisfing these constraints, they do not seem to be satisfiable in the case of present concern. Of course, one may, in the face of this objection, defend the idea that the theory that dispenses with parthood is more likely to be true merely by reference to the fact that ideological parsimony is (so it is claimed) valued in theories of physics. But if that is the avenue taken, then we must again confront head-on the issues raised in the last section concerning the difficulties of importing features valorized as epistemic virtues in the case of empirical theories into the context of theories of metaphysics. It therefore seems that we find ourselves staring in the face once again the same problems that plagued Cameron.

I conclude at this stage that neither of these two arguments drawn from the contemporary metaphysics literature aiming at fundamentalist conclusions via theoretical virtues have been at all convincing. There is simply no obvious route from the nice-making features that metaphysical theories are supposed to possess to rational beliefs that this world is amenable to fundamentalist

[49]Again, though somewhat extraneous to present concerns, exactly the same thing may be said, *mutatis mutandis*, of the nihilist theory of fundamentalist worlds and its non-nihilistic counterpart.

description. Given that Cameron's and Sider's arguments are, to my knowledge, the only serious arguments to be found in the current metaphysics literature as to why we ought to believe in a fundamental level, and given that each has been found deeply wanting, the prospect that science might be marshalled to argue against fundamentality remains, at this stage, very much a live option.

Chapter 4

The View from Science: *A Posteriori* Arguments For and Against Fundamentality

In the last chapter, I showed that the arguments circulating in the contemporary metaphysics literature for the existence of a fundamental level are not at all persuasive. The space thus seems to be clear for asking whether we can use *science* to argue against the existence of such a level, and in this chapter I will begin to consider how we might go about doing so. First, however, a little more groundwork is required. The reason is that, as I made clear at the start, I hold that a *naturalistic* approach to metaphysics should be adopted wherever possible. The fact that armchair metaphysics fails to provide us with good grounds for assuming a fundamental level can therefore hardly be taken to exhaust the competition. What we must also contemplate is whether naturalism itself somehow enjoins us to commit to fundamentalism, and it is this issue that I want to address now.

4.1 Methodological Grounds for Fundamentalism?

An obvious way in which one might try to argue that naturalism itself entails commitment to fundamentalism would be by appealing to scientific methodology. Given that physicists often describe the basic business of physics as the search for the 'deepest layers' of reality, the 'ultimate building blocks' of the world and the 'most fundamental' laws of nature, one could easily get the impression that belief in fundamentality and the very practice of physics go hand in hand with one another.[1] The fundamentalist might therefore cite this feature of physicists' own conception of their enterprise in support of their view. If, after all, it turns out to be a *presupposition* of the scientific enterprise that there is a fundamental level, how could the naturalistic metaphysician possibly find herself in a position to deny that there exists such a thing?

While the view just cited might represent the majority view amongst physicists, there are nonetheless both philosophers of physics and physicists themselves who reject this idea that a fundamentalist perspective is demanded by physics practice. Bohm, for example, held that the physics was perfectly consistent with the 'qualitative infinity of nature', and that choosing to regard the world as bottoming out into fundamental entities was reflective of a purely philosophical prejudice, and not in any way dictated by either the evidence or practices of science. As he wrote,

> the mechanistic thesis that certain features of our theories are absolute and final is an assumption that is not subject to any conceivable kind of experimental proof, so that it is, at best, purely philosophical in character.[2]

But Bohm did not hold that questions of fundamentality should thereby be regarded as wholly underdetermined, since according to him there are in fact good methodological reasons for preferring the anti-fundamentalist stance. He held that it is more *useful* for physicists to suppose that what they are studying at any given time is just a limited portion of the 'qualitative infinity of nature', since that supposition

> constitutes a broader point of view, in the sense that it contains within it all of those consequences of mechanism which represent

[1]For a variety of statements to this effect, see, for example, the testimonies of the particle physicists who were engaged in the debate with condensed matter physicists over the construction of the superconducting supercollider, described in detail in Martin [forthcoming]. This was in many ways a debate over the *meaning* of the term 'fundamental' in physics, and although it is a fascinating episode I will only refer to Martin's discussion of it here.

[2]Bohm [1957], p132. I will discuss the possibility of anti-fundamentalist hypotheses being empirically testable in Chapter 5 below.

> a genuine contribution to the progress of scientific research, while it does not contain those which make no such contribution and which impede scientific research... [N]ot only can nothing of real value for scientific work be lost if we adopt the notion of the qualitative infinity of nature in the specific form that has been described here, but on the contrary, much can be gained by doing this.[3]

For Bohm, to adopt the fundamentalist perspective is to risk missing out on valuable new contributions to scientific knowledge, while adopting the contrasting perspective may offer rich gains.

The same sentiment that anti-fundamentalism constitutes the most natural and fruitful scientific world-view may also be found in Popper. It is perhaps somewhat predictable that Popper might hold such an outlook, since fundamentalism is rather disconsonant with his portrait of scientific activity as a process of successive 'conjectures and refutations'. After all, if scientists are always engaged in attempts to falsify, and thus to go beyond, even our best current theories, then it must make sense to deny that scientists are ever committed to the fundamentality of those theories. And if we see science – and thus, for Popper, this process – as continuing without limit, then a positively anti-fundamentalist perspective quickly follows suit. Popper himself explicitly committed to such a view. As he put it,

> the task of science constantly renews itself. We may go on for ever, proceeding to explanations of a higher and higher level of universality – unless, indeed, we were to arrive at an *ultimate explanation*; that is to say, at an explanation which is neither capable of any further explanation, nor in need of it.
>
> *But are there ultimate explanations?* [...] I do not believe in the essentialist doctrine of ultimate explanation. [...] Although I do not think that we can ever describe, by universal laws, an *ultimate* essence of the world, I do not doubt that we may seek to probe deeper and deeper into the structure of our world or, as we might say, into properties of the world that are more and more essential,

[3] *Ibid.*, pp134-6.

of greater and greater depth.[4]

Popper's vision of scientific knowledge may therefore be described as one in which there is continual *progressivism* without *perfectionism*, and as such it may be described as an anti-fundamentalist world-view.[5]

The view that the practice of physics in itself somehow imposes fundamentalist commitments upon us is therefore one that has some illustrious critics. It is, however, abundantly clear that the interminable progress of scientific research and limitless accumulation of knowledge that Popper and Bohm envisage is only possible if there is in fact no fundamental level. (After all, the fact that scientists could continue to *ask* new questions of nature indefinitely need not mean that those questions will in fact turn out to have new answers.) As such, appealing to the view of science as an endlessly progressive enterprise in any attempt to defend anti-fundamentalism cannot but beg the question. Nevertheless, the very *coherence* of the idea that physics might fruitfully take place against an anti-fundamentalist background undercuts any temptation to think that there are substantive methodological reasons, based on some stated 'aims' of science, that force the naturalist to buy into fundamentalism. In order to assess whether there is anything in the concept of naturalism that should incline us towards fundamentalism, we should therefore consider whether there are any *epistemic* considerations that might marshal support for this view.

4.2 Epistemic Grounds for Fundamentalism?

An obvious epistemic consideration we might invoke in the attempt to extract fundamentalism from naturalism is, of course, the audacious success of the reductive paradigm since the time of Newton.[6] Given that many theories of nature have assumed the existence of fundamental entities and been so outlandishly successful, we might try to argue that the assumption of fundamentality has been indirectly confirmed along with those theories. Indeed, it

[4] Popper [1972], pp194-196.

[5] Cf. Schaffer [2003], p404.

[6] Many historians of physics hold that this paradigm became genuinely successful only with the work of Newton; see e.g. Friedman [2001], Lecture 1.

is hard to imagine what *better* justification a naturalist could hope to have for belief in fundamentality than that our best theories of physics have posited it and were subsequently highly confirmed.

But it is immediately obvious that such an argument has little hope of convincing anyone. Even if we focus just on the twentieth century, it cannot be disputed that there have been plenty of theories that treated their subject matter as fundamental and were highly successful in spite of the fact that their fundamentality assumptions subsequently turned out to be wrong.[7] It is therefore clear that the truth of any fundamentality assumptions that a theory might contain is by no means a necessary condition on the success of that theory.[8] Thus the obvious epistemic route from the success of fundamentalist science to belief in fundamentality seems blocked off at the outset by history.

This point that the historical success of fundamentalist science does not in itself constitute an argument for fundamentalism has been put forward somewhat recently by Schaffer.[9] However, Schaffer wants to go much further than merely undermining the idea that the success of fundamentalist science lends support to fundamentality assumptions, for he argues that the history of science in fact gives us reason not just to *fail to commit* to fundamentalism, but to adopt a *positively anti-fundamentalist* stance. Schaffer's is in fact the *only* major argument in the extant literature for such an anti-fundamentalist

[7]Early, highly successful theories of nuclear physics, for example, supposed protons and neutrons to lack internal structure; likewise, as hadron physics proper came into being in the 1950s and early 60s, hadrons in general were taken to be elementary particles. How this latter point of view came to be abandoned will be described in Chapter 8.

[8]It should be noted that success of a fundamentalist theory despite the falsity of its (implicit or otherwise) fundamentality assumptions can be attributed simply to the fact that the relevant more fundamental entities were *empirically inaccessible* in the period in which the theory was successful. This empirical inaccessibility thus need not impinge upon the fact that the qualitative properties of the non-fundamental entities described in the theory, bar their fundamentality status, were correctly described in that theory. The fact that a fundamentalist theory can be successful in spite of the falsity of its fundamentality assumptions is therefore much less troubling than the claim made by anti-realists in the context of the 'pessimistic meta-induction' (cf. Laudan [1981]). The latter, of course, is that theories can give descriptions of nature that are *wildly qualitatively inaccurate* and yet be empirically successful, not simply that they fail to provide an *exhaustive* description of nature and be successful.

[9]Schaffer [2003].

conclusion.[10] Since the question we are currently concerned with is that of whether we can use physics to deny fundamentality, then if Schaffer's argument can be shown to succeed in its objectives it seems that the job will have been done – and the question answered in the affirmative. It is therefore imperative to examine Schaffer's argument, and I will do so carefully now.

4.3 Introducing Naturalistic Anti-Fundamentalism: Schaffer's Meta-Induction

Schaffer's challenge is directed to the assumption that there exists a set of fundamental objects that ultimately compose everything. It is therefore, as presented, an argument against fundamentality mereologically construed. His argument consists of a reflection on over a century of developments in the study of the structure of matter, and he urges that in spite of the success of the theories involved, the more scientifically informed position does not – as might at first have been assumed – sanction belief in a fundamental level at all. In fact, quite the opposite is true. Though annotated with facts and observations of various sorts, Schaffer's core argument is easy to summarise; it is (what we might call) a meta-induction, and in a nutshell it is this.

> The history of science is a history of seeking ever-deeper structure. We have gone from 'the elements' to 'the atoms' (etymology is revealing), to the subatomic electrons, protons and neutrons, to the zoo of 'elementary particles', to thinking that the hadrons are built out of quarks [...] Should one not expect the future to be like the past?[11]

[10]I am not aware of any *a priori* arguments for similar anti-fundamentalist conclusions. See, however, Arntzenius [2008] for a (somewhat) *a priori* argument that a Whiteheadian, 'gunky' structure for spacetime is to be preferred over its 'pointy' rival, on grounds of parsimony considerations.

[11]Schaffer [2003], p503. Note that the examples he chooses are not all on a par with one another: that the relationship between 'the elements' and 'the atoms' is compositional in anything like the sense that atoms are composed of nuclei and electrons is far from clear. But I will not pursue this here.

In other words, the claim is that, since progress in the study of matter has largely consisted of instances of fractioning entities thought to be fundamental into the more fundamental entities they are composed of, it is better in keeping with the history of physics to positively deny the existence of a fundamental level. As noted, the claim is that we have good inductive and naturalistic grounds for denying, in particular, mereological fundamentality, though history presumably has similar implications for fundamentality theses that are cashed out in supervenience-based or nomological terms.[12]

It should be immediately clear that if Schaffer's argument succeeds in its ambitions, it will be a remarkable result. The question of the infinite divisibility of matter was, after all, one of Kant's antinomies.[13] A clear demonstration, on the basis of history, that one should not believe in fundamental entities would dismantle an edifice of prevalent contemporary metaphysical thinking in strikingly succinct terms. And there are some who believe that it *does* succeed. Though citing discomfiture with certain aspects of Schaffer's approach – specifically his use of mereological concepts in expressing priority (cf. the discussion in Chapter 2, Section 3) – Ladyman and Ross are tentatively supportive of the spirit of Schaffer's proposal, citing that

> arguably we do have inductive grounds for denying that there is a fundamental level since every time one has been posited, it has turned out not to be fundamental after all.[14]

[12]Callender [2001] notes his misgivings about the fact that Schaffer's discussion concerns particles and compositional relations. This, Callender feels, is illegitimate on the grounds that the 'fundamental particles' are today conceived of as fields, and that although fields are 'in some sense infinitely divisible', they are only 'horizontally' so. But it seems to me that this objection has, as it stands, yet to be fully made out, since particle physics does apparently recognise a distinction between fundamental and composite *fields*. There is, for example, currently an open question in 'beyond the Standard Model' physics of whether the Higgs field ought to be regarded as fundamental or as a composite of top quark fields; Salam [1979], for instance, is full of examples of models utilizing composite lepton and gauge fields. But note too that Callender himself recommends that we be charitable and understand Schaffer's argument 'loosely' in less contentious, supervenience-based terms (which is what Brown and Ladyman *op.cit.* also do), and the argument against Schaffer that I will adduce below will apply equally to either construal.

[13]Kant [1965], A435 / B463.

[14]Ladyman and Ross [2007], p178. Of course, what they are saying is not quite right insofar as the entities we *currently* regard as fundamental have not yet turned out to be otherwise!

It is therefore clear that Schaffer's argument has its supporters. I, however, do not recommend that we join them in endorsing his meta-inductive approach. However appealing it may in some sense be – and though it should certainly give the fundamentalist *pause* – Schaffer's argument fails both to secure its anti-fundamentalist conclusion and, moreover, to exemplify the naturalistic approach that I seek.

To cite the first problem with the argument, and one that Callender has highlighted, Schaffer surely stretches the inductive evidence – a handful of cases – beyond breaking point to take it to support the infinite amount of work that the argument needs it to do.[15] As goes without saying, science must itself inevitably use forms of induction, including the simple enumerative induction that Schaffer deploys in making his case.[16] Furthermore, sometimes scientists do themselves draw conclusions, as Schaffer does, via enumerative methods on the basis of relatively few observations, as when the boiling point of a chemical substance is inferred through a small number of repeated experiments.[17] However, the legitimacy for the latter inductions is typically underwritten – or at least is taken to be underwritten – by appeal to the fact that such inductions relate the members of a given 'natural kind'.[18] But the idea that the *particles in general* – that is, the totality of particles that are, or ever will be, studied in physics – comprise a natural kind in anything like the sense that a given particle or chemical kind does will obviously not stand up. Schaffer therefore cannot likewise defend the slimness of his inductive base on these grounds. We may note further that the induction involved in Schaffer's argument is from a known domain into domains that we *ex hypothesi* know nothing about. Yet there is surely a gulf between inducing that, for example, unobserved protons will behave as the observed protons do, and inducing that a set of entities about which one can say almost nothing will continue to possess features that observed particles, such as protons, do. In particular, in the latter case we cannot usefully exploit the inductive principle that 'things that are similar in some respects are liable to be similar in others', so this cannot be invoked to support Schaffer's inference either.

[15] Callender [2001], p3.
[16] See, e.g., Earman and Salmon [1999], Chapter 2.
[17] Cf. Norton [2003], p649.
[18] *Ibid.*

There are thus deep problems associated with inductively projecting from the historical evidence in the manner that Schaffer does. A second significant problem, however, is that Schaffer's use of that historical evidence arguably begs the question. Indeed, this is not just Schaffer's problem, for it seems that *any* historico-inductive argument aimed at establishing a fundamentality-related conclusion must be guilty of the same fallacy. To see this, note that Callender claims that given 'the simple fact that science has (virtually) always gone for a fundamental level', it follows that the history of science does not support an infinite descent more than fundamentalism – if anything quite the opposite'.[19] Thus Callender takes the historical track record to speak in favour of fundamentalism – a conclusion that is diametrically opposed to that which Schaffer draws. So which conclusion *does* the history of physics support?

Let us grant that Callender is right that science does 'virtually always' posit a fundamental level, and that Schaffer is also right in that these posits – at least until the last such posit – have all been refuted. Then *both* of these sets of facts constitute the historical evidence in play. Given that evidence, it seems that one could claim, as Schaffer does, that the historical process of refutations of fundamentality assumptions implies that there is no fundamental level *only if* one also assumes that the historical process of successively postulating a fundamental level will repeat forever – or, in other words, if one assumes that there is no fundamental level. Likewise, one can argue alongside Callender that the repeated postulation of a fundamental level supports the idea that such a level exists *only if* one also holds that the process of subsequent refutation will come to an end – or, in other words, if one assumes that that there is a fundamental level. It therefore appears that the argument for *either* conclusion on the basis of the historical evidence must simply beg the question at hand. And it is hard to see how any such meta-induction, whether for or against fundamentality, could avoid doing the same.

Each of these objections represent grave problems for Schaffer's basic strategy. But there is a yet more pertinent and structural difficulty with his approach from a naturalistic point of view. Consider again the picture that Schaffer is offering us. It is a picture in which that which was thought to be fundamental

[19]Callender *op. cit.* Callender's is, of course, a statement of the widely-held idea that the reductive paradigm in science supports fundamentalism.

is revealed as being ontologically secondary to other things. Again and again the assumed fundamental basis changes, but what remains the same throughout is the *nature* of the priority relation connecting the various levels, and indeed its *structure*. (One obviously cannot infer from an observed finite segment of a chain of partially-ordered priority relations to its being infinitely long without already assuming that the relations will continue to form a chain in the hitherto unobserved regimes.) But since the fundamental is standardly defined as that which is ontologically secondary to nothing, it is ontological priority that constitutes the central concept in any fundamentality debate. If we want that debate to be naturalized, then surely *we cannot permit questions of either the nature or the structure of that relation to be insulated from the jurisdictions of physics* – any more than we want the question of their well-foundedness to be so insulated.[20] It would surely, in any case, be *unwise* to make assumptions as to the priority structure that will be suggested to us by the metaphysics of future physics.

This idea that it would be unwise to project contemporary priority assumptions into hitherto unknown regimes has already been mooted by Bohm. In the context of his discussion of the 'qualitative infinite of nature', he writes:

> We are not supposing that the same pattern of things is necessarily repeated at all levels, and secondly, we are not even supposing that the general pattern of levels that has been so widely found in nature thus far must necessarily continue without limit... More generally still, it is evidently quite possible that as we penetrate further still, we will find that the character of the organization of things into levels will change so fundamentally that even the

[20]Note that when I presented the levels structure above in Chapter 2, I (i) provided a variety of candidate priority relations, without presenting those relations as exhaustive; (ii) was happy to concede that some of these relations – including mereological relations – may simply become inapplicable at some point in the levels hierarchy, and (iii) did not lay down any *a priori* prescription on the logical form of any of these relations. I did not insist, for example, that compositional relations are necessarily asymmetric, nor that supervenience was either – for I defined the latter merely as the failure of independent variation of A given B, which does not preclude, for example, that B might likewise not vary independently of A. Thus in presenting the levels structure, *I* did not assume that priority relations must take some pre-meditated logical form. Schaffer, on the other hand, does. And I do not find that naturalistically acceptable.

> pattern of levels itself will eventually fade out and be replaced by something quite different... This notion [of the qualitative infinity] does not require *a priori* the continuation of any special feature of the general pattern of things that have been found thus far, nor does it exclude *a priori* the possibility that any such feature may continue to be encountered, perhaps in new contexts and in new forms, no matter how far we go. Such questions are left to be settled entirely by the results of future scientific research.[21]

One could even claim, in fact, that this possibility of priority assumptions being subject to revision in the face of physics is not a mere possibility, but in fact already realized. We have already seen above in Chapter 2, Section 3 that it has been claimed that mereological relations may take on logical forms in the quantum context than are different to those postulated in classical mereology. But additionally, the fact that the property supervenience structure exhibited in composite quantum systems can be argued to be (in some sense) the opposite of what would have been expected classically arguably also confirms that *a priori* assumptions regarding priority structure in hitherto unknown regimes are apt to go awry.[22] Given the surprises that modern physics has thrown at us, it would not be outrageous to hazard that the only safe inference to make from the history of physics is that we essentially have *no idea* of what it is going to throw at us in the future, beyond that there will be correspondence in the limit.[23] As Oppenheimer described how he would place his bets,

> Physics will change even more... If it is radical and unfamiliar... we think that the future will be only more radical and not less, only more strange and not more familiar, and that it will have its own new insights for the inquiring human spirit.[24]

[21]Bohm *op. cit.*, p139.

[22]A classic paper on this issue is Teller [1986].

[23]Cf. Post [1971]. Of course, correspondence in the limit is consistent with the successor theory being radically different from the precursor theory – as history itself counsels us. And although, as Post also reminds us, we can fruitfully use certain features and pathologies inherent within current theories as heuristics for constructing new theories, such features remain just that – *heuristics*.

[24]From the transcript of Oppenheimer's 1953 BBC Reith Lectures, quoted in Salam [1979]. (While Oppenheimer and I differ on many, many things, this is one thing that I think we can agree on.)

But if physics can change in ways that we cannot yet hope to envisage, facts about priority relations between physical entities may be presumed to be subject to change too. Therefore, given that meta-indutive arguments against fundamentality necessarily bank on the idea that a given priority relation, with a structure imposed either *a priori* or on the basis of past observation, is going to be relevant infinitely far into the future of high-energy physics research, I suggest that naturalism counsels us that Schaffer's argument is not one that we can accept.

In summary, then, it seems that there are insuperable problems with historic-inductive arguments against fundamentality such as Schaffer's in that they (i) rest on wild inductive leaps into (by assumption) infinite domains that we know almost nothing about; (ii) beg the question at issue, and (iii) necessarily rest upon speculative assumptions regarding the metaphysics of future physics that are surely at odds with the naturalistic agenda.[25] As such, while Schaffer's argument certainly *problematizes* the fundamentalist's assumptions, given its failure to secure the sought-for anti-fundamentalist conclusion I do not think it recommends anything more than agnosticism, pending further argument, regarding the existence of a fundamental level. In particular, there is just no escaping the fact that the historical record cannot inductively support the expansive conclusion Schaffer needs it to sustain.

Nevertheless, Schaffer's argument represents a first stab at using science to deny the existence of a fundamental level, and as such we should treat the criticisms of it in as constructive a fashion as possible. Let us therefore now consider how, if at all, the problems just raised might inform us of a more

[25]The reader may be wondering why I regard Schaffer's argument to be so weak, given that the pessimistic meta-induction – which is *also* historico-inductive in structure – is regarded as a devastating argument against scientific realism (cf. Laudan [1981]). The answer is that, despite the surface similarity, there are critical differences between the two arguments. Most saliently perhaps, the PMI uses a handful of cases from the history of science to argue that we should regard our current best theory (or theories) as false. Thus it moves from a small number of theories to extrapolate a conclusion about another small number of theories. Schaffer, on the other hand, uses a small number of cases to make an induction about *infinitely many* theories, and moreover theories that we know nothing about, and is therefore much more problematic *qua* inductive argument than the PMI. (Note also that it is *essential* that Schaffer moves 'infinitely far' beyond current theories, for even the fundamentalist may be happy to admit that the entities that are presented as fundamental in current theories may not in fact be fundamental.)

fruitful naturalistic approach to denying fundamentality.

4.4 Introducing the Internal Approach

One thing that the problems raised above make clear is that the history of science is compatible with the fundamentalist *and* the anti-fundamentalist possibilities.[26] It seems we may therefore conclude that patterns in the history of science do not constitute a good starting point for defending an anti-fundamentalist worldview. But given that, in the absence of clairvoyance, speculations as to the *future* of science surely cannot constitute an acceptable naturalistic basis for argument, it seems that the only viable alternative is to approach the issue directly through the structure and content of a *given, extant* scientific theory, and such a theory alone.[27] It therefore appears unavoidable that if we are to avoid the problems that blight Schaffer's speculative approach, arguments against fundamentality should always be formulated not from *historical patterns between* theories, but from *within the perspective* of a physical theory that we already have in hand, understand, and know how to use. What we therefore need to do, I claim, if we want to use physics to argue against the existence of a fundamental level is to investigate whether there exist physical theories that can be argued to imply that there is no such thing. Arguments against fundamentality that have this form I will call, with a nod to Lakatos, *internal* arguments against fundamentality.

Adopting such an internal approach to denying fundamentality would without question be thoroughly naturalistic. Insofar as fundamentality questions are framed with respect to a given theory, which describes in detail a given portion of reality, arguments developed through the internal approach would not rest upon speculations as to the content of physical regimes about which we

[26]Indeed, the case can easily be made that not only is that history *consistent* with each, but to be *expected* by each.

[27]By 'extant scientific theories', I include both empirically successful, recognizably 'mature' theories that we commit to at present, as well as theories that have that enjoyed such status in the past *even though* they have since been discarded. The reason I will include past theories in my discussion is because I am primarily interested in the question of whether it is *possible* to use physical theories to deny fundamentality, not so much in that of whether we *should in fact* deny fundamentality. More on this follows below.

can say nothing. Such a theory would also bring in its train an ontological package, so that the set of priority relations appropriate to it, and indeed the structure of those relations, may be surveyed from within the perspective of that theory. Were we to adopt this approach to denying fundamentality, then, we would circumvent the need to simply stipulate what relations will be relevant, and what logical form they will take, in regimes infinitely removed from the purview of current theories. And given that, as Callender points out, science 'virtually always' posits a fundamental level, it would clearly not be question-begging of the anti-fundamentalist to approach the issue in this way.

We should also be clear that there is *in a sense* nothing new about this approach to anti-fundamentalism, insofar as it is simply the mirror image of (what I take to be) the most convincing naturalistic motivation one could have for committing to fundamental entities – namely, that our best physics supports it.[28] According to the proposed approach, we should likewise deny the existence of fundamental entities when and only when our best current theories recommend to us that there are no such things. But while the strategy is in some sense already familiar to us, and the potential advantages of it are clear, what is much *less* clear is that realistic physical theories *do in fact* have the capacity to positively deny, as opposed to assert, fundamentality assumptions. While as naturalists we might *like* it if the absence of a fundamental level could be argued for through physics and not remain the purview of purely armchair speculation, there is a legitimate worry that the proposition that reality extends to infinite depth is simply so metaphysical in character,

[28] Of course – and as I have already pointed out – Schaffer's argument should breed a healthy scepticism about the idea that the success of a theory that made fundamentalist assumptions is indicative of the truth of those assumptions. Nonetheless, since as a naturalist I do not believe that metaphysics ought to be approached from some Archimidean point, but rather always *at some time* and *from the perspective of some physical theory*. Therefore if we are given a highly successful theory that posits fundamental entities, and if we have at that time no reason to doubt on empirical grounds that those entities are fundamental, then I believe that the rational thing to believe in is the fundamentality of those entities. That of course does not imply that such a belief should not be regarded as *defeasible*. Nonetheless, what I want to investigate here is whether there have been theories that themselves imply (defeasible) anti-fundamentalist conclusions. Since it typically seems to be assumed (cf. Schaffer and Callender's discussion) that theories 'always' posit fundamental entities and it is only ever history that proves them wrong, *that* is the novel aspect of the discussion I am about to engage in.

so far removed from experience, that it is just not the sort of claim that can admit of empirical support. Furthermore, in the pictures painted by Schaffer (and indeed Popper), anti-fundamentalism is equated with (something like) the successive falsification of successive theories. How, then, can we use *a* theory to ground anti-fundamentalist claims?

My principal purpose in the remainder of Part 1 is to show that these worries can be overcome – to show, in other words, that it *is* possible to deny fundamentality through the internal approach, and thus to do so through genuinely naturalistic means. I will argue for this claim by showing that physical theories have already been developed whose internal logic can be used in support of anti-fundamentalist interpretations: that it is *not the case* that theories of matter must assume fundamentality and that it is only ever history that proves those assumptions wrong. This I will do through two case studies. The first will be a theory of particle physics, and in particular a theory of the strongly-interacting particles, from the late 1950s and early 60s. This is the *Analytic S-matrix theory* of the strong interactions, to be discussed in the next chapter.[29] The chief protagonist of this theory, Geoffrey Chew, pushed the anti-fundamentalist implications of this theory almost from its inception, and in particular pressed the idea that it can be marshalled in support of the idea that there is *no mereological fundamentality* (that our world is a 'gunky' world, if you will).[30] Since compositional structure is taken to be the 'central connotation' of priority structure, this is a nice place to start.[31] Nonetheless, and as I will be the first to point out, this theory may still be deemed to constitute a rather odd starting point from which to address the issue of whether we can deny the existence of a fundamental basis to the actual world

[29]This is also sometimes known as the 'bootstrap theory' of strong interactions.

[30]It should be noted, however, that since the S-matrix theory concerns only the strongly interacting particles, it has nothing to say on the existence or non-existence of fundamental particles of any other sort (such as leptons). Nonetheless, the existence of even a proper subset of objects for which the 'chains of dependence' do not terminate is sufficient to refute the idea that the world possesses a mereologically fundamental basis.

[31]Schaffer [2003], p500. Recalling my comments in Chapter 2, Section 3 above, I reiterate that with the term 'mereological' I do not wish to connote a commitment to any purely philosophical theory of composition, such as with 'fusion' and an *a priori* prescription on logical form, etc. Rather, I will attempt below to extract the appropriate logic of part-whole relations in this context from the S-matrix theory's own assumptions.

through science; the theory has, after all, barely seen the light of day since the 1960s. However, the principal question I am interested in answering is not so much whether we *should in fact believe in* anti-fundamentalism about the actual, but whether *it is possible* for physics to deny fundamentality. If a respectable physical theory can be found that has these implications, then presumably the answer to that question is *yes* – even if that theory, and hence the case it makes for anti-fundamentalism, is subsequently disconfirmed.[32] Viewed in this light, the choice is in fact very fitting since S-matrix theory is both well-understood and contains a number of theoretical and empirical features that are highly relevant for this purpose. Indeed, the very means through which it ultimately came to be rejected will prove to have interesting consequences for the present project.

Despite that, it would be nice if there were an example of a live theory that is taken to have anti-fundamentalist implications – for if those implications can be sustained, we will be in a position to believe not just that physics *can* deny fundamentality, but that it *does* deny it (and thus that so should we). As it turns out, there is arguably just such a theory (or better, *framework* for theories) – namely, *quantum field theory* and the so-called 'effective' approach to it.[33] Indeed, while criticising Schaffer's meta-inductive strategy, Callender notes that 'Schaffer perhaps misses the best support he has in science by neglecting a recent debate concerning effective quantum field theories', and thus some discussion of the matter here seems almost inevitable.[34] On account of its live nature, this case has received more discussion in the recent philosophy of physics literature than the previous one. However, the

[32]Indeed, to appreciate that it is worthwhile considering previously successful but now defunct theories in assessing whether it is possible to use physics to argue against fundamentality, one need only imagine that we are trying to do so 50 years ago, and thus during the period in which it was the S-matrix theory that held sway in hadron physics. An argument for the conclusion that *we should in fact* deny fundamentality would then have been framed with respect to this theory. This sanctions weaker conclusion that *it is possible for* physical theories to have anti-fundamentalist implications.

[33]Though I will not discuss it here, there is also a growing body of literature on how dualities in string theory can undermine fundamentalist assumptions: see for example Rickles [2011], Section 3.2, and Castellani [2009]. While I have chosen not to attempt to interpret these here, it is my hope that they will be included in the prospective next phase of this project.

[34]Callender *op. cit.*

arguments for anti-fundamentalism that have already been made in this context have been almost universally criticized, and it will be worthwhile to consider afresh the physical and philosophical assumptions required to extract anti-fundamentalist conclusions from this theory. That will be the topic of Chapter 6.

The point of these case studies is therefore to argue that questioning the existence of a fundamental basis can be regarded as just as legitimate a topic for naturalistic metaphysics as more familiar topics in the metaphysics of physics. And, given that fundamentality issues are almost by definition connected to everything else in metaphysics, we can expect the discussion I am about to engage in to connect with other key issues in the philosophy of science. But, of course, these claims are – at least at this point – more than a little premature. So let me now backpedal somewhat and go back a little over half a century to the childhood of accelerator physics, and to the theoretical *impasse* that occasioned the introduction of a new approach to strong-interaction theory.

Chapter 5

Arguing Against Fundamentality 1: The Analytic S-Matrix

5.1 Introducing the Analytic S-Matrix

By the turn of the 1960s, quantum field theory (QFT) was experiencing critical complications. The fundamental conceptual problem was that relativity requires fields to be defined at localized space-time points, which – via the Heisenberg uncertainty relation – implies that field interactions cannot be finite in energy.[1] A great number of theoretical studies seemed to confirm that an 'insuperable pathology' plagued the concept of the local field interactions, as can be appreciated by flicking through the proceedings of the 12th Solvay Conference in 1961.[2] While it was known that these divergences could be formally tempered via the process of renormalization, that procedure, at least then, was regarded with deep suspicion.[3] Aside from the crises in QFT in general, however, specific problems faced the possibility of a field theory of the strong interactions in particular. The renormalization procedure that was developed by Tomonaga, Schwinger and Feynman took place within the framework of perturbation theory, but the large coupling constant associated with the strong force at hadronic distances – around $g = 15$ – rules out the use perturbation theory here. The prospects for a field theory

[1]See Cushing [1990], pp18-19.
[2]See Chew [1968b], p763.
[3]Renormalization will be discussed in Chapter 6.

of the strong interaction therefore seemed highly remote, and with it the prospect of a relativistic quantum theory of the hadrons. Fortunately, however, another approach was waiting in the wings, largely thanks to the work of Heisenberg. Some years before, in 1937, Wheeler had introduced the concept of the scattering matrix, or 'S-matrix', into nuclear physics.[4] Heisenberg then proposed, in 1943, that this object should be made fundamental in a root-and-branch revision of the whole approach to relativistic quantum theory.[5] His proposal was that we try to circumvent the route from the Hamiltonian to the S-matrix – a route which, by the late 1930s, was well-known to be plagued by divergence difficulties – and to work with the S-matrix directly.[6] It was this basic idea in Heisenberg's work that laid the foundation of the theory to be examined here.

The S-matrix is in one sense a very simple object. Like any matrix, it is an array of numbers. The elements S_{ij} of this matrix encode the probability of obtaining a state j of free particles as the output of a collision event given that state i, another state of free particles, serves as the input. These probabilities can be directly inferred from experiment (provided, of course, that the relevant experiment can in fact be performed). Since all that quantum mechanics predicts are these probabilities of measurement, the totality of such elements would constitute the entire empirical output of any theory of quantum particles.[7] Thus if one could find a method of reliably and (at least 'in principle') exhaustively computing these elements, one would have a claim to possessing a complete quantum theory of the strong interactions. This in any case was the view of Geoffrey Chew, the chief architect of the theory that came to be based on this object. Chew took it that

> since elements of the S-matrix describe all hadron experiments, ability to predict this matrix would constitute a complete hadronic theory.[8]

Heisenberg's strategy for predicting the elements of this matrix while bypass-

[4]See Cushing [1990], pp30-1 and Chew [1966], p4.

[5]References to the Heisenberg's works in this field may be found in Cushing [1990], p33.

[6]Heisenberg himself referred to the S-matrix as the 'characteristic matrix'.

[7]Thus 'general quantum theory makes no predictions beyond those made by S-matrix theory' (Stapp [1971], p1303).

[8]Chew [1968b], p763.

ing the use of any Hamiltonian was based on the idea that certain *constraints* placed upon the matrix as a whole would suffice to determine its individual elements. For example, Heisenberg demanded that all of the elements had to be Lorentz invariant functions of the particle variables (so that it would be suitable for relativistic regimes), and that the matrix had to be unitary (in order to respect the basic principles of quantum mechanics). However, Heisenberg failed to identify a principle that the S-matrix had to satisfy that might determine the results of experiments involving *interacting* particles. He could not find anything, in other words, that could mimic the codification of forces in the traditional Hamiltonian method. In consequence, it was not long before Heisenberg lost interest in his attempt to build up relativistic quantum theory on the basis of the S-matrix alone.[9] Instead, he threw all of his efforts behind developing a field-theoretic approach of his own.[10]

According to Chew, however, Heisenberg gave up too quickly. In Chew's eyes, the problem was that

> the property now called maximal analyticity was not appreciated in the forties... and without this notion S-matrix theory lacked dynamical content. Heisenberg and the other S-matrix students of that period eventually lost interest when they realized they had no way to compute interparticle forces, and more than a decade elapsed before the S-matrix was resurrected as a competitor with quantum field theory.[11]

When this crucial 'analyticity' postulate was added to the principles already taken to govern the S-matrix, however, a qualitatively new theory, *Analytic*

[9]Heisenberg grew to deplore his attempt as at best a substitute or proxy for a genuine theory, eventually holding that 'the S-matrix is an important but very complicated mathematical quantity that should be derived from the fundamental field equations; but it can scarcely serve for formulating these equations' (Heisenberg [1957], p270).

[10]This was Heisenberg's 'unified field theory', in which it was taken that a single field described all matter and all forces. The more specialized field theories, such as QED, were conceived of as low-energy approximations to this theory. It has a superficial similarity to S-matrix theory in that there is no distinction between matter and force fields, but was intended as much wider in scope. It did not make much traction with mainstream physics, though it was pursued at length by H.P. Dürr. See Chew [1962], p4 for a brief discussion of this and Cassidy [1991], pp539-543 for an outline of the theory and its reception.

[11]Chew [1966], p.4; also Chew [1968a], p.65.

S-matrix theory, came into its own. This theory may, moreover, be argued to contain deep within it some radical anti-fundamentalist implications, and in this chapter I will discuss why that is. My strategy will be as follows. In the next two sections, I will give an outline of the basic framework of S-matrix theory. The first section introduces some of the mathematical apparatus, while the second introduces the axioms of the theory and sketches how they delivered the detailed dynamical picture that Heisenberg missed out on. With this in place, I will then discuss the arguments as to why S-matrix theory can be interpreted to preclude fundamentality. The discussion that follows draws throughout on material that is covered in more detail in the flagged-up sections of the Appendix that may be found at the end of this thesis. The basic dialectical moves will nonetheless all be elucidated here in the chapter. To be clear, however, any equation that is referred to without being explicitly presented here in the chapter may be found in that Appendix, and the numbering used in this chapter will follow the numbering that is used there.

5.2 The Structure of the Analytic S-Matrix 1: The Mathematical Framework

As already noted, the basic theoretical object in play in this theory is the scattering matrix, whose individual elements S_{ij} encode the probability of obtaining a state j from a scattering event given that a state i was fed into the reaction. The states related are the states of *free* particles. Any such state is described by giving the 4-momentum and the type of each particle in the state. The type of particle is specified by the relevant set of good quantum numbers, and the quantum number corresponding to any state is given by the sum of the quantum numbers of all the particles in the state. The 4-momentum of the state as a whole is likewise given by the sum of the momenta of the particles involved.

Since the states are partly characterized in terms of their precise values of 4-momentum, it follows from Heisenberg's principle that these states cannot be regarded as being in familiar 4-space.[12] While obviously counter-intuitive,

[12]'Not only conventional Dirac quantum mechanics but even a meaning for micro-

there are very good formal reasons for preferring to work in momentum space – the most salient being that it permits the exploitation of conservation laws. It does, however, mean that we cannot think of the states as undergoing evolution in space and time. This might suggest that the comparatively abstract Heisenberg picture of quantum mechanics is employed here, but in fact the S-matrix formalism is more pared down even than that. This is because the only operator to be found in S-matrix theory is the S-matrix itself.

> S-matrix theory does not employ the full apparatus of quantum mechanics, maintaining only the superposition principle. There is neither a Hamiltonian nor any other operator and there are no state vectors that evolve in time.[13]

With no Hamiltonian, there is no equation of motion and hence no superposed solutions to it. In fact, the incorporation of the superposition principle in this theory amounts to little more than that the S-matrix is a linear operator, as very little use or mention is made of superposed states. Protagonists of S-matrix theory were in fact deeply suspicious about the validity of the concept of the quantum state in relativistic regimes.[14] It was therefore seemingly a quantum theory of strongly interacting *particles* without being one that took seriously the quantum *state*.

Since the S-matrix theory was explicitly predicated on observable quantities, namely the scattering matrix and the principles that governed it, the theory was vulnerable to objections that it was merely '"sophisticated phenomenology" without real content'.[15] While advocates held that this accusation was dispelled once the existence of forces could be argued to emerge from the analysis (on which more below), it is nevertheless clear that many of the traditional metaphysical issues embedded in conventional quantum mechan-

scopic space-time is abandoned in describing interactions between hadrons' (Chew [1968b], p763).

[13]Chew[1966], p5.

[14]This is disclosed by the following footnote: 'We share the point of view of Stapp... that the state vector concept is to be understood as a non-relativistic approximation, not as the basic vehicle for expressing quantum superposition. We take the description of the quantum world to be realized through scattering amplitudes between asymptotic states' (Chew [1971a], footnote 4).

[15]Chew [1966], p99.

ics will simply fail to be issues here.[16] And without operators, there are no state-dependent properties (beyond their momentum properties) to predicate of the particles: all the particles have are their state-independent properties, encoded in the global SU(3) flavour algebra that was developing in parallel at the time.[17] Furthermore, the permutation symmetries of the states themselves play no essential role; there is seemingly no fundamental distinction between bosons and fermions, and particles of either kind play identical roles (most saliently, both bosons *and* fermions mediate forces here). Given that one highly influential approach to quantum individuality presupposes such a fundamental distinction, it is obvious that the issue of particle identity in S-matrix theory cannot be handled in a familiar manner.[18] But since there is no spatio-temporal framework and hence no hope of individuating particles by their trajectories, and given that all particles of a given type have all and only their state-dependent properties common to every token of a given type, it is entirely unclear what the alternative analysis of identity appropriate to this context would be. Rather than throw in the towel regarding the existence of distinct particles at this point, however, in what follows I shall simply take the existence of numerically distinct particles of a given type as given; that is, I will offer no analysis or principle of individuation of different tokens of a given type, and simply take it as primitive.[19] I will therefore assume throughout the existence of numerically distinct hadrons.

With these various differences between this theory and familiar quantum mechanics, and the above disclaimer, in place, I now turn to the positive theses of S-matrix theory.

[16]For example, since it deals explicitly only with free particles, the characteristic quantum feature of entanglement, and the associated non-supervenience, is therefore nowhere to be found here.

[17]This episode will be discussed in Chapter 8.

[18]See Saunders [2003].

[19]In this I resemble Morganti [2007].

5.3 The Structure of the Analytic S-Matrix 2: Axioms

The basic idea behind S-matrix theory is that certain constraints on the scattering matrix suffice to determine it uniquely. As already pointed out, such a method of determination could be taken to constitute a complete quantum theory, and thus these principles may be taken to function as the axioms of the theory. Though more fully spelled out in the Appendix, the axioms of the early phase of S-matrix theory are the following.

1. *Strong interaction forces are short range.*
2. *Superposition.*
3. *Lorentz invariance.*
4. *Unitarity.*
5. *Maximal analyticity of the first kind* – that is, the principle that the amplitudes should be *analytic functions* of the linear momentum variables.

Postulates 1-4 have obviously physical underpinnings. The short-range postulate is empirically evidenced and means that we can treat the states related by the S-matrix as essentially free.[20] The second is a fundamental postulate of quantum mechanics. The third is necessary insofar as the goal is to construct a relativistic theory of quantum mechanics – though note that here we have no *choice* but to construct a relativistic theory, since the binding energies in strong interactions are comparable to the rest-mass energies of the constituent particles.[21] The fourth ensures the conservation of probability.

The fifth, however, may seem rather out of place: given that it postulates that the amplitude is an analytic function, it seems wholly *mathematical* in character, and thus what it is doing as an axiom of physical theory is, as it stands, unclear. And although there is a well-known connection with analyticity and 'causality', it is inapplicable here.[22] Chew himself was rather

[20]Collins and Squires [1968], p7.

[21]Chew [1971b], p141.

[22]There is a well-known *classical* connection between analyticity and 'the principle of causality' – that is, the idea that scattered processes cannot happen before the particles are initially brought together. However, the problem with the derivation

open about the fact that the principle of maximal analyticity of the first kind was simply a postulate, admitting of no conclusive physical motivation. But in his eyes, the burden of proof was rather on those who would question its presence in the theory. As Chew argued, physicists tend to assume that natural laws are *smooth* functions of their arguments, and this is at least suggestive of analyticity:

> I assert that it is natural for an S-matrix element to vary smoothly as energies and angles are changed, and that a natural mathematical definition of physical smoothness lies in the concept of analyticity. The fundamental principle therefore might be one of maximum smoothness...[23]

Furthermore, physicists' practice of expanding functions in power series suggest that analyticity is an unmentioned but essential part of the physicists toolkit.

> ...Physicists tend to forget the exceptional status of analytic functions in mathematics. Fermi used to say: 'When in doubt, expand in a power series.' This statement reflects the belief, shared by most of us, I am sure, that natural laws are likely to depend analytically on any physical parameter which is continuously varied.[24]

Note that, though analytic functions of any type are (by definition) expandable as power series, the assimilation of *smoothness* with analyticity implies that the functions involved here must be complex.[25] So while the assumption of analyticity is justified if it is necessary that all functions in physics are expressible as power series – surely itself a contentious assumption – we can only use smoothness to justify the principle if we are willing to claim that the

of this relationship is that it must both assume that the wave packets that represent quantum particles are localized and exploit precise values for their energy and momentum. It can therefore do little more than establish the need for causality in the classical limit; see for example Collins [1977], p11-12.

[23]Chew [1962], p3.

[24]Chew [1966], p1.

[25]Analytic functions, either real or complex, are defined as those given by a locally convergent power series. It can be shown that if a complex function has a power series then all derivatives of the function exist; hence all complex analytic functions are 'smooth'. The converse is true also, so that the complex smooth functions are identical with the complex analytic functions. There are however smooth real functions that are not analytic; see, e.g., Stewart and Tall [1983], pp177-183.

only functions that physics can truck with are functions of a complex variable. But however the principle is justified *ab initio*, it certainly earns its keep – for it turns out to be this which elevates the S-matrix to what one may claim to be a genuine physical theory. The reasons for this are spelled out more fully in the Appendix, in particular Sections A1.5 to A3, but I will survey them briefly here.

The assumption that the S-matrix is analytic means, at least in this context, that it has only isolated singularities.[26] As shown in the Appendix, this property permits, via Cauchy's theorem, the expression of the amplitude in terms of its singularities. It can furthermore be shown that at a fixed centre of mass energy for the 'direct channel' – that is, at a fixed energy for the collision we are directly performing – the amplitude can be partly expressed in terms of the singularities corresponding to particles produced in the 'crossed' channels – that is, those obtained from the direct channel by interchanging an output particle for an input anti-particle. (What all this means will become clearer below.) That is, with s standing for the square of the direct channel 4-momentum, t as one of the crossed channels and u as the other, and with u fixed at some value u_0, we can derive the *Mandelstam representation* of the amplitude (see Appendix, Section A3):

$$A(s,t,u_0) = \frac{g_s^2}{m_s^2 - s} + \frac{g_t^2}{m_t^2 - s} + \frac{1}{\pi}\int_{s_b}^{\infty} \frac{\mathfrak{Im}A(s',t,u_0)}{s'-s}ds' + \frac{1}{\pi}\int_{t_b}^{\infty} \frac{\mathfrak{Im}A(s,t',u_0)}{t'-t}dt' \tag{A3a}$$

Here, the first two terms on the RHS correspond to pole singularties, and the third and fourth terms to branch-cut singularities. But these terms may be shown to give contributions to the amplitude of the same form as those found in QFT, in the Born approximation, to be due to one-particle and superposed Yukawa potentials respectively (see Appendix, Section A4.1). And since Yukawa interactions are taken to be manifested by *particle exchange* processes, it can with some justification be maintained in this theory that it is the exchange of the particles produced in the cross-channel reactions that

[26]That is, the functions are assumed to be meromorphic, not holomorphic. But since meromorphic functions can be expressed as ratios of holomorphic functions they behave just like their 'properly' analytic counterparts at every non-singular point. See *ibid.*, p208.

supplies the forces to bind the particles produced in the direct channel.[27] It is the seemingly purely mathematical property of analyticity, then, that may be argued to deliver the forces required to form new particles and thus to give us back *dynamics*.[28]

Now, as also explained in the Appendix (Sections A2 to A4), the symmetry between the variables in the Mandelstam representation implies that the role of the intermediate particles produced in the direct channel, and the role of the force-generating particle produced in the crossed channel driving the generation of the direct-channel particle, can be *interchanged* with one another. This situation gives rise to what Chew calls the 'reciprocal bootstrap': the principle that the types of particles that generate a given type of particle are in turn generated by *it*.

> By considering all three channels on this basis we have a self-determining situation. One channel provides forces for the other two – which in turn generate the first.[29]

To make things a little more concrete, let me take a simple example and show what is going on diagrammatically (a method Chew often used in his expositions).[30] I will stick for now to a low-energy approximation in which only one one-particle intermediate state can be produced in each channel, and suppose the direct channel is the reaction

$$\alpha + \beta \rightarrow \gamma + \delta \qquad (\sigma)$$

[27]'The forces producing a certain reaction are due to the intermediate states that occur in the two "crossed" reactions belonging to the same diagram. The range of a given part of the force is determined by the mass of the intermediate state producing it, and the strength of the force by the matrix elements connecting that state to the initial and final states of the crossed reaction' (Chew [1962], p32).

[28]Note that this improvement on Heisenberg's original S-matrix theory is predicated on an explicit formal analogy with a result from the rival QFT programme (in terms of which both the Born approximation and the Yukawa interaction were originally formulated). Given that S-matrix theory was advertised as an alternative to QFT, this is of course somewhat ironic.

[29]Chew [1962], p32.

[30]For a discussion of the use of diagrams in S-matrix theory, see Kaiser [2005], Chapter 9.

which I will call 'Reaction σ'. Suppose too that the single-particle intermediate state produced in Reaction σ is particle Σ. We can represent this as in Figure 5.1.

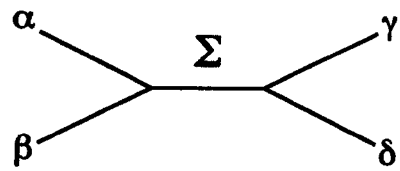


Figure 5.1: Direct channel process (Reaction σ)

A crossed channel reaction corresponding to this process then would then be

$$\alpha + \overline{\gamma} \rightarrow \delta + \overline{\beta} \qquad (\tau)$$

which I will call 'Reaction τ', and which we can represent as in Figure 5.2.

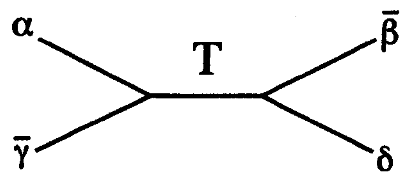


Figure 5.2: Crossed channel process (Reaction τ)

According to the S-matrix theory, and specifically the interpretation of the Mandelstam representation according to which the singularities correspond to Yukawa-type exchanges, the force required to bind Σ from the input particles is provided, in part, by the exchange of the particle T between α and β, and γ and δ. This is represented in Figure 5.3.

Moreover, if we now assume that it is in fact Reaction τ that is the reaction we are performing, so that Reaction σ is now a 'crossed' channel, then we can

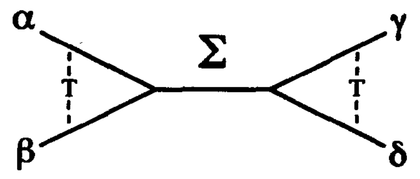


Figure 5.3: Particle exchange in the direct channel

play the same game with the generation of particle T. When we do so, we will find grounds for saying that it is the exchange of the particle Σ that is responsible for T's generation. This is the basic idea that underpins Chew's 'reciprocal bootstrap' concept.

One-particle states produced in the sort of scattering events central to S-matrix theory are called 'resonances', and their existence is inferred from a sharp peak in the cross-section of a scattering reaction. Observing such a peak is taken to be evidence of the existence of a composite particle – that is, a particle that has its origin in an interaction in which the inter-particle forces become strongly attractive. In fact, there are two types of composite particles – namely, bound states and resonances – that are recognized by particle physics. The difference between the two is that resonances have a mass greater than or equal to the total mass of the particles which go into the reaction from which the particle arises, and bound states have a mass that is strictly less. This entails an important *practical* difference between the two, since only resonances can be observed in scattering processes such as the one sketched above.[31] But since the distinction is only one of stability, it is in general not considered to be a fundamental one: stable particles are simply those whose lifetimes are much longer than those of the resonances.[32] In keeping with Chew's usage, then, I shall subsume both types of composite particles under the banner of 'bound states'. To stick to more of Chew's terminology, a particle

[31]Note that the bound state contribution to the observed amplitude can nevertheless be detected: see Chew [1966], p99, for references.

[32]For example, Martin and Spearman state 'The distinction between particles [i.e bound states] and resonances is simply one of stability and should probably not be regarded as a fundamental difference' ([1970] p8); likewise, see Heisenberg [1966], p3.

that is *not* composite, and hence that does not have its origin in an interaction, is an 'elementary particle'. Not being composed of anything else, these we may consider to be fundamental particles, and – as I have already disclosed – S-matrix theory protagonists took the theory to imply that none exist.[33]

Before I can examine why it was that S-matrix theorists believed that this was the case, however, the question that must of course be addressed is what these 'composite particles' are 'composed' of. A natural answer in the case of the intermediate state Σ above is that its constituents are simply the input particles α and β; this, after all, is what Σ was 'created' from (though one must not forget the role of the binding particles). But it might be felt that this obvious choice is compromised by the fact that Σ does not *decay back into* α and β (the reaction is *inelastic*). This brings us to Chew's concept of a composite.

5.4 The Concept of a Composite in S-Matrix Theory

Chew is unambiguous in his statement of what a composite particle is: a composite particle in S-matrix theory is 'a bound state of those channels with which it communicates', where a channel is 'any collection of more than one particle', and '"communicating" channels are nuclear states that possess all the same quantum numbers as a particular particle'.[34]

Particles of any given type, then, are taken to be composed of collections of particles such that they, in the aggregate, have the quantum numbers of the particle. Note that since the definition of a channel concerns only the sum of the quantum numbers of the particles involved, the property of being a certain channel is closed under the addition of particle – anti-particle pairs. It follows that particles of any one type can feature as constituents of a particle of any

[33]More precisely, no fundamental *strongly interacting* particles: it has nothing at all to say regarding other particles. Nonetheless, as mentioned in the last chapter, the existence of even a proper subset of objects in a world that lack any fundamental parts will be enough to entail that that world lacks a fundamental mereological level.

[34]A discussion of the S-matrix concept of composite particles from a metaphysical point of view, and with an eye on the parallels with Leibniz, may be found in Gale [1974].

other type (provided, of course, that the latter is indeed composite).[35] In other words, bound states of any given hadron type T may contain constituents drawn from *any* hadron type whatsoever – including T itself.

Returning to our question, then, of what exactly Σ consists, it is clear that there is no unique answer – for any decomposition with the right quantum numbers will do. Now, one might initially feel dissatisfied by this statement; one might be tempted to think that if there are objectively existing composite particles, then there must be a unique objective fact regarding what a given particle – say our particle Σ – consists of. But this would be mistaken, for quite generally, and as Lewis states, 'a whole divides exhaustively into parts in many different ways'.[36] For example, simple combinatorics tells us that a mundane object of experience, with (something of the order of) Avagadro's number of molecules, will admit an *enormous* number of possible decompositions. As Lewis points out, the best that one might hope for in such cases is that, 'if we distinguish some parts of a fusion as "nice" parts, then a fusion will have a unique decomposition into nice parts'.[37] And as an example of what Lewis means by 'nice' parts, he cites the *mereological atoms* of the object concerned, so that one might hope that an ordinary object at least divides up into 'elementary particles' in some unique way. But the reason we are interested in S-matrix theory, of course, is precisely because it was taken to imply that no strongly interacting particle is mereologically atomic, and hence this cannot be appealed to here. Nor – and to quote more Lewisian terminology – can we distinguish various parts as 'more natural' than others in this picture. The reason for this is that since all hadrons are taken to be composed of hadrons of every other type, all hadrons – from the pion to the uranium nucleus – are regarded as being on the same ontological footing and hence presumably are all equally 'natural'. I therefore think that we must simply rest content with the reality of the enormous variety of decompositions on offer. It is, after all, not clear what exactly is offensive about it; as already pointed out, it is in many ways nothing unusual, and in the absence of fundamental or otherwise privileged parts it seemingly cannot in any case be avoided. It is, however, crucial to note that a composite particle is not simply

[35]This assumes that the energies available for binding are limitless – an assumption reflected in the ranges of the integrals in (A3a).

[36]Lewis [1991], p5.

[37]*Ibid.*, p22.

a 'fusion' of its constituents (to quote a term by which composition is often described in analytic metaphysics). The constituents *must be interacting* with one another by means of particle exchange in order for a bound state to form, for otherwise we have only a state of free particles. The bound state exists for only so long as these interactions take place.

We are now in a position to sketch a definition of 'parthood' appropriate to S-matrix theory and show that it partially orders the set of hadrons. From the definition of a hadron as 'a bound state of those channels with which it communicates', we know that there are two necessary conditions on being a constituent of a token composite particle. We may say that x is a composite (i.e. a bound state) of particles $y_1...y_n$, where $n \geq 2$, just if the following conditions are satisfied:

1. $\sum_i^n QN(y_i) = QN(x)$, where '$QN(x)$' denotes 'the quantum numbers of particle x', etc.; and

2. The $y_1...y_n$ are interacting to give a net strongly attractive force.

I will write 'x is a composite of particles $y_1...y_n$' as '$x = B(y_1...y_n)$' where 'B' denotes 'in a bound state'. Since a composite object is a composite of all of its components, we should impose that the decomposition into the object's constituents is maximal (i.e exhaustive):

3. If $x = B(y_1...y_n)$, then $x \neq B(y_1...y_n, z)$ for any $z \neq y_i$, $y_i \in \{y_1...y_n\}$.

These three conditions are all necessary. But they do not yet seem to be sufficient, for the above conditions do not ensure that they are the parts of particle x of hadron type T and not of some $x' \neq x$ also of type T. However, the ability to specify a further condition that delineated the analysis of composition to the level of distinct tokens of the same type clearly presupposes an analysis of the *distinctness* of two tokens of the same type. Since my primary purpose here is not with the issue of quantum individuation but to make contact with philosophical arguments against the existence of fundamental entities, I will here (as mentioned in Section 2) simply take the distinctness of tokens as primitive and leave the analysis of this distinctness to another occasion (assuming that there can indeed be one that is appropriate to this theory). So to ensure that the compositional analysis applies to these (by assumption) distinct tokens, I will just put in by hand the *uniqueness of composition*:

4. If $x = B(y_1...y_n)$, then for no $x' \neq x$ is $x' = B(z_1...z_n)$ if $\{z_1...z_n\} = \{y_1...y_n\}$.

These four necessary conditions now seem to be jointly sufficient.

From this analysis of a composite, we may define what it is for y to be a constituent, or proper part, of a particle x, or yPx:

yPx if and only if $x = B(y_1...y_n)$ and $y \in \{y_1...y_n\}$.

With this in place, it is now easy to show that the parthood relations appropriate to S-matrix theory form partial orderings. The crucial ingredient is the observation that the existence of the composite implies the existence of its parts, but not *vice versa*. For if $x = B(y_1...y_n)$, then (since they are identical) the existence of x implies the existence of $B(y_1...y_n)$, and by the 'adjective drop' inference, the existence of the $y_1, ..., y_n$ ***in a bound state*** implies the existence of the $y_1...y_n$; hence every $y_i \in \{y_1...y_n\}$.[38] Therefore the existence of a composite particle implies the existence of all of its parts. The converse however is not true: the existence of the $y_1...y_n$ is not sufficient for x, since the particles in the set may be free, violating condition 2, and so no $y_i \in \{y_1, ..., y_n\}$ is sufficient for x either. Let us call this asymmetry between parts and wholes 'the asymmetry of existence'.

Given the asymmetry of existence, it follows immediately that parthood relations are asymmetric. For assume otherwise: that is, assume that we have that $x = B(y_1...y_n)$, and that, for some $y_i \in y_1...y_n$, we also have $y_i = B(x, z)$ for some z. Then we have that (i) y_iPx and that (ii) xPy_i, so that parthood is symmetric. So by (i) and the asymmetry of existence, x implies y_i and y_i does not imply x; and by (ii) y_i implies x. But this is contradictory.

Irreflexivity follows similarly. For assume that $x = B(x, z)$ for some z, so that xPx and parthood is reflexive. Then again via the 'adjective drop' inference we have that x *qua* composite implies x *qua* part; but then given the asymmetry of existence x *qua* part does *not* imply x *qua* composite. But this is once again contradictory.

To establish transitivity, what we must show is that, if yPx and zPy, then zPx. By the definition of 'is a part of', this is equivalent to if $x = B(y, y_1...y_n)$,

[38]For a discussion of the limitations of applicability of the adjective drop inference, see Schaffer [2009], p356.

for some $y_1...y_n$ $(n \geq 1)$, and $y = B(z, z_1...z_m)$ for some $z_1...z_m$ $(m \geq 1)$, then $x = B(z, z_1...z_m, y, y_1...y_n)$. But the definitions of x and y mean that $x = B(B(z, z_1...z_m), y_1...y_n)$, and by utilizing the adjective drop inference once again we have $x = B(z, z_1...z_m, y_1...y_n)$, as required.

What this shows is that the parthood relations appropriate to S-matrix theory partially order the constituents of hadrons. Contact has thus been made with the sort of mereological ordering that Schaffer alludes to, though on wholly internal (and not on *a priori*) grounds. If we are now to argue that these partial orders are non-well-founded, what must be shown is that the theory implies that there are no particles in this theory that do not themselves have parts. That such non-well-foundedness indeed held sway among the hadrons was certainly the opinion of the theory's founder. In his opinion, hadrons are citizens in

> a democracy governed by Yukawa forces. Each strongly interacting particle is conjectured to be a bound state of those channels with which it communicates, owing its existence entirely to forces associated with the exchange of particles that communicate with 'crossed' channels. Each of these latter particles in turn owes its existence to a set of forces to which the original particle makes a contribution. In other words, each particle helps to generate other particles, which in turn generate it.[39]

Here we meet Chew's colourful neologism of 'particle democracy'. A translation might be in order:

> If one wishes to relate this idea of particle democracy to the older language of bound states or composite particles, it amounts to saying that each particle is a composite of all the others.[40]

But of course, nothing in the above *description* of what it is to be a composite particle in S-matrix theory implies that all such particles *are in fact* composite. What I therefore want to do now is turn to the arguments as to why S-matrix principles were taken to imply precisely this, and hence why it is that the S-matrix theory may be taken to provide us with a robust example of an

[39]Chew [1964a], p34 (though this quote is repeated *verbatim* in countless other places).

[40]Martin and Spearman [1970], p8.

internal argument against fundamentality.

5.5 S-Matrix Arguments Against Fundamentality

A variety of arguments against fundamentality can be found circulating in the S-matrix literature, and in what follows I shall look at three of them. In increasing order of sophistication, they are the *argument from superfluousness*, the *argument from holism* and the *argument from analyticity*. It will help to take these in order, so I begin with the first.

5.5.1 The Argument from Superfluousness

The first major reason for the disavowal for elementary particles is that the structure of the theory does not *a priori* require them. While this may sound like a weak motivation, it is nevertheless the case that S-matrix is rather unusual in being a theory of particle dynamics that does not require an *a priori* specification of the properties of certain particles. Consider for example how one would approach hadron dynamics from the Lagrangian perspective – say the dynamics of pion-nucleon scattering. The only relativistic Lagrangian which leads to consistent results in this case is

$$\mathcal{L} = \mathcal{L}_0 + ig\overline{\psi}\gamma_5\psi\tau_i\phi_i + \lambda\phi_i^2\phi_j^2$$

where $\mathcal{L}_0$ gives the free Lagrangian.[41] Here g is a coupling constant associated with the nucleon and λ that associated with the pion. The nucleon field and the pion field each come as a package complete with their spins and masses; the handle on this equation will then be turned to arrive at all the composite structures that the pion-nucleon reaction may give rise to. But since this Lagrangian is the basis of all deductions in the theory, these properties cannot derive from anywhere else in the theory. Hence Lagrangians by design require 'arbitrarily assignable components in a theory', or, in Chew's words,

[41]See Cushing [1990] p132.

'fundamentons'.[42] And elementary particles, in contrast to bound states, would count amongst the fundamentons.

> If a particle appears only when the forces become strongly attractive [i.e. if it is composite], then its mass and couplings are calculable... It is also possible to introduce particles into the S-matrix which are present independently of the strength or sign of the forces. The masses and couplings of these particles cannot be calculated – just as masses and couplings inserted into a Lagrangian are arbitrary – and we choose to call such particles elementary since we cannot explain them.[43]

To adopt a Lagrangian-based approach is therefore to concede there are physical facts of central importance that cannot be explained by the theory, and hence should properly be regarded as 'arbitrary' (from the perspective of that theory at least).

Contrast this picture with that of the S-matrix. While there is no fundamental dynamical equation here in the sense of an equation of motion, there is a *methodologically* central equation, namely the unitarity equation (A1.4a):[44]

$$2\mathfrak{Im}A_{ij} = \sum_n A_{in}A_{nj} \tag{1.4a}$$

However, this is better viewed as a *schema* for equations than an individual equation, since any particle whatsoever – including those that are incontrovertibly non-fundamental – can feature in the states i and j in just the same way. So this equation, although in some ways the most analogous to the Lagrangian above in terms of the function it performs, does not privilege particles in the way the latter does.

This is not to say, however, that nothing counts as a 'fundamenton' in this

[42] Chew [1971a], p2334; Likewise Chew writes of electromagnetism: 'Whether one speaks of the photon or of the electromagnetic field, there exists an *a priori* central component of the theory whose existence is accepted as given – not explained as a necessary consequence of general principles.'

[43] Frautschi [1963], p2.

[44] Veneziano calls unitarity 'the fundamental dynamical condition' (Veneziano [1969], p35).

theory, for the origins of the *axioms themselves* are left largely unexplained. Chew concedes for example that

> the superposition principle is accepted on an *a priori* basis and not explained. In other words, we take for granted the existence of a quantum world.[45]

Thus certain things are treated as fundamental in the context of this theory, but they are *principles*, not *particles*. And of course, if the structure of the theory does not *require* fundamental particles in the way that other theories might, then a simplicity principle can be brought to bear to argue against their inclusion. It was with the above observations in mind that Chew felt

> the aesthetic principle of the 'lack of sufficient reason' may be invoked. There is no 'need' for elementary hadrons.[46]

The thought here seems to be that whatever is superfluous to a theory should not be countenanced by it. However, this seems to be more than just an 'aesthetic' principle – for we may view it as a sound methodological one. In any case, it was a principle that inspired Chew a great deal.[47]

S-matrix theory therefore had good methodological grounds not to countenance the existence of fundamental particles. However, given that what is at stake is so central a supposition of so much modern scientific and philosophical thinking, one would ideally like a rather stronger motivation for dissenting on fundamentality. Methodological considerations in general, after all, can only carry so much ontological weight, and indeed many (myself included) would take the even-handedness between composite and putatively fundamental particles outlined above to recommend at best *agnosticism*, not atheism, when it comes to the existence of fundamental particles. Chew himself in fact conceded that the above sorts of considerations implied that 'elementary hadrons were no longer essential to the dynamics, but they were not excluded', and hence the argument outlined in this section cannot itself

[45]Chew [1971a], p2331. Likewise see Chew [1966], p2 for a statement that 'it is pointless to seek the origin' of why useful functions usually turn out the be analytic.

[46]Chew [1971b], p14.3

[47]As he put it, 'The possibility seemed dazzlingly attractive that, in the hadronic domain, already identified S-matrix principles might render unnecessary the very idea of elementarity. To me at least this possibility was, and is, irresistible' (*ibid.*).

be regarded as a conclusive argument against fundamentality.[48] Rather, it should be seen as a feature that suggests it may be worth *looking* for such an argument. Fortunately for S-matrix theorists, however, two further arguments were ready to be deployed that suggested that the theory was not only compatible with the absence of fundamental entities, but such that it positively prohibits them. The first of these I will call 'the argument from holism'.

5.5.2 The Argument from Holism

It is not only the schematic and non-privileging nature of the unitarity equation that suggests it may be inhospitable to the fundamentalist. It is also the highly *holistic* implications of this equation that could be taken to suggest democracy.

To see why this is, we must recall Chew's association of fundamentality and arbitrariness:

> By definition, a fundamental component is one that is arbitrarily assignable.[49]

What this amounts to in this context is that the *mass* of a particle and its *couplings* to other particles are arbitrarily assignable, since these properties are functions of the binding energy and hence can in principle be deduced from the S-matrix dynamics.[50] Now, for any theory of particle physics that aspires to describe composite structures, it is clear that not *all* of the properties it ascribes to particles could be 'arbitrarily assignable' in this sense. S-matrix theory is no different in this respect. Having already alluded to the 'pole-particle correspondence' relating pole singularities and particles, here is how one of the textbooks puts the matter.

> It would be surprising if *all* the poles could be specified arbitrarily. For instance suppose we include the neutron and proton poles in the S-matrix. We would then expect the deuteron pole to be generated by the 'force' between these two particles, so there

[48]Chew [1968a], p67.

[49]Chew [1970], p23.

[50]As mentioned in the Appendix, Section A1.3, the state-independent properties of the particle encoded in the SU(3) symmetry are *not* explained in this theory.

> should be no need to put it in beforehand. Our expectation about this is clearly based on the feeling that the deuteron is a composite particle, and that composites should be consequences of the theory, not part of the postulates. In quantum-electrodynamics one has to specify the masses and charges of the electron and positron, but not those of positronium, which can be calculated. To add to the theory the requirement that the positronium mass take some particular value other than the experimental one would certainly be inconsistent. A theory of strong interactions which enables one to specify the masses and couplings of all the particles arbitrarily is almost certainly similarly contradictory.[51]

It is here that we meet the connection between fundamentality and *consistency*. Where we have composite particles described in a theory, their properties ought to be deducible in that theory; stipulating some value for them and putting it in by hand is then very likely to result in inconsistency. The suggestion here is that the number of composite particles and the number of arbitrary parameters should be (in some sense) 'inversely proportional' to one another: in the limiting case in which *all* particles are composite, as was Chew's belief, the inference was that *no* parameters should be arbitrary and thus that all should be derivable from the others in a self-consistent or 'bootstrapping' way. As one textbook put it,

> Intuitively, it seems clear that if all the hadrons are to be composites of each other, and all the forces are due to the exchange of particles, then some form of self consistency is necessary[.][52]

But if any theory feasibly provides the requisite degree of self-consistency, then it seems that the S-matrix theory does. The reason for this is that the structure of the S-matrix, and in particular the central role of the unitarity equation, puts *enormous* self-consistency requirements on the theory. I will now try to explain why this is.

As we know, the purpose of S-matrix theory is to compute the scattering amplitude for all strong-interaction processes. The imaginary part of this amplitude (in the physically possible regions of the momentum variables) is

[51] Collins and Squires [1968] p33, italics added.
[52] Collins [1977], p73.

given by the unitarity equation. Looking at this equation (which I repeat here for convenience),

$$2\mathfrak{Im}A_{ij} = \sum_n A_{in}A_{nj} \tag{A1.4a}$$

it is immediately obvious that the amplitude governing any one process is a function of all the amplitudes for all transitions to which the external particles may be connected consistently with the various conservation laws. Thus, as energy increases, any one transition is a function of *all possible processes* with the same quantum numbers that are permitted by energy-momentum conservation.

But in fact things are even worse than this. Consider again the Mandelstam representation of the scattering amplitude (A3a):

$$A(s,t,u_0) = \frac{g_s^2}{m_s^2 - s} + \frac{g_t^2}{m_t^2 - s} + \frac{1}{\pi}\int_{s_b}^{\infty} \frac{\mathfrak{Im}A(s',t,u_0)}{s'-s}ds' + \frac{1}{\pi}\int_{t_b}^{\infty} \frac{\mathfrak{Im}A(s,t',u_0)}{t'-t}dt' \tag{A3a}$$

As explained in the Appendix, Section A4.1, the amplitude for any reaction is a function not just of the singularities in the direct channel (s) but also of those (such as t) that are obtained by 'crossing' from the direct channel (see Appendix, Section A2 for discussion of crossing). To see just how complicated matters can get here, consider first of all the pole singularities. Their positions are given by the masses of the direct- and cross-channel one-particle intermediate states, and their residues are identified with their couplings to the input and output channels (the form of which is given in (A1.5c)). Consider now the branch-cut singularities. These singularities contribute to the amplitude a function of the discontinuity across the cuts, each of which, in physical regions of the Mandelstam plane, is equal to the imaginary part of the direct- and crossed-channel scattering amplitude respectively (see Appendix Section A3). But this in turn is given by the unitarity equation for the direct- and crossed-channel amplitudes, and hence (for the reasons given in the paragraph above) in terms of all those states that can be connected to these channels consistently with the conservation laws. Looking at the δ-functions in (A1.4b) and (A1.4c), and generalizing to a n-particle intermediate state, we see that the singularities – which are related to the δ-functions via (A1.5b) – are fixed by the masses of the particles appearing in these intermediate states.[53] But because we know that the quantum numbers of a given channel are closed under the addition of particle–anti-particle pairs, then as the direct- and cross-channel energies increase to infinity (as the integrals prescribe), eventually a singularity from *every type of particle* will appear.

The net result of all this is that a calculation of the amplitude for any one, general reaction incorporates (1) the scattering amplitudes for all reactions with the same quantum numbers as the reaction in question; (2) the scattering amplitudes for reactions obtained by crossing from the reaction under consideration; (3) the coupling constants of all particles with the quantum numbers of the direct and crossed channels, and (4) the masses of all the strongly interacting particles. This means that even in the case of the simplest reaction, pion-pion elastic scattering,

> The end result is that a full knowledge of the forces governing pion-pion scattering amplitude involves a knowledge not only of the pion-pion scattering amplitude, but also of practically every other strong interaction. Thus, what we actually have is not a self-consistency condition on the the pion-pion amplitude by itself, but a set of very complicated consistency conditions inter-connecting all strong-interaction amplitudes.[54]

In other words, to understand one particle, one essentially has to understand all. In Chew's words,

> A 'bootstrapped' S-matrix contains an infinite number of poles and no single one can be completely understood without an understanding of all the others.[55]

The principal significance of this feature for our purposes is simply that, given that the number of free parameters and the self-consistency of a theory are plausibly 'inversely proportional', the very high self-consistency requirements on S-matrix theory suggests there may be *no* particles permitted by this theory

[53] The precise form of this generalization was given by Cutkosky and can be found in Collins [1977], p15.

[54] Omnès [1971], p300. This reaction is particularly simple because it is 'closed under crossing', i.e. all channels obtained by crossing are identical.

[55] Chew [1968b], p765.

with 'arbitrarily assignable' properties, and hence no fundamental particles. As Chew puts it,

> in this circular and violently non-linear situation it is possible to imagine that no free parameters appear and that the only self-consistent set of particles is the one we find in nature.[56]

However, the high degree of holism has other theoretical and empirical consequences. The first and most obvious of these concerns the intimidating prospect of actually *solving* the S-matrix equations – for it is clear that no exact solution is humanly possible in the general case. While the imaginary part of the amplitude, and hence the solution of the equations, could be directly measured in special cases (such as forward-scattering in elastic processes; see Appendix Section A3 and references therein), in the general case

> [s]olution of the unitarity equations involves solution of infinite sets of coupled, non-linear, singular, integral equations... one would have to solve the entire strong interaction problem in one fell swoop.[57]

The sheer intractability of these equations was enough to put many people off.[58] Clearly, the only way to tackle the problem in the general case was by making some approximations, but pending any idea of what the exact solutions were supposed to look like, it was not in general possible to formulate in a principled way hypotheses regarding which processes might be approximately decoupled from the rest and where truncations could be imposed.[59] One could therefore never be sure that a given piece of evidence really did confirm the theory. Nonetheless, the successes that *were* obtained under these approximation schemes, together with the successes obtained in the special (elastic) cases, meant that there was reason for optimism that the theory was

[56]Chew [1964a], p34

[57]Collins and Squires [1968], p140.

[58]'[A] reason for dislike by some of a dynamically governed democratic structure for nuclear society, with no elementary particles, is that it makes life exceedingly difficult for physicists. We must await the invention of entirely new techniques of analysis before such a situation can be thoroughly comprehended' (Chew [1966], p97).

[59]'The difficulty is that one can never be sure just how bad an approximation one is making, since *a priori* the corrections might turn out to be larger than the effects included' (Collins and Squires [1968], p139).

on the right track.[60]

In any case, with no prospect for either an exact solution of the equations in the general case, or a decisive test of the relations between the various parameters, the *intuition* that there could be no arbitrary parameters in the S-matrix could neither be theoretically demonstrated nor conclusively empirically tested. And of course, there was always the prospect that the high degree of self-consistency required is not met in the first place, and thus that the S-matrix equations as they stand in fact admit of *no* solution.[61] Recalling the idea of Heisenberg that the S-matrix postulates are insufficient to capture dynamics, Chew noted that

> the constraints are so severe that no calculation has come close to satisfying all at the same time. Far from fearing that Lorentz invariance, unitarity and maximal analyticity are insufficient to define a complete dynamical theory, I worry that these requirements may be too much for any S-matrix.[62]

The idea that the high degree of holism inherent in the S-matrix formalism prohibits fundamentality therefore remains, at this point, a hunch and a hope. But it can in fact be demonstrated that, under very general conditions and in the absence of any further postulates, the relation can *not* in general be asserted to hold. The reason is that the Mandelstam expression for the amplitude given in (A3a) – and which I have argued is highly holistic in character – was obtained under the assumption that the amplitude disappears asymptotically (see Appendix Section A3). But this turns out to not be a realistic assumption, for what *is* generally found is that the asymptotic behaviour is only power-bound. As shown in the Appendix, Section A5.2, this means that, where N is this power, terms containing $N-1$ *undetermined subtraction constants* will have to be added to the amplitude to restore convergence. But the presence of these undetermined constants means that the amplitude is not

[60] As one textbook put it, 'With so many approximations, no test can ever be crucial, but a sufficiently large number of partial successes have been achieved to make the more optimistic feel that the hypothesis may be true' (Collins and Squires [1968], p140).

[61] Streater ([2007], p120) states that Claude Lovelace showed that the bootstrap equations in fact have no solution, but no details are given.

[62] Chew [1968a], p67.

in general determined by the unitarity equations.[63] Thus in spite of the *intuition* that the highly holistic character of the S-matrix prohibits undetermined parameters – and hence may exclude elementary particles – the asymptotic behaviour introduces an ambiguity in the representation that suggests this may not be the case after all.

Fortunately for the Chewians, however, this ambiguity in the representation of the amplitude can be effaced with the *extension of the analyticity postulate* – an extension that is desirable on a number of independent grounds. It is here that the Analytic S-matrix theory reaches its apogee, and where it finally manages to translate intuitions concerning the absence of fundamentality into precise empirical predictions.

5.5.3 The Argument from Maximal Analyticity

The absence of any mention of angular momentum in the initial five postulates of the Analytic S-matrix theory was a conspicuous one. After all, the decomposition of total amplitudes into their partial wave counterparts – amplitudes for specific values of angular momenta – was already an essential part of the toolkit in non-relativistic scattering theory; given that S-matrix theory is about scattering through and through, it was therefore 'inevitable that the angular momentum decomposition should receive major attention' in this theory. [64]

Not only does the use of partial wave analysis permit individual waves to be scrutinized, it also permits partial diagonalization of the unitarity formulae. Hence it in principle offers a great deal of simplification of the 'baffling' unitarity equations. But while the motivations for deploying partial wave analysis in this context were clear, and as explained in the Appendix, Section A5.1, it was obvious that the standard wave decomposition of the amplitude would not do. This was because the standard decomposition fails to produce

[63]'Requiring that the amplitude satify maximal analyticity of the first kind, with all the singularities given by the Landau-Cutkosky equations, is not necessarily sufficient to determine it completely. It would be sufficient if it were known to vanish suitably at infinity, but otherwise subtractions, which may introduce arbitrary parameters, are needed' (Collins and Squires [1968], p30).

[64]Chew [1966], p40.

amplitudes that are compatible with crossing symmetry – a central plank in the S-matrix dynamical scheme. Fortunately, it was known that that problem could be resolved by extending angular momenta to complex values, as Tullio Regge had done in the context of non-relativistic quantum mechanics only a few years before.[65] What Regge showed in that context was that the partial wave amplitudes had singularities in the complex angular momentum plane at physical (integral or half-integral) values, and that such poles were 'moving' poles – that is, functions of the energy. The angular momenta of the bound-state solutions of the Schrödinger equation were thereby shown to be connected by smooth functions or 'Regge trajectories', denoted by $\alpha(E)$. Unlike in the non-relativistic case, however, the applicability of 'Regge theory' – that is, the incorporation of complex angular momenta into scattering theory – was essentially a conjecture in this context, as there was no equation of motion whose analyticity properties could be explicitly studied. But the conjecture that the amplitudes were to be continued to complex functions helped to solve in an elegant way a number of theoretical difficulties and, most importantly, was sustained by a rich edifice of phenomenological evidence.[66] Furthermore, it was with its assimilation that the companion notions of arbitrariness, compositeness and analyticity finally assumed a well-articulated form.

The that the amplitudes should admit of complex continuation in angular momentum (ℓ) presupposes that the amplitudes should be maximally analytic functions of ℓ. 'Maximal analyticity' is understood in the case of angular momentum perfectly analogously to the case of linear momentum (discussed in the Appendix, Section A1.5), in that it is taken to mean that the amplitude should admit continuation to complex ℓ-values with only such singularities as are demanded by unitarity. This postulate of *maximal analyticity of the second kind*, that is, in the *angular* momentum variables, was therefore a natural extension of a key postulate already present, and its inclusion completed the architecture of the S-matrix.

The story which asks to be told here is rather long and detailed, but I shall recount just the crucial steps in the reasoning that surrounded the incorporation of the postulate and the subsequent empirical and metaphysical implications;

[65] See Regge [1959], [1960].
[66] Barone and Predazzi [2002], p84.

more (but by no means all of the relevant) detail may be found in the Appendix, Section A5. The postulate of maximal analyticity hypothesizes that the partial wave amplitudes can be analytically continued to complex ℓ for all physical (real and integral or half-integral) ℓ. It can be *demonstrated* that an analytic function for the partial wave amplitudes exists for all ℓ greater than the power of the divergence of the Mandelstam representation (mentioned at the end of the last section and discussed in the Appendix, Section A5.2), where this function is given by the *Froissart-Gribov representation* (see Appendix, Section A5.3). The validity of the extended analyticity postulate, which postulates continuation for all ℓ, was therefore tightly bound up with the power of this divergence. It was then proved (see Appendix, Section A5.4) that unitarity demanded that this power, in the region of the amplitude in which the centre of mass energy in a given channel lay below zero, could be no greater than one. The existence of this *Froissart bound* on the power of the divergence therefore established that the possibility of the invalidity of the postulate was highly constrained, since a unique analytic continuation of the amplitude to complex values of ℓ, for all $\mathfrak{Re}(\ell) > 1$, was demonstrably possible.

The deep significance of this was that the singularities of those partial waves that *did* admit of analytic continuation – the 'Regge poles' – occurred at physical ℓ values and had a Breit-Wigner form (cf. (A5.8a)). But this is the form, familiar from the earliest days of nuclear physics, that corresponds to bound states and scattering resonances – in other words, to composite particles. The particles corresponding to Regge poles – and thus those lying on Regge trajectories – were thereby established as composite. Appearing as a Regge pole in the appropriate partial wave amplitudes was therefore put forward as an *operational definition* of a non-elementary particle in relativistic theory.[67]

An immediate consequence of all this was that *there could be no elementary particle with spin greater than one* (on account of the Froissart bound). Thus analyticity in ℓ placed stringent constraints on the properties that any putative

[67]'The [original] pole-particle correspondence fails to distinguish between 'elementary' and composite' particles, but... Fraustchi and I conjectured that Regge asymptotic behaviour might be used in the relativistic hadron S-matrix to *define* "compositeness"' (Chew [1970], p764, italics added; see also Gribov [2003], p54).

fundamental particle may have. Nevertheless, and this highly significant achievement in constraining fundamentality notwithstanding, it remained that the status of particles with spins of $\ell = 0$, $\ell = 1/2$ and $\ell = 1$ were left hanging by the existence of the bound.[68] Since the constraints on fundamentality would have to be extended into the region of these low values if 'nuclear democracy' was to be sustained, demonstrating that analytic continuation into the lowest partial waves was possible became a pivotal problem for the theory.[69] However, no general method for doing so was established.[70] That the amplitude was an analytic function for *all* ℓ therefore entered, and remained, as a postulate.[71] It was thus this hypothesis regarding the singularity structure in the complex ℓ plane that became the mathematical correlate of the metaphysical hypothesis that no particle was fundamental.

> Let us postulate that the Froissart-Gribov amplitude can be continued to all physical [ℓ] values... and that the actual physical amplitudes are thereby always achieved. This conjecture... we shall designate as maximal analyticity of the second degree. It is equivalent to the concept of nuclear democracy...[72]

One consequence of the postulate that the amplitude was an analytic function of ℓ for *all* ℓ was that the undetermined polynomial in the Mandelstam representation disappeared (see Appendix Section A5.5). Since it was the presence of this polynomial that undermined the intuition that the holism of the S-matrix precluded fundamental particles, the plausibility of the intuition was in this way restored. But this *intuition* was no more than that; barring

[68]In the Appendix, I restrict my attention to the case of integral momentum for simplicity.

[69]'The key problem of bootstrap dynamics is to find a technique of continuing that Froissart-Gribov formula down to values of angular momentum for which poles appear. Not general technique has yet been developed.' (Chew [1966], p60).

[70]Some progress on this issue was made, but the argument was long and complicated, and in any case only applied to elastic processes. See the discussion of Martin's proof in Collins and Squires [1968], p141.

[71]'Assuming analyticity in s, the Froissart limit evidently precludes such a special status for any physical [ℓ] larger than 1, but to date the general principles [so far introduced] have not been shown to ensure that these three lowest [ℓ] values must be ordinary citizens in a nuclear democracy... It may eventually develop that complete democracy is the only way to achieve maximal analyticity of the first degree. Currently, however, it seems necessary to invoke an additional postulate.' (Chew [1966], p54).

[72]*Ibid.*, p55.

a solution of the equations or a precise empirical test of the amplitudes – neither of which were forthcoming in the general case – this disappearance of the polynomial was, though interesting, in itself inconclusive. The much more important implication of the postulate was that *all* particle poles, including those corresponding to the lowest partial waves, were Regge poles. The reason that this is a more important consequence of the postulate is that not only does it give a precise formal meaning to the idea that all particles are composite, it also renders it *empirically testable*.

That this empirical handle on the fundamentality of particles exists may be deduced as follows. As already pointed out, maximal analyticity implies that angular momentum is a continuous function, and it is in fact a function of the energy. As the energy increases, a pole at a given value of ℓ moves along on its trajectory $\alpha(E)$ to its new value ℓ'. As a result of this, poles which contribute to one wave are functionally related to poles in others. This gives rise to the asymptotic behaviour that is shown in (A5.7e). On the other hand, as shown in Appendix Section A5.7, poles that contribute to only one partial wave give rise to δ-function type singularities and the behaviour of (A5.7f). Being non–Regge, these poles can be taken as candidates for those corresponding to elementary particles. Crucially, these two types of poles produce not only distinct but *detectably* distinct asymptotic behaviours. It may therefore be said that

> since bound states clearly lie on Regge trajectories whilst CDD [i.e. non-Regge] poles, in particular partial waves, give rise to Kronecker delta singularities in [the partial wave amplitudes], Regge theory offers a precise way of distinguishing between composite and elementary particles, and therefore of testing the idea of nuclear democracy that there are no elementary particles.[73]

It is therefore the postulate of maximal analyticity, upon which Regge theory rests, that results in the *empirical testability* of nuclear democracy. And it seems that Chew in fact placed his entire anti-fundamentalist capital on the hope that the Regge asymptotic prediction would prevail over that of the rival fundamentalist δ-function.

Thus there exists at least one possible path for experimental de-

[73] Squires [1971], p74.

> molition of the hadronic bootstrap: the discovery of non-Regge poles among hadrons.[74]

Moreover, not only was empirical support for the democracy thesis possible, a great deal of empirical support was initially forthcoming. Perhaps most significant of it all was that fact that the nucleon was shown to lie on a Regge trajectory, which was significant for at least two reasons.[75] First of all, it had spin 1/2, and hence lay within the contentious region (i.e. within the 'Froissart bound'), lending support to the idea that other particles in this region would be 'Reggeizable' too. But secondly, the nucleon was at the time thought to be an elementary particle. The undoing of this assumption through Regge theory made the latter's revolutionary potential immediately manifest, and that there were in fact *no* fundamental particles was suddenly a very real possibility. Jacob and Chew describe the shift in attitudes as follows.

> From the time of their discovery the nucleon and the pion were accorded a status parallel to that of the photon and the electron, respectively. It was taken for granted that the π and the n masses and other properties could not be calculated but must be accepted as fundamental constants of nature.
>
> When the possibility of Regge particles families sharing all quantum numbers except spin was proposed, however, it was immediately noticed that the nucleon (spin 1/2) could be associated Regge-wise with a spin 5/2 particle that clearly was not elementary. This discovery broke the spell, and attempts were then made to compute nucleon properties on a dynamical basis. The results have been sufficiently successful to convince many physicists that the nucleon is a composite state in the same sense as the deuteron.[76]

It was the asymptotic Regge behaviour that rendered maximal analyticity of the second kind, and hence nuclear democracy, falsifiable; and falsified it was. Despite much hope that it was down to experimental error, the (spin-zero) pion consistently refused to exhibit Regge behaviour.[77] Since, as already pointed out, Chew apparently staked his entire anti-fundamentalist capital on

[74] Chew [1968b], p764.
[75] See Cushing [1990], p145, and Chew [1964b], p6.
[76] Jacob and Chew [1964a], p127.
[77] See Chew [1967], p189; also Cushing [1990], p164.

the absence of deviations from Regge behaviour, it seems that we can say that the idea of nuclear democracy was thereby *falsified*.[78]

5.6 Conclusions

Having now examined the grounds upon which S-matrix theorists justified their claim that there are no fundamental hadrons, let me briefly retrace my steps. I noted in Chapter 1 that contemporary metaphysics is pervaded by the assumption that chains of dependence relations must terminate. I then marshalled the S-matrix theory in the service of demonstrating that one could conceivably find oneself in the position of denying this on naturalistic grounds. From there I described a composite, or bound state, in S-matrix theory and showed that the compositional relations applicable there form partial orders. Three increasingly compelling arguments as to why this compositional ordering should be regarded as non-well-founded were then advanced. The *argument from superfluousness* maintained that elementary particles may be eschewed on the grounds that they are not demanded by the formalism. But of course, that there is no *need* for elementary particles does not in itself preclude them. The *argument from holism* then suggested that such particles are indeed positively forbidden. But in lieu of an exact solution to the equations, there could be no demonstration of this, and indeed the need to deal with the divergence of the Mandelstam amplitude under more realistic assumptions positively suggested against it. However, the *argument from analyticity* was able to place stringent limits on this divergence, and in so doing translate

[78]Of course, in reality experiments are ever regarded as so crucial. Cushing's interpretation of the demise of the 'autonomous S-matrix programme' is that it was due to 'degenerating Regge phenomenology' and that it exemplifies a 'degenerating research programme in Lakatos' sense of that word' (Cushing [1990,] p154). Likewise, according to Redhead 'the bootstrap programme was not so much refuted as overtaken by the new fundamentalist approach involving truly basic constituents like quarks and gluons' ([2005], p573). While these are no doubt accurate assessments of the history, given Chew's insistence that non-Regge poles represented the 'demolition' of the theory and the argument underpinning it, I think we can hazard that these experiments may be taken to have a far more destructive significance for the claims of the programme than they perhaps did in practice. (Note that other avenues of refutation were also envisaged as possible, such as the violation of the Levinson theorem regarding the high-energy phase shifts; see Collins and Squires [1968], p145, and Jacob and Chew [1964a], p127.)

the S-matrix's flagship claim into a precise conditional: *if* the postulate of maximal analyticity of the second kind is true, *then* there are no fundamental hadrons. This postulate was motivated on a variety of independent grounds, and admitted of a precise empirical test. The failure of this test tells us that the picture S-matrix theory offers – of hadrons being composed of other hadrons without end – is after all not true of this world.[79]

As an example of an internal argument against fundamentality, the above study demonstrates a number of things.

1. *Fundamentality questions can be empirical questions.* We need not view questions of whether it is necessary or otherwise that 'chains of dependence must terminate' as the exclusive purview of armchair speculation. Now, given that S-matrix theory is so closely connected with phenomenology – its central theoretical component is, after all, the S-matrix's compendium of observable results – I would hazard that such a high degree of direct *empirical* contact with fundamentality-related propositions is not something that we should expect to be a general feature of internal arguments against fundamentality. Nevertheless, S-matrix theory does furnish an example of the attempt to frame what many would take to be a quintessentially metaphysical hypothesis – the infinite divisibility of (a certain kind of) matter and hence the existence of 'gunk' – in thoroughly empirical terms.

2. *Arguments against fundamentality need not be meta-inductions.* The S-matrix argument against fundamentality proceeded entirely from within its own deductive system. It has thus been demonstrated that it can be the internal logic of a physical theory – the implications of its system of physical postulates – that furnishes us with a means to deny the existence of fundamental entities. It follows from that, of course, that arguments against fundamentality need not trade in speculative assumptions regarding the progress of future physics. (It may also be inferred from the fact that the anti-fundamentalist hypothesis became equated with a postulate concerning the extension of angular momen-

[79]Of course, today we do believe that all hadrons are composite (indeed now 'hadrons' are now usually *defined* as those particles that are composed of quarks). But this of course is compatible with the falsity of the S-matrix proposition that all hadrons are composites of other *hadrons*.

tum functions to complex values that there may be nothing *a priori* obvious about the guise in which an anti-fundamentality hypothesis might appear in!) The above discussion also makes salient something that is implicit in the very concept of an internal argument against fundamentality. That is that the anti-fundamentalist must in all cases be committed to *something* that is at least *dialectically* fundamental to their argument – namely, the set of physical *principles* from which their ontologically anti-fundamentalist conclusions follow. As mentioned, for example, Chew was very open about the fact that the superposition postulate was simply taken as an axiom, ultimately being regarded to issue from no more fundamental source. This defining aspect of the internal approach has another important consequence.

3. *Internal arguments against fundamentality are limited.* By definition, internal arguments proceed from a set of postulates, formulated by means of a finite set of predicates. As such, there is only a certain amount of qualitative variation permitted in the descending ontologies they are capable of describing. Those who were hoping that we could have naturalistic grounds for thinking that the world unfolds into stories as different as classical and quantum mechanics again and again *ad infinitum* as we descend more deeply into matter are likely to be disappointed.[80] The picture that S-matrix theory presents us with, for example, is one in which compositional chains go on forever, but also in which the types of particles that feature in these chains recur *ad infinitum*.[81] Although it is not clear to me at this point how best to define the fundamentality of properties, it seems at least intuitively plausible that S-matrix theory implies fundamental *properties* even though it precludes fundamental *particles*. Schaffer refers to worlds such as this – worlds in which the property structure repeats itself indefinitely as we plunge deeper down chains of priority – as 'boring worlds'.[82] Although the degree of homogeneity in the descending sequence need not be quite so

[80] Such a world is conjectured and described by David Bohm ([1957], Chapter 5 (cf. Chapter 4, Section 1 above).

[81] As Veneziano puts it, 'It could be that we have an infinite variety of particles that interact with each other in a small region of space... in such a way as to form bound (or resonating) states that possess again the properties of the constituents' ([1969], p36; quoted in Gale [1974]).

[82] Schaffer [2003], p505.

> dramatic as this in general, the kind of anti-fundamentality that we can hope to establish through the internal approach can only ever be a sort of 'half-way house' in which the theoretical *framework* stays the same, even as the dependence structure never ends.

In spite of the limitations and lacunae outlined above, what the argument just adduced demonstrates is that that the intuition that 'chains of dependence must terminate' is not one that the naturalized metaphysician need share. While there remains a great deal to say on the relation between the frameworks that internal arguments must take as fundamental and the ontological notion of fundamentality – topics that will be discussed in later chapters – I believe that that conclusion represents a philosophical accomplishment in itself.

Chapter 6

Arguing Against Fundamentality II: Effective Quantum Field Theories

6.1 Introduction

The question that concerns me in Part 1 of this thesis is that of whether we might deny the existence of a fundamental basis to the actual world on naturalistic grounds. I have argued that the best approach to adopt in any attempt to do so is the *internal* approach, in which fundamentality questions are addressed through the lens of extant physical theory. So far I have looked at one theory, the Analytic S-matrix theory, that shows that the internal approach can provide us with grounds to deny the existence of a mereologically fundamental basis. That theory, however, is unquestionably as dead as a dodo, and it would be nice to be able to discuss an example of a theory with potential anti-fundamentalist implications that represents a live theoretical possibility. But there is indeed such an example, and this is *quantum field theory* (QFT). For reasons that will become clear later on – if they are not already – QFT is better referred to as a 'framework' for physical theories than a theory *per se*, and it is a particular approach to it – namely, the 'effective interpretation' of the theories formulated within its framework – that has been claimed to have radically anti-fundamentalist implications.[1] Indeed, as already mentioned in Chapter 4, Callender has pointed out in reply

[1]On the notion of a 'framework' for theories, see Shimony *op. cit.*, p209.

to Schaffer that the debate concerning effective field theories is perhaps 'the best support [Schaffer] has in science' for drawing an anti-fundamentalist conclusion, and thus it is almost inevitable that some discussion of this debate would feature in the course of this work.[2]

The debate that Callender refers to – at least insofar as it is played out in the philosophy literature – revolves around the 1993 paper by the historians and philosophers of physics Tian Cao and Silvan Schweber.[3] Cao and Schweber have probably gone further than anyone in advocating the idea that the shift to the effective paradigm in QFT has radically anti-fundamentalist implications, and their paper on the subject has received a good deal of attention.[4] In that paper, they argued that the possibility of using effective quantum field theories in particle physics heralded 'a pluralism in theoretical ontology, an antifoundationalism in epistemology, and an antireductionism in methodology', where the latter is taken to involve a commitment to nomic anti-fundamentalism.[5] Unfortunately, however, their argument for the idea that QFT supports the non-existence of fundamental laws has been subject to a great deal of criticism. Amongst other problems, both technical and philosophical, that have been raised against their argument, it has been claimed that while QFT might be *consistent* with anti-fundamentalism about laws, it certainly does not entail it – contrary to what Cao and Schweber suggest in their paper.[6]

Seminal although it may have been, I will therefore not begin by providing an exegesis of Cao and Schweber's discussion. Rather, I will discuss whether we can use QFT to argue against fundamentality by taking a different route than they. Specifically, rather than focussing, as they do, on the details of the *renormalization procedure*, I will focus more directly on the *renormalizability principle* ('RP') and the changing perception of it (though I will make parenthetical, and mostly critical, references to their work where especially relevant). Nevertheless, this route will – as perhaps all routes must – converge

[2]Callender [2001].

[3]Cao and Schweber [1993].

[4]See for example Hartmann [2001], Castellani [2002] and Huggett and Weingard [1995].

[5]*Op. cit.* p69. Exactly what these 'effective' theories consist of will be introduced below.

[6]See e.g. Huggett and Weingard *op. cit.*, p187; Castellani *op. cit.* p.264.

on the same *underdetermination* regarding fundamentality that was noted in response to Cao and Schweber. However, rather than simply registering that underdetermination yet again, I will put it a little more under the spotlight than it has been hitherto. In particular, by drawing on themes regarding fundamentality as it is conceived of in both physics and metaphysics, I will consider whether anything might motivate vouching for the anti-fundamentalist interpretation, given that the debate revolves around the standing of this principle.

Before I get to that point, however, there is a fair bit of ground to cover in describing the relevant aspects of QFT and motivating the effective interpretation of the theories formulated within it. In what follows, I will begin with the briefest of discussions of the QFT formalism, and indicate the problem of *divergences* inherent within it that ultimately gives rise to the anti-fundamentalist claims made on behalf of it. This will be followed by a summary of the method of dealing with these divergences – namely, the ugly and arduous procedure of *renormalization* – but this material is sufficiently involved to justify requesting the uninitiated to defer to the textbooks for more detail. This will then be followed by a brief discussion of why this process, and the renormalization *principle* that is intimately associated with it, have been regarded with suspicion. From there I will describe the features of so-called 'effective field theories', or 'EFTs', which although quantum field-theoretic fail to satisfy this principle, and furthermore motivate the acceptance of them as legitimate QFTs. With all that in place, I will in Section 8 introduce the underdetermination regarding fundamentality that QFT is taken to involve, and consider some strategies for defending the idea that the best response in the face of it is to say that there are no fundamental laws. (Those familiar with the concept of EFTs and their historical emergence may wish to skim the material until this section.) As something of a coda, I will end by gesturing toward an additional layer of complexity that I will have up until that point neglected (though flagged up) – namely, the phenomenon of *asymptotic safety* – and discuss how this additional but thus far neglected aspect may significantly qualify the conclusions drawn directly before, but nonetheless offer additional insights into the features of internal arguments against fundamentality. (Hopefully why I have adopted this seemingly rather back-to-front strategy should be at least understandable by the end.)

With so much ground to cover, let me therefore turn to the basics of the QFT framework and the problem of the *divergences* to which it inevitably gives rise.

6.2 The QFT Framework and its Problems

Quantum field theory is the physical theory of relativistic quantum systems. It is the theory that solves the problems of negative energy solutions and the failure of probability densities to be positive definite that were inherent in the earlier quantum relativistic particle mechanics. It is regarded as the most fundamental framework for physics that we can currently submit to test, and is taken to have facilitated the most accurate quantitative predictions ever made.

QFT arguably also has a claim to being a highly natural theory of relativistic quantum physics. In the first volume of his magisterial series of textbooks, for example, Weinberg argues that QFT is – *modulo* some caveats and qualifications of various sorts – the *unique* framework that is implied by the principles of quantum mechanics and relativity. More specifically, he argues that 'the whole formalism of fields, particles, and antiparticles seems to be an inevitable consequence of Lorentz invariance, quantum mechanics, and cluster decomposition' – or more succinctly 'that quantum mechanics plus Lorentz invariance plus cluster decomposition implies quantum field theory'.[7] To impose the condition of Lorentz invariance upon the dynamics is of course just to require that the dynamics be relativistic. By 'the principles of quantum mechanics', Weinberg intends the principles that (i) states are to be represented as rays in a Hilbert space (and thus respect the principle of superposition), (ii) physical operators are Hermitian, and (iii) probabilities are given by the Born rule and sum to 1 – hence that the dynamics is *unitary*.[8] 'Cluster decomposition' is taken to be an uncontroversial requirement on any empirical theory, comprising a condition on the factorization of the S-matrix that amounts to the demand – which Weinberg takes to be a basic precondition of any experi-

[7] Weinberg [1997a], pp.6-7. This paper provides a succinct description of Weinberg's QFT programme, described more fully in his textbooks.

[8] See Chapter 2 of Weinberg [1995b].

mental science – that distant experiments yield uncorrelated results.[9] Now, I should note that Weinberg does not *prove* this claim that QFT is the unique framework suited to describing quantum relativistic systems, but rather calls it a 'not-yet-formulated theorem' – though one that he admits already has a counter-example *of sorts* in the shape of string theory.[10] But his presentation of how the structure of QFT dynamics, and its field-theoretic ontology, may be deduced from certain assumed physical and empirical principles should certainly remind us of S-matrix theory, in that in each case both the dynamics and the associated ontology are deduced from the *principles* that are taken to govern the dynamics (and thus the corresponding S-matrix).

Compared with S-matrix theory, however, QFT is of course far more general: none of the principles adduced above demand that the dynamics is short-range, for example, and as such QFT is able to accommodate all of the fundamental interactions that we currently know of, barring (for the moment at least) gravity. It is in virtue of the fact that QFT can accommodate the theories of all these various interactions that it is best thought of not as *a* theory, but rather as a *framework* within which different theories can be formulated.[11] Like S-matrix theory, however, empirical contact with the dynamics is made via the scattering matrix, and thus an essential aspect of QFT practice consists in working out S-matrix elements. Most workaday physicists compute these integrals by means of a *perturbation series* – namely, the Feynman-Dyson series, or 'sum over Feynman diagrams'. Since the perturbative expansion is made about the relevant interaction coupling, this diagrammatic method requires that these couplings be small if we are to be able to use it to calculate the relevant matrix elements. As was mentioned in the last chapter, this is why

[9]See Chapter 4 of Weinberg [1995b]. Note that this is not simply to say that distant *measurements* yield uncorrelated results – something clearly incompatible with quantum mechanics.

[10]Weinberg [1997a], p8. How Weinberg's claim relates to the existence of axiomatic quantum field theory is not something I will discuss here.

[11]Note that this approach to QFT that Weinberg works with – and that I will present – is that which is used by almost all working physicists and taught in almost all graduate physics courses; it is the version that Wallace calls 'naive', 'conventional' or 'Lagrangian' QFT (see Wallace [2006]; [2011]). *It is not* the much more rigorously formulated axiomatic version of QFT developed by Wightman and co-workers in the 1960s. The latter has received precious little in the way of empirical support, and I will have nothing to say about it here; see nonetheless Fraser [2009] for a defence of it.

one must make recourse to essentially different methods when calculating amplitudes in hadron physics, and it is for this reason that hadronic calculations are notoriously difficult in spite of the mathematical elegance of the QCD Lagrangian. The situation with hadrons is far from hopeless, however, for although fully general non-perturbative techniques as present elude us, techniques do exist in some cases and developing them represents an active area of current research. But in any case, perturbative methods of solution serve us very well in a great many instances, and to begin this discussion of QFT and its implications for fundamentality I will discuss how it is that S-matrix elements are typically calculated by means of such methods.

6.2.1 The Problem of Divergences

The substance of my discussion of QFT and its metaphysical implications clearly has to start somewhere, and I will begin with an expression for the scattering amplitude.[12] Let us restrict ourselves to considering neutral scalar fields of mass m. Writing $S = 1 + iA$, where A represents the non-trivial part of the scattering amplitude, and where the momenta k_i pertain to the ingoing and p_j the outgoing particles, we have for $m \rightarrow n$ scattering

$$\prod_{i=1}^{m} \frac{i\sqrt{Z_\phi}}{k_i^2 - m^2} \prod_{j=1}^{n} \frac{i\sqrt{Z_\phi}}{p_j^2 - m^2} \langle \vec{p_1} \ldots \vec{p_n} | iA | \vec{k_1} \ldots \vec{k_m} \rangle \tag{6.1}$$

$$= \prod_{i=1}^{m} \int d^4x_i e^{-k_i . x_i} \prod_{j=1}^{n} \int d^4y_j e^{-p_j \cdot y_j} \langle 0 | T\{\hat{\phi}(x_1) \ldots \hat{\phi}(x_m) \hat{\phi}(y_1) \ldots \hat{\phi}(y_n)\} | 0 \rangle,$$

where Z_ϕ is a *wavefunction renormalization* and T is the *time-ordering operator*. The $\hat{\phi}(x)$ are *local field operators*, which are expressed as a sum of creation and annihilation operators for particles at spacetime point x. This expression constitutes the *LSZ reduction formula*, which relates generic S-matrix elements to the time-ordered product of local field operators acting on the vacuum. The fields that appear in this correspond to the asymptotic states – that is, the fields we feed into and subsequently extract from scattering experiments – where I have distinguished their coordinates by x_i and y_j above for ease of

[12]In producing the following I used a number of texts, primarily Peskin and Schroeder [1995], Ryder [1996], Maggiore [2005], Das [2008] and Weinberg [1995b]. Any of these may be consulted to derive this equation.

interpretation; from now on, I will relax this distinction by representing all spacetime coordinates by x_i. The empirical task that QFT poses is to compute this object, and I will deploy the path integral formalism to this end.[13] In this formalism we can deduce

$$\langle 0|T\{\hat{\phi}(x_1)...\hat{\phi}(x_n)\}|0\rangle = \frac{\int D\phi\phi(x_1)...\phi(x_n)e^{iS}}{\int D\phi e^{iS}}, \tag{6.2}$$

where $S = \frac{1}{2}\int d^4x\mathcal{L}$ is the action, $D\phi$ represents the integration over all *field configurations* that are created from and ultimately end up back in the vacuum, and the $\phi(x)$ are such configurations. The time-ordered product on the LHS of this equation is identified with, and defines, the 'n-point Green's function' $G^n(x_1...x_n)$.

In the case of the free theory, which for a scalar field ϕ corresponds to an action given by $S_{free} = \frac{1}{2}\int d^4x(\partial^\mu\phi\partial_\mu\phi - m^2\phi^2)$, the integrals on the RHS correspond to Gaussian integrals that may be solved exactly. In solving for the n-point Green's function for the free theory (and also, as it turns out, for a completely generic theory) it is expedient to introduce the Feynman propagator, defined as the 2-point Green's function for the free action:

$$D_F(x_1 - x_2) \equiv \frac{\int D\phi\phi(x_1)\phi(x_2)e^{iS_{free}}}{\int D\phi e^{iS_{free}}}. \tag{6.3}$$

Roughly speaking, the Feynman propagator encodes the probability of a free particle created at x_1 to propagate to x_2. The reason this object is so useful in general is due to Wick's theorem, which relates a generic n-point Green's function for the free theory to sums of pairwise products of Feynman propagators.[14] The time-ordered products we are generally interested in calculating, however, describe not free but interacting fields and we therefore

[13]The alternative is to compute the n-point Green's function via the 'canonical quantization' approach, which proceeds purely in terms of Hamiltonian operators. However, I choose to represent the solution to the problem posed by (6.1) in terms of the path integral approach (where (6.2) is the equation that connects the two formalisms) because this, unlike canonical quantization, produces a representation of interacting field theories that is *in principle* independent of perturbation theory (see Maggiore [2005], p219). Since the possibility of a field theory that is conceptually independent of perturbative solutions will be important for the final conclusions I will draw, I want to make clear at the beginning that a perturbation-independent notion of field theory in principle exists.

[14]See Peskin and Schroeder [1995] pp88-90 for the proof by induction.

have to consider the corresponding interaction Lagrangians. To keep things as simple as possible, I will suppose that we are working with $\frac{\lambda\phi^4}{4!}$ theory featuring a spin-0 scalar field interacting with itself, for which the Lagrangian is[15]

$$\mathcal{L} = \frac{1}{2}\partial_\mu\phi\partial^\mu\phi - \frac{m^2}{2}\phi^2 - \frac{\lambda}{4!}\phi^4. \tag{6.4}$$

Here m is assumed to correspond to the mass of the field and λ the (self-interaction) coupling. Slotting this into the formula (6.2) gives us the RHS of the LSZ reduction formula (6.1), thus furnishing the relationship between the ϕ^4 scalar interaction and S-matrix elements that we are looking for. Since this expression relates (at least formally) a set of empirical results with a given set of fields and the interaction between them, we may say that *a physical theory* of a certain set of fields undergoing a certain interaction formulated within the QFT framework is *defined* by a Lagrangian, and thus by a *law of nature*. However, it is easy to see that when we slot this Lagrangian into (6.2), what we get on the RHS is a non-Gaussian integral that cannot be performed exactly on account of the interaction term. Nonetheless, *if* the coupling λ attached to the interaction is sufficiently small, we can tackle the problem of computing the S-matrix elements by means of a perturbation series. In such a case, the powers of ϕ^4 that feature in the exponent can be 'pulled down' out of the exponential and into the enveloping integral, where we know how to treat them (via 'Wick contraction' methods). The problem of interacting fields – so long as the coupling is sufficiently weak – is now essentially reduced to computing a variant of the free theory (so that the Feynman propagator (6.3) is extremely useful in generic scattering processes even though it ostensibly describes $1 \to 1$ scattering only). If, on the other hand, the coupling is not small then in general we do not know what to hit this expression with – though as mentioned, non-perturbative methods prove workable in some cases, and produce good results.

On the assumption that the coupling *is* sufficiently small, we can expand the amplitudes into a series, where each of the terms in this series comes with its own Feynman diagram. The series as a whole is arranged by the number of *internal loops* in these diagrams, so that the higher the order of perturbation

[15]The ϕ^4 is the simplest non-trivial QFT. The Higgs field is such a field, but since in reality the Higgs field is coupled to the electroweak force this Lagrangian does not fully describe it.

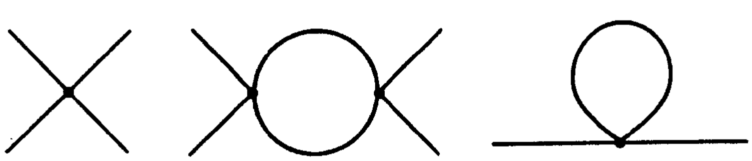
theory and hence the higher the power of the coupling involved, the higher the number of internal loops. If we take the trouble to work out the LHS of the LSZ reduction formula for ϕ^4 theory by means of a perturbative expansion about λ, then if it is $2 \to 2$ scattering that is of interest we must compute the appropriate '4-point functions' (where the '4' refers to the number of external particles involved and hence the number of 'external legs' on the corresponding Feynman diagrams). These can be straightforwardly related via Wick's theorem to products of 2-point functions, and by following the analysis through we find the following contributions to the S-matrix.

1. *Zeroth order in* λ. Here we find no contribution, which is as expected since there is no interaction and we are here considering the non-trivial part of the amplitude iA (cf. equation (6.1)).

2. *First order in* λ. Non-zero terms are obtained only when each of the four $\phi(x_i)$ are contracted with one of the four $\phi(x)$ coming from the interaction term. This produces an amplitude

$$\langle \vec{p_1}\vec{p_2}|iA|\vec{k_1}\vec{k_2}\rangle\rangle = i\lambda(2\pi)^4\delta^4(p_1 + p_2 - k_1 - k_2)$$

and a 'tree' diagram shown in Figure 6.1(a).



(a) Tree (b) Loop (c) Tadpole

Figure 6.1: Feynman diagrams for 4- and 2-point functions to one loop

3. *Second order in* λ. In this case the RHS of the LSZ equation gives

$$\langle \vec{p_1}\vec{p_2}|iA|\vec{k_1}\vec{k_2}\rangle\rangle =$$
$$\frac{(-i\lambda)^2}{2}(2\pi)^4\delta^4(p_1 + p_2 - k_1 - k_2)\int_0^\infty \frac{d^4k}{(2\pi)^4}\frac{i}{k^2 - m^2 + i\epsilon}\frac{i}{(p-k)^2 - m^2 + i\epsilon}, \tag{6.5}$$

where $p = p_1 + p_2$ and k is the undetermined loop momentum, corresponding to the 'loop' diagram in Figure 6.1(b). But it seems that here we encounter a serious problem, for this expression is logarithmically divergent. Since probability functions are, of course, necessarily finite, it is clear that something has gone badly wrong.

Likewise, if we consider the amplitude in the two-point function (corresponding to $1 \longrightarrow 1$ scattering), we again find divergent quantities at the one-loop level (which is order λ in this case). This is associated with the 'tadpole' diagram shown in Figure 6.1(c), which contributes to the amplitude

$$\langle \vec{p_1} | iA | \vec{k_1} \rangle = \frac{(-i\lambda)}{2} \delta^4(p_1 - k_1) \int_0^\infty \frac{d^4k}{(2\pi)^4} \frac{i}{(k^2 - m^2)}. \tag{6.6}$$

But this term is quadratically divergent.

It is in general the case that it is the internal loops that cause the divergence problems, such as these, in arbitrary QFT amplitudes. But since such loops – and hence these divergences – arise in *all* interacting QFTs in four dimensions, not just ϕ^4, it appears that there is a fundamental problem residing deep within the QFT formalism. QFT, after all, postulates that the dynamics is *unitary*, not that its amplitudes shoot up to infinity and become meaningless. Something must therefore be done to repair the situation if QFT is to be regarded as a self-consistent description of nature.

6.2.2 Patching things up

What has just been shown is that the amplitudes of our ϕ^4 theory – at least as they are revealed to us perturbatively – contain infinite divergences, thus violating unitarity and apparently reducing the scattering matrix, and with it the theory's empirical predictions, to physical and mathematical nonsense. To restore our theory to some kind of sense, the solution is to subject it to the complicated process of *renormalization*. This may be thought of as a three-step process. Step 1 is that of *regularizing* the theory to isolate the divergences. This involves the temporary use of a regulator to render the problematic integrals finite in order that the details of their divergences structure can be scrutinized. Step 2 is *revising* the Lagrangian so as to cancel the divergences that would otherwise recur when the regulator is removed to infinity. And step 3 is to *register* any arbitrariness that has been introduced into the theory through the removal of divergences by producing a statement of the invariance of the physics under variations of these changes to the Lagrangian. To give a flavour of what exactly this process involves, I will here simply summarize the essentials of what is described in full detail in the textbooks.

Step 1: Regulate

As noted, the essential purpose of the renormalization procedure is to remove the divergences that are endemic to any realistic quantum field theory. The first step is to move beyond the unhelpful statement that 'the integrals are infinite' and uncover exactly the form that the divergences take. This requires the use of a *regulator,* which temporarily renders the integrals finite. The two techniques most often discussed that can be deployed to this end are *hard cut-off* and *dimensional* regularization. The first of these is by far the most intuitive and simply consists of imposing a finite upper bound on the range of integration. This method, however, suffers the disadvantage of violating Lorentz (and other) symmetries and is almost never used in practice. Much more popular is the more abstract dimensional regularization method, which exploits the fact that the divergence of the integrals such as (6.5) and (6.6) is sensitive to the number of spacetime dimensions over which the integration is performed. By treating this number as a free parameter and analytically continuing away from 4 spacetime dimensions to $4 - \epsilon$, the structure of the divergences in the limit is revealed. Either of these methods allows the integrals to be temporarily well-behaved, so that what must be done to temper them in the infinite-momentum limit can be deduced. Quantities in the regulated theory will in general be dependent on (i.e. functions of) the regulator.

Step 2: Revise the Lagrangian

A study of the divergence structure of the problematic integrals revealed via the regularization procedure shows that there are two ways of removing these divergences. The first of these is Feynman's original procedure, developed for QED, of *reparameterizing the constants*. In this approach, we drop the original assumption that the mass(es) and coupling(s) that appear in the original

Lagrangian represent finite, measureable quantities, and replace it with the idea that they are in fact infinite quantities. These quantities are taken to correspond to properties of 'bare' particles – that is, particles considered in the absence of any interaction (including self-interaction). But since such interactions are always present, these are highly abstracted quantities that – from an empirical point of view at least – we are free to redefine as we see fit. By taking these quantitites to be functions of the regulator, it can then be shown that we can do so in such a way that the divergences of the integrals are absorbed into the redefined parameters, producing expressions for the amplitudes that are finite (to this one-loop order) and independent of the regulator.[16] On the other hand, and more commonly in practice, we may choose to infer from the divergences not that the parameters that feature in the original Lagrangian are unphysical, infinite quantities, but rather that this Lagrangian misrepresents the structure of the interactions that are in play. As such, we can add to the Lagrangian regulator-dependent 'counterterms' whose forms are carefully chosen so that the divergences, and also the regulator dependence, disappear in the limit. In the case of the two-point function for ϕ^4 theory, for example, we have to add a term of the form $c_2\phi^2$, with c_2 constant. Similarly in the case of the four-point function, a term of the form $c_4\phi^4$ must be added.[17] Whatever new constants are added, however, their values cannot be deduced from the theory. We must therefore determine them via experiment.

Register the Arbitrariness

As a result of the procedure outlined above, the amplitudes for ϕ^4 theory at one-loop level – at least insofar as we are interested in $2 \rightarrow 2$ and $1 \rightarrow 1$ scattering only – can be restored to health. However, attending to the details of how this procedure is actually implemented makes clear that certain arbitrary choices must inevitably be made if we are to define the renormalized quantities – choices which amount to a choice of the 'renormalization

[16]In fact, this method only works in the case of so-called 'renormalizable' theories such as ϕ^4: otherwise the method of counterterms must be used. More on this below.

[17]See e.g. Maggiore [2005], section 5.6

scale'.[18] Since the physics cannot vary with the choices that we make – just as it cannot depend on what coordinate system we choose in which to write down the equations – we then demand that the S-matrix, which represents all the observable quantities associated with the theory, is invariant under the transformations between each possible reparameterization. Such transformations form the *renormalization group*. Demanding this invariance under reparameterizations gives rise to the Callen-Symanzik equation, which expresses the invariance of the renormalized vertex functions (and hence of the S-matrix) under variations of the scale relating different renormalization prescriptions.[19] In this equation, the theory's β-function plays a role, which for a singly-coupled theory is defined by

$$t\frac{\partial\lambda(t)}{\partial t} = \beta(\lambda, t). \tag{6.7}$$

This function thus describes how the coupling λ varies as the renormalization scale is ramped up by a factor t. For a theory with more than one coupling, the β-function for the ith coupling will in general be a function of all the couplings in the theory, so that we have

$$t\frac{\partial\lambda_i(t)}{\partial t} = \beta_i(\lambda_i, \lambda_j, ..., t). \tag{6.8}$$

It can be easily shown, moreover, that these equations can be interpreted to describe not just how the couplings change as the renormalization scale is changed, but also as the interaction energy is ramped up. As such, they potentially contain a great deal of information about the high-energy behaviour of the theory. This will be important later on.

[18]For example, when defining the renormalized quantities in the dimensional regularization scheme, certain finite parts may or may not be subtracted in addition to the divergent pole part in ϵ (thus defining the minimal subtraction scheme MS and the alternative $\overline{MS}$). As Collins puts it, 'The infinite parts of the counterterms are determined by the requirement that they cancel the divergences, but the finite part is not so determined. In fact, the partition of a bare coupling into the sum of a finite renormalized coupling g_R and a singular counterterm δg is arbitrary. One can parameterize a theory by transforming a finite amount from g_R to δg without changing the physics' (Collins [1984] p2). The renormalizaton group equations are designed to express the invariance of the predictions of this theory under variations in these prescriptions.

[19]See e.g. Ryder [1996], p325.

For now, however, the process just described is how the ϕ^4 theory of $1 \rightarrow 1$ and $2 \rightarrow 2$ scattering is patched up to one-loop order in perturbation theory, and the same sort of procedure applies to any quantum field theory (to one loop). However, since going up order by order in the perturbation series is equivalent to going up in the number of loops, there are in principle infinitely many loops to consider, and also infinitely many $m \rightarrow n$ reactions that have a slot in the S-matrix. There should therefore be a nagging suspicion at this point that the steps just described represent only the tip of the iceberg of the work that we have to do if we want to restore the ϕ^4 *theory* to sense – not just a few elements of its S-matrix to some low order of perturbation theory. Whether that worry is justified is what I will now discuss, and that will lead straight into the principle that lies at the heart of QFT's fundamentality dialectic.

6.3 The Renormalizability Principle: A New Principle of Theory Assessment

The worry that the work so far done is nowhere near enough to patch up ϕ^4 theory is certainly a legitimate one. The first concern we should have is that, since we have repaired only the one-loop level of these two contributions to the amplitude (i.e. the 2- and 4-point Green's functions), we have only patched up each amplitude to a certain (low) order of perturbation theory, and that this will not itself suffice. After all, each new term in the perturbation series brings with it a new loop, and it is the loops that are at the root of all the problems. It is thus reasonable to suspect that the terms of the series will diverge at every order, and thus that the theory cannot be rendered self-consistent without an infinite amount of work. The second worry is that, even if it turns out that we *do* have to do a finite amount of work in patching up the diagrams of the 2- and 4-point functions, it may be that each n-point function for $n > 4$ will need to be patched up separately. If that were the case, then for each amplitude we will need to add a new counterterm with a new parameter to be matched to experiment. But if *that* were the case it would apparently be disastrous, since each of these terms brings in its wake a new (and undetermined) constant. Since we cannot deduce quantitative predictions from a theory until its parameters are set, it seems that we would again have to do an infinite amount of work before we were able to use the theory to make any testable predictions. As such, even if Nature were to turn out to be described by such a theory, it seems that we would never be in a position to believe it.

Ascertaining which diagrams in an expansion of the amplitude need to be separately renormalized is therefore of paramount importance in assessing any QFT. But in spite of the complex and confusing nature of the renormalization procedure, the recipe for discovering which diagrams can be cured by curing lower-order diagrams turns out to be mercifully simple. It can be deduced very easily that, if the dimension of the spacetime is 4, then if the *mass dimension* of the coupling associated with an interaction is non-negative then there is an upper bound on the number of processes that can produce independent divergences.[20] To see this, note that the *superficial degree of divergence* in four dimensions is defined as

$$D \equiv (N-4)V + 4 - n, \tag{6.9}$$

where N is the power of the coupling in $\lambda\phi^N$, V is the number of vertices in a diagram and n is the number of external legs (i.e. the number of asymptotic states involved in the process).[21] The point of this expression is, as the name suggests, to codify the degree of divergence associated with a given Feynman diagram (and hence its contribution to the amplitude): whenever this expression is positive the integral will be divergent, and the more positive it is, the greater the divergence. It can be immediately inferred from this formula that the degree of divergence for any n-point function will increase with the number of vertices (and hence loops) if $N > 4$, and that a divergence will afflict every n-point function eventually. One can also see that the greater the number of external particles involved, and thus the higher the n, the

[20]See Weinberg [1995b], Section 12.1 for a description of how to compute the mass dimension of any coupling from its free Lagrangian. But basically the idea is just that if a coupling has dimensions of $1/M^N$ for some N, then it is said to have negative mass dimension.

[21]See Maggiore [2005] p140 for an intuitive explanation of why this formula holds. As the name 'superficial' suggests, the superficial degree of divergence is not necessarily all there is to know about the contribution of an arbitrary Feynman diagram – notably in gauge theories. I will ignore these complications here, but see Das [2008], p721 for more details.

higher the order in perturbation theory it will be that a new divergence emerges. Since the action – which contains a d^4x term – is required to be dimensionless, this is equivalent to saying that the divergence of any n−point function will increase as we go up through perturbation theory (and hence the number of vertices involved) if the mass dimension of the coupling is strictly less than zero. We thus say that interactions with negative mass dimension are *nonrenormalizable,* because new corrections to the parameters have to be made every time we go up an order in the perturbative expansion. The task of removing the associated theory's divergences therefore seems to require an infinite amount of work.

On the other hand, if $N < 4$ and thus if the coupling has a *positive* mass dimension, it may be seen that after some n there will be no new divergences in the n-point functions, and for $m < n$, only finitely many terms in the perturbation series will need to be attended to. Finally, if $N = 4$ and hence the coupling has *zero* mass dimension, only the 2-, 3-, and 4-point functions will need to be patched up, and the degree of divergence will moreover be independent of the number of vertices and hence the order of perturbation theory.[22] As such, we call interactions that conform to either of these latter two conditions *renormalizable.* Otherwise, they are said to be *nonrenormalizable.* These observations can in fact be generalized outside of scalar field theory, and we can say quite generally that

> *terms in the Lagrangian whose couplings have either a positive mass dimension or are dimensionless are renormalizable. Terms with negative mass dimension are nonrenormalizable.*[23]

Likewise, *theories* in which all interactions are renormalizable are also said to be renormalizable. In such theories, there are only a finite number of divergences that need to be patched up and hence a finite number of counterterms added, or parameters redefined, to restore them to some kind of sense.

With all that in place, it is now easy to see that we have done all the work we need to do to patch up our ϕ^4 theory. Here, $N = 4$, and we have already dealt with the 2- and 4-point functions; the 3-point function turns out to be forbid-

[22] Couplings with positive mass dimension are often called 'super-renormalizable', but such couplings turn out to be pathological in QFT (see Le Bellac [1992], p204).

[23] Maggiore [2005], p140.

den on spacetime symmetry grounds. This feature that only a finite number of divergences need to be repaired in order to repair the full perturbative expansion is the definitive feature of renormalizable theories.

Nonrenormalizable theories, on the other hand, naturally lack this feature: in these cases qualitatively new divergences will show up as we consider diagrams with more and more external legs, at some order of perturbation theory at least. But it is crucial to be clear that the divergences produced in nonrenormalizable theories can *also* be cancelled, in principle, by the addition of counterterms, just as the divergences that appear in renormalizable theories can. The difference consists in the fact that *infinitely many* counterterms need to be added in the case of nonrenormalizable theories.[24] It can indeed be shown that for the renormalization process to work in the case of such theories, then barring special cancellations it is essential that *every possible interaction term consistent with the symmetries of the theory is included.* It is also easy to deduce that, for any given set of fields and any given symmetry, there are infinitely many terms containing those fields that respect that symmetry.[25]

Given, then, that the divergences in nonrenormalizable theories can in principle be removed, but only by the addition of infinitely many new terms, each of which must be matched to experiment, the perceived problem with such theories is *not so much their divergence* as *their lack of predictive power.*[26] After all, the full renormalized Lagrangians for these theories, if they are to be regarded as internally consistent, must contain an infinite number of terms each of which contains a constant to be matched to experiment – something that seems to make it impossible in practice to ever extract any predictions from such theories. And since it is of course primarily predictive power that separates physics from purely metaphysical speculation about the structure of reality, predictivity is a non-negotiable property of acceptable physical theories. Thus it seems that nonrenormalizable theories are wholly unacceptable as theories of physics – and thus also unacceptable as candidates of theories that could be used to deny fundamentality on *naturalistic* grounds. As Schweber puts the matter,

[24]Weinberg [1995b], p506; see also Lepage [1989].
[25]More on all this may be found in Weinberg [1995b], Chapter 12.
[26]Weinberg [1995b], p500.

> since the aim of foundational physics is to formulate theories with considerable predictive power, 'fundamental laws' must contain only a finite number of parameters. Only renormalizable theories are consistent with this requirement.[27]

It was for these reasons that it was *laid down as a condition* on quantum field theories that they could feature only renormalizable terms – that is, terms whose couplings have non-negative mass dimension. This constraint on theories was denoted the *renormalizability principle* ('RP'). Since it turns out (and as is easily shown) that, for any given set of fields and any given symmetry, there are very few interaction types that are renormalizable – indeed sometimes none at all – but always infinitely many that are not, to require that a theory is renormalizable is to place an extremely stringent constraint on it.[28] A famous restriction that the RP placed on QED was its forbidding of the presence of the Pauli term that would otherwise have been permitted if only Lorentz and U(1) gauge invariance were required. The presence of this term would have made the magnetic moment of the electron an adjustable parameter, but its exclusion meant that this property was precisely determined – which in turn facilitated one of the most impressive predictions ever produced by science.[29] While it was already taken as read that conformance with the RP was *necessary* for theories to even qualify as empirical, this success was viewed as a compelling *empirical validation* of the fact of that conformance.[30]

Through these considerations, the RP was elevated to fundamental status in particle physics. Indeed, it was sometimes even presented as being on a par with symmetry principles. For example, speaking in 1979 and thus arguably at the zenith of the Standard Model, Weinberg declared that

> To a remarkable degree, our present detailed theories of elementary particle interactions can be understood deductively, as consequences of symmetry principles and of the principle of renormalizability which is invoked to deal with the infinities.[31]

[27]Schweber [1993], p147.
[28]Weinberg [1995b], p517.
[29]See *ibid.* and references therein.
[30]*Ibid.*
[31]Weinberg [1980a], p515.

Insofar as it was regarded to be of similar standing to symmetry principles with respect to its deductive role, the RP had a claim to being not just a *sine qua non* of any empirical theory of quantum relativistic regimes, but as a fundamental principle of nature.[32] In spite of the RP's seemingly non-negotiable character, however, not all physicists were comfortable with presenting things in this way. Perhaps the principal reason why physicists' were so suspicious of the renormalizability *principle* was because the renormalization *procedure* was also regarded with a great deal of suspicion. The latter was in fact predominantly viewed as an *ad hoc* act of mathematical hocus-pocus and something of an embarrassment until at least the 1980s – even by the pioneers of renormalization theory themselves.[33] But since the RP only has meaning against the backdrop of this procedure, many physicists did not share the confidence that Weinberg apparently voiced regarding its foundational status. Somewhat ironically, however, it was in fact the eventual tempering of the worries about the renormalization procedure that contributed to the eventual displacement of the RP as a mandatory constraint on theories. This in turn opened the door to the possibility that QFT might contain radically anti-fundamentalist implications, as I will now begin to try to explain.

6.4 Disaffection with the Renormalizability Principle

Arguably the principal reason that the renormalization procedure was regarded with such scepticism is that the divergences in QFT seem to indicate something fundamentally wrong in the foundations of the theory. While it seems as though the renormalization process restores the amplitudes to some kind of sense, it is hard to not get the feeling that to simply doctor the infinite quantities by hand, as opposed to prevent them arising in the

[32]As Zinn-Justin puts it, 'Demanding that Fundamental Interactions should be described by renormalizable Quantum Field Theories had been the guiding principle for the construction of the Standard Model. From the success of the program it could have been inferred that the principle of renormalizability was a new law of nature.' ([1998], p9).

[33]See for example Dirac [1969], [1987] and [1978], p36; Feynman [1965]; also references to Tomonaga and Schwinger in Cao [1998], Chapter 7.

first place, is to merely put a plaster on the problem. This is what Feynman had in mind when he spoke of the problems in QFT being 'swept under the rug' by his procedure.[34] A second and related source of distrust with the process was that, even while the above process of subtracting out infinities seems at least to work, it remained deeply *unintuitive* that it should in fact do so. During the renormalization process, we tune the couplings and / or add counterterms in such a way as that the divergences disappear in the limit. In the case of renormalizable theories, the contribution of the high-energy processes can be modelled either in terms of a modification of the theory's original parameters or the addition of new terms with the same form as the original terms.[35] But since the high-energy processes presumably make the most important contributions to the amplitude, it is very counter-intuitive that their contribution could be modelled in such a simple way. As Peskin and Schroeder put it,

> the cancellation of ultraviolet divergences is essential if a theory is to yield quantitative physical predictions. But, at a deep level, the fact that high-momentum virtual quanta can have so little effect on a theory is quite surprising... It is not easy to understand how the quantum flunctuations associated with extremely short distances can be so innocuous as to affect a theory only through the values of a few of its parameters.[36]

It therefore appears that, in section of the physics community, one could find both embarrassment about the superficial-looking nature of the renormalization procedure and bewilderment as to why it should even work at all.[37] But since the significance of the RP was framed entirely in terms of this process, many physicists rightly felt uncomfortable about insisting on renormalizability as a fundamental constraint on laws. While it was regarded as (in some sense) *a priori* necessary that any empirically acceptable physical theory had to con-

[34]Feynman *op. cit.*

[35]In the case of nonrenormalizable theories, infinitely many terms must be added, the vast majority of which will not have the same form as the originals.

[36]Peskin and Schroeder [1995], p19.

[37]There were other reasons to be puzzled about the significance of the renormalizability principle in the wake of 'beyond the Standard Model' speculations, which were encouraged by the SU(5) theory of Georgi, Glashow and Weinberg, and also the fact that a putative quantum theory of gravity would never be renormalizable. But I will not discuss these here.

form to the RP, not much could be offered by way of *physical* grounds for why any such theory *should in fact* be expected to conform. One high-energy physicist, John Donoghue, registers his misgivings about imposing satisfaction of the RP in the pedagogical context as follows.

> When we teach a course in quantum field theory, we typically give the following rules for building and applying a theory:
>
> (1) Construct an action which is invariant under the desired symmetries. In the archetypical case of QED, the Lagrange density must be a Lorentz scalar, invariant under U(1) gauge invariance.
>
> (2) Keep only renormalizable interactions. This restricts the Lagrangian to terms of canonical dimension less than or equal to four.[38] In the QED example, one drops Lorentz and gauge invariant terms such as $F_{\mu\nu}F^{\mu\nu 2}$, $\overline{\psi}\sigma^{\mu\nu}\psi F_{\mu\nu}$.
>
> (3) Quantize the theory and calculate scattering processes. When using perturbation theory, this consists of the calculation of tree and loop diagrams.
>
> (4) Determine the physical parameters from experiment and express the predictions in terms of the physical parameters. One must measure the charge and masses of the theory, and the predictions of QED amount to relations between many experiments, all parameterized in terms of the physical scale (e, m).
>
> Of these ingredients, #1, 3, 4 seem to be logically necessary, and one cannot imagine modifying these steps. However the issue of renormalizability is less obvious, and almost seems to be inserted more for the convenience of the physicist doing the calculation.[39]

This expression of the feeling that the RP lacks the physical intuitiveness of the other principles governing the construction of laws in QFT, and instead looks rather 'inserted' without a clear physical mandate, may be found echoed

[38]That is to say, in four space dimensions, to terms whose couplings have non-negative mass dimension.

[39]Donoghue [1991], p3.

elsewhere.[40] In lieu of any compelling physical story about what was going on in the renormalization process, this perceived lack of foundation could be viewed simply as consequence of the poor understanding of that which motivated the principle in the first place. But as things were to turn out, even when some insight *was* given into the physical nature of that process, it failed to simultaneously provide a rationale for insisting on the RP as a mandatory constraint on theories. This played a major part in instigating a fundamental re-assessment of the role of the renormalizability principle.

6.5 Dispensing with the Renormalizability Principle

The work of the condensed matter physicist Kenneth Wilson is widely (though not universally) taken to at last have given a coherent physical explanation of what is going on in the process of renormalization, beyond a mysterious 'cancellation of infinities'.[41] What Wilson realized was that if we are to understand what is going on in in the process of renormalization, we have to explain why the high-energy contributions of the theory can be modelled as they are in that process. In order to explain *that*, we need to understand the effects that the short-distance degrees of freedom have on the interaction amplitudes (and other observable quantities). Wilson's idea was to study the contribution of these high-energy flunctuations appearing in the relevant integrals by directly *integrating out* the short-distance degrees of freedom, so

[40] See e.g. Peskin and Schroeder [1995], p402, 406, and 81; also Lepage [1989], p1, and Zinn-Justin [1998], p9.

[41] An approachable introduction to Wilson's work may be found in Wilson [1983] and [1979]. Huggett and Weingard [1995] and Huggett [2002] both argue that Wilson's work lays to rest fears about the legitimacy of renormalization; Fraser [2009], p551, on the other hand, expresses doubt that this is in fact the case. Indeed, Fraser [unpublished] is sceptical that Wilson's work can legimately be transplated from its original context into high-energy QFTs in order to explain renormalization in the latter at all, since Wilson's analysis of high-energy contributions is always formulated relative to a finite cut-off which destroys Lorentz invariance (on which more *anon*). All that I rely on for present purposes, however, is that Wilson's work has *some* non-trivial contribution to make to the explication of the renormalization procedure, and that that contribution invited a positive re-assessment of nonrenormalizable theories.

that the influence of high-energy field modes on the predictions of the theory could be studied by comparing the results with the original integrals.[42] In this picture, 'high' is defined relative to an arbitrarily high but nonetheless finite hard momentum cut-off; this cut-off is then lowered, and what we are comparing in this analysis is therefore the effect of integrating out the field states between two finite cut-offs. What he found was that this removal of high-energy states could be compensated for, at (relatively) low energies, by a modification of the parameters in the original action and the addition of new terms in the low-energy fields – and all in such a way as to remove the dependence on the cut-off in all observable quantities. Thus he showed, amongst other things, that high-energy effects could to a good approximation be 'mocked-up' at low energies by the modification of parameters, and the addition of new local interactions amongst the low-energy fields.[43]

These effects that Wilson demonstrated to follow from integrating out 'shells' of high-momentum field space is widely taken to give physical insight into why the contribution of (relatively) high-energy states can be compensated for in the renormalization procedure by a change in the parameters of the pre-existing interactions and / or the addition of new interaction terms, since it is precisely these effects that were shown to accompany changes in the available energy space.[44] In this way, a partial explanation of the change in QFT Lagrangians as the regulator is removed was regarded, by swathes of the physics community, as having been provided at last.[45] However, the resultant 'Wilson action' featuring only the low energy modes contains, strictly speaking, an infinite string of new local interactions featuring the low-energy fields *in which every term consistent with the symmetries of the theory eventually appears.*[46] As discussed in Section 3, this implies that every *nonrenormalizable* as well as renormalizable term is ultimately included in the action generated. Now, in the renormalization story that I told above, it was only in the case of so-called nonrenormalizable theories that all these infinitely-many terms must be included as counterterms.[47] It follows that

[42]Cf. Peskin and Schroeder [1995], p394.

[43]For proof of the locality of these terms, see Collins [1984], p125.

[44]Burgess [2007], p22-3.

[45]See for example Wallace [2006]; Rivasseau [2002]; Peskin and Schroeder [1995], Chapter 12; and Huggett and Weingard [1996] (see also references on page S163).

[46]See, e.g. Peskin and Schroeder [1995] p399, Weinberg [1995b], Section 12.4.

[47]In fact, when stated in the language of counterterms, a theory is said to be

if we take the introduction of terms through Wilson's procedure to explain what is going on in the renormalization process, and thus to explain the appearance of counterterms in modelling high-energy effects, then it seems that we cannot regard the original Lagrangian we are in the process of renormalizing to contain only renormalizable terms. But the paradox implicit in *that*, of course, is that we used facts about the renormalization process to disqualify Lagrangians containing nonrenormalizable interactions from serving as acceptable physical theories in the first place. How, then, can we regard Wilson's insights as furnishing a physically acceptable story of what is going on in renormalization?[48]

The way out of this seeming paradox begins with the realization that the empirical argument that I – and indeed the physics community in the 1960s and 70s – put forward in favour of the RP in fact yields too strong a conclusion, for it is *not the case* that nonrenormalizable theories necessarily lack predictive power. The reason for this turns out to be disarmingly simple, since one can

renormalizable iff the counterterms are of the *same form* as those appearing in the original Lagrangian; see Das [2008] p.682. (There are some exceptions to this rule, however: see Le Bellac [1992], p213. In such cases, we say that the theory is regarded as renormalizable if the number of counterterms is finite. But I ignore such complications for now.)

[48]One may find statements in the literature to the effect that Wilson's insights make it 'irresistable' to regard *all* QFTs as inherently nonrenormalizable (see e.g. Burgess [2007], p22). But it seems that any such argument has yet to be rigorously made out. For one thing, the above results concern field contributions evaluated between two finite cut-offs. But as we will see, finite cut-offs in energy space are associated with merely *effective*, hence nonrenormalizable, field theories. Therefore to unqualifiedly claim that the above analysis 'explains' renormalization seems to *presuppose* that we are dealing with nonrenormalizable theories, and thus cannot be used to argue that QFTs are universally nonrenormalizable. (After all, to give a full explanation of the renormalization procedure in the case of fundamental – hence as we will see renormalizable – theories, we must look at the contributions in the infinite energy limit.) Furthermore, as we will see, there are physicists who are motivated by Wilson's work but also regard the existence of a fundamental renormalizable theory as a coherent possibility, which should be enough to convey that this analysis does not *force* upon us the idea that all QFTs are nonrenormalizable (regardless of how nice that may turn out to be for the naturalistic anti-fundamentalist). I in any case do not need Wilson's analysis to foist upon us any such belief; all I will require for my purposes is that (i) his analysis gives at least *some* physical insight into the renormalization procedure (since the contributions between two finite cut-offs are clearly at least *relevant* to the issue), but that (ii) those insights do not after all furnish us with a physical rationale for insisting on the RP, as might initially have been hoped.

in fact view it as a consequence of some straightforward dimensional analysis applied to nonrenormalizable theories. Recall that the RP was introduced to rule out theories that produce independent divergences at every order. Recall too that the divergences in nonrenormalizable theories are just as remediable as those in renormalizable ones if – but only if – infinitely many new counterterms are added to mop them up. Since each such term is accompanied by a coupling that needs to be fitted to experiment, the apparent problem with such theories is therefore their predictive power, and not their divergences *per se*. Now, to make things concrete, suppose that an interaction coupling has negative mass dimension – so that it has the dimensions $\frac{1}{M^2}$ say, as the ϕ^6 theory's coupling would – and thus is classified as nonrenormalizable. We can then write a perturbative expansion for the m-point function to order $(\frac{1}{M^2})^n$ as

$$A_m(E) = A_m^0(E)\Big(1 + f_1\frac{E^2}{M^2} + f_2\frac{E^4}{M^4} + \ldots + f_n\frac{E^{2n}}{M^{2n}} + \ldots\Big), \tag{6.10}$$

where the f_i are dimensionless contributions that encode the Feynman diagram structure and E is the interaction energy.[49] As mentioned in Section 3, the hallmark of a nonrenormalizable theory is that the amplitudes for any m – that is, for reactions involving m external particles – are not adequately dealt with by patching up the amplitudes for $m' < m$, since a new divergence at some order of perturbation theory is guaranteed to appear in the m-point function. Because of the new divergence at that order, call it n', a new interaction term with a new undetermined coupling c_n' must be added to the Lagrangian to mop up the divergence in $f_{n'}$. This, of course, is the source for the loss of predictivity associated with nonrenormalizable theories that the RP was introduced to avoid.

However, it is clear by inspection that if we are in a low-energy regime in which $E << M$, then this loss of predictivity on the c_n' is in fact completely irrelevant, because it will always appear in observables multiplied by the very small quantity $(E/M)^{2n}$.[50] In this sense, nonrenormalizable theories at sufficiently low energies can be used exactly as renormalizable ones, since (i) only finitely many of the problematic terms containing the undetermined

[49]See Maggiore [2005], pp.144-45.

[50]I am glossing over some subtleties in the choice of regularization scheme here; see Kaplan [1995], pp30-31.

parameters in any $m-$point function need to be considered (since, as just discussed, they become ever more vanishingly small as we increase the order of perturbation theory), and (ii) as we increase m, the new divergences appear ever higher up in the perturbation series, so that the divergences there need not be taken into account either (for essentially the same reason). Therefore in such low-energy regimes, if we want to make predictions within a given accuracy – as we inevitably do – then just as in the case of renormalizable theories, *we need only measure a finite number of parameters in order to generate real predictions*. Thus nonrenormalizable theories, *if* confined to low energies, can be as predictive as renormalizable theories – and this is so in spite of the fact that self-consistency requires that they contain infinitely many constants.[51]

The work of Wilson therefore invited a re-appraisal of nonrenormalizable theories, and simple dimensional considerations revealed that such theories could after all be regarded as empirical despite their failure to satify the RP. However, that principle can only be dispensed with as a constraint on theories if the apparent empirical *fruitfulness* – a fruitfulness seemingly testified to in the case of the Pauli term – of demanding its satisfaction can be accounted for. But it is immediately obvious that the same simple dimensional analysis of (6.10) again contains the seeds of an anwer. To quote Weinberg once again,

> if renormalizability is not a fundamental principle, then how do we explain the success of renormalizable theories like quantum electrodynamics and the standard model?... The success of theories of the electroweak and strong interactions shows only that M [the mass scale associated with an interaction] is very much larger than the energy scale at which these theories have been tested.[52]

In other words, the significance of renormalizability and its origins is simply that low-energy approximations to arbitrarily high-energy dynamics *can* be

[51]Given that nonrenormalizable theories had been on the table since Fermi's work on the weak interaction in the 1930s, and it was not until the late 1970s that this was pointed out, it is fair to say that this realization came conspicuously late (see Weinberg [1997b], p42). In retrospect, it seems almost baffling that such a simple point could have been missed for so long (Rothstein [2004], p6). Nonetheless, for present purposes this need not be regarded as anything more than a curiosity.

[52]Weinberg [1995b], p519.

formulated in terms of renormalizable interactions, since the nonrenormalizable interactions would be suppressed by powers of $1/M$, and thus in such a way that we can expect the theory to *look* renormalizable.

In light of this very simple observation, the supposed triumphs of the renormalizability principle may be seen to be quite overstated. In may be shown, for example, that the famed prediction of the magnetic moment of the electron in QED to 12 places of decimal entails only that the energy scale at which new interactions might come into play and render this parameter adjustable must be larger than 10^{12}eV.[53] Why the RP might nonetheless have been a fruitful heuristic can also be explained from this new point of view, however, since imposing it encourages us to take into account only those interactions that are not highly supressed at accessible energies.[54]

For all these reasons, satisfaction of the RP was no longer seen as *sine qua non* of workable field theories. As a result, Weinberg's earlier statement to the effect that the principle was of fundamental significance, needed alongside symmetry principles in order to explain the Standard Model's success, seemed to constitute an overstatement. Whether there are nonetheless grounds for considering the RP to be a fundamental principle of nature is something that I will discuss shortly. For now, however, a little more detail on the properties of nonrenormalizable theories, and their relationship to fundamentality, is required.

6.6 The Concept of a Merely 'Effective' Field Theory

As I have just argued, it was simple dimensional considerations that played a pivotal role in both casting doubt on the RP as a fundamental principle of

[53]See Lepage [1989], p13; also Weinberg [1995b] p520.

[54]As Burgess ([2007], p34) puts it: 'Renormalizable theories represent the special case for which it suffices to work to only zeroth order in the ratio p/M. This can be thought of as the reason why renormalizable theories play such an important role in physics.' Note that this does not mean that we can *only* detect renormalizable interactions, since symmetry breaking and other 'exotic' effects can make certain otherwise highly suppressed processes detectable. See e.g. Weinberg [1980b].

nature and explaining its practical usefulness. However, those same dimensional considerations that make clear that nonrenormalizable theories are workable at relatively low energy also make clear that they inevitably lose all predictive power at the order of the mass scale M that characterizes its leading coupling (cf. equation (6.10)), as then we are into the regime in which the features that initially caused the aversion to nonrenormalizable theories begin to surface again. Furthermore, 'were we to take the expansions literally' when the energy exceeds that value, 'the results for S-matrix elements would violate unitarity bounds' demanded by quantum mechanics, since the terms now grow successively (and rapidly) larger.[55] To put things a little more precisely, there seem to be just two possibilities for what happens at such energies. One is the seemingly magic circumstance that the 'growing strength of the effects of the nonrenormalizable interactions somehow saturates, avoiding any conflict with unitarity'.[56] The other is that unitarity, and hence the consistency of the theory as a description of quantum systems, does indeed break down at the scale M, signifying that 'new physics of some sort enters' at that scale.[57]

The first possibility just mentioned is expressed by saying that the theory is 'asymptotically safe'. That such a conspiracy of couplings is possible might initially strike one as implausible, and indeed there are reasons to expect it to be an extremely rare property of theories. While I will have at least something more to say on it towards the end, for now I will put this possibility to one side. The reason I do this is that the entire discussion of renormalizable and effective quantum field theories that I have engaged in so far has *assumed perturbative analysis*. For example, these two classes of theories have been defined in terms of the dimension of their couplings via the superficial degree of divergence; but the latter was computed via considerations based on the structure of Feynman diagrams, which are in turn expressive of the structure of the terms that appear in a perturbative expansion. But the relevance of this to the phenomenon of asymptotic safety is that, *insofar as we conceive of QFT in perturbative terms*, and for reasons that will be gestured at in Section 10, *renormalizability is a necessary condition of asymptotic safety*. I therefore cannot discuss the possibility of the asymptotic safety of a *nonrenormalizable*

[55] Weinberg [1995b], p523.
[56] See Weinberg [1995b], p523, footnote 15.
[57] Weinberg [1995b], p523.

theory without essentially abandoning everything I have presented so far. Moreover, while there are at present some QFTs (such as lattice QCD) that are amenable, at least to an impressive degree, to workable non-perturbative techniques, there is no known non-perturbative method that works for QFTs *in general* – making an appropriately general discussion of non-perturbative QFT all but impossible at this time.[58] This remainder of this chapter will therefore primarily investigate the fundamentality implications of QFT through the lens of perturbation theory; I will, however, consider the effect of lifting the perturbative assumption at the end of the chapter.

For now, therefore, I focus just on the second possibility, namely that the consistency of effective theories *does indeed break down* at some high but nonetheless finite energy scale. Since such theories cannot be regarded as *fundamental* theories, owing to their breakdown at some high but finite energy, nonrenormalizable theories are thus alternatively denoted *effective field theories* ('EFTs'). They are 'field theories' in that one employs the full field-theoretic formalism in constructing them, but 'effective' betokens the fact that they do the duty for a more fundamental theory that must take over when the energy gets high enough – where 'high enough' is indicated by the mass scale associated with the nonrenormalizable couplings that they by definition contain.[59]

These EFTs can with justification be regarded as highly novel entrants into physics. The reason for this is that – *modulo* the above disclaimer regarding asymptotic safety – any such theory may be said to 'contain the seeds of its own destruction' within its very structure, and indeed to wear this fact upon its sleeve.[60] As Zinn-Justin put it,

> the main difference between [effective] quantum field theory and non-relativistic quantum mechanics or Newtonian mechanics [is

[58]Of course, one could say something at least *similar* for perturbative QFT, since that method only works for regimes in which the theory's couplings are small. The relevant difference, however, is that all theories with small couplings may be treated the same way, *viz.* by perturbation theory; by contrast, theories that cannot be treated perturbatively will in general require different techniques and treatments in different cases.

[59]See Manohar [1996] for a very nice introduction to the concept of effective field theories.

[60]Collins *op. cit*, p123.

> that in the latter] the *mathematics* doesn't tell you that it is just an approximation. Mathematically it is a fine theory. You know just from empirical evidence that it is an approximation.[61]

Such a theory is to be contrasted with a renormalizable theory, the hallmark of which is that it claims to be 'complete in itself', containing 'no direct indications of whether it is part of a larger and more complete theory.'[62] But now that the RP has been dropped as a requirement on theories in general, and given that any putatively renormalizable theory can consistently be *regarded* as at root nonrenormalizable but with the nonrenormalizable effects sufficiently suppressed, whether we ought to regard *any* theory as in fact possessing this property must be regarded as in doubt. Indeed, today even our most fundamental theory, the Standard Model, is regarded as at best effective on account of its many perceived imperfections, even though we standardly present the equations as containing only renormalizable terms. As Weinberg puts it,

> We now think the field equations of the Standard Model are not of the very simple type that would be renormalizable but that they actually contain every conceivable term that is consistent with the symmetries of the theory.[63]

It is at this juncture that we begin to glimpse how the internal structure of QFT may have direct implications for the debate over the existence of a fundamental level. After all, if a theory fails to satisfy the RP, *it cannot be a fundamental theory* (from the perspective of perturbation theory at least), but what grounds there are for regarding *any* theory as satisfying this principle are, by this point, very unclear. Before I develop that thought regarding QFT's fundamentality dialectic, however, I need to establish the sort of *levels structure* that we may take EFTs to define.

[61] Zinn-Justin [2009].
[62] Collins [1984], p123.
[63] Weinberg [1993], p165; see also Weinberg [2009a], p13.

6.7 The Construction of an EFT

To convey the relationships that successive EFTs stand in, I will work within the dimensional regularization scheme and begin with the case in which a higher-energy theory is assumed to be known, but one nonetheless wishes to work with an 'effective' counterpart that suffices to capture its low-energy phenomena. Considering EFTs subject to this assumption will help to give a sense of the principles through which a sequence of EFTs is structured, independently of whether we are in fact in possession of such a theory.[64]

In order to obtain a low-energy EFT from a higher-energy theory, an intuitive set of steps, with their intellectual origin in the work of Wilson, must be undertaken. These are as follows.[65]

(i) The degrees of freedom appropriate at a given high-energy scale μ must be chosen. Only these will be explicitly taken into account.

(ii) The Lagrangian of the full theory at μ is given by $L_0(\phi, \Phi)$, where the Φ denote the light fields with $m << \mu$ and Φ the heavy fields with heaviest mass $M \leq \mu$. This is then divided into two pieces: $L_0 = L(\phi) + L(\phi, \Phi)$.

(iii) The Callen-Symanzik ('renormalization group') equation (see Section 2) is then used to scale the theory's parameters and amplitudes down to $\mu = M$. To proceed further down in energy, one *integrates out* the heavy fields from the action.

(iv) One obtains in this way a Lagrangian of the form $L(\phi) + \delta L(\phi)$, where $\delta L(\phi)$ is a string of nonrenormalizable interactions among the light fields that can be organized as an expansion in powers of $1/M$. Non-local heavy particle exchanges are in so doing replaced by a tower of local (nonrenormalizable) interactions among the light particles.[66] Note that this act of integrating out preserves symmetries.[67]

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[64]It is a primary *pragmatic* virtue of such 'top-down' EFTs that they facilitate the study of low-energy phenomena associated with some interaction without us having to take into account phenomena that may not be relevant at such low energies.

[65]The following is adapted from Pich [1998] p13; p34-5.

[66]See Burgess [2007], p14.

[67]Note that although the process of 'integrating out' preserves symmetries, at sufficiently low energy the EFT may *appear* to have more symmetries than the underlying theory: that is, the Lagrangian consisting only of terms that are non-

(v) The coefficients that are going to be relevant to a given accuracy can be read off directly from the resultant Lagrangian.[68] Those that are not relevant can be ignored. These parameters are then subject to minimal subtraction (meaning that only the pole part ϵ – where ϵ is the parameter which analytically continues the dimension of spacetime – is removed in order to define the renormalized coupling), and the resultant renormalized quantities are determined by *matching* the high and low energy theories at the scale $\mu = M$: that is, by demanding that the two theories produce the same S-matrix elements at that scale. The information on the heavier degrees of freedom is then contained in the couplings of the resulting low-energy Lagrangian and the suppressed nonrenormalizable interactions. The parameters of $L(\phi)$ are not the same in the high- and low-energy theories; the differences are also given by the matching conditions.[69]

(vi) This procedure can then be iterated as the renormalization scale passes through another particle mass, if any.

The EFT so constructed describes the low-energy physics, to a given accuracy ϵ, in terms of a finite set of parameters. It has the same infra-red (but, of course, different ultra-violet) behaviour from the underlying theory, and the only remnants of the high-energy dynamics are in the low-energy couplings and the symmetries of the EFT. It is clear that this process can be repeated to scale down to the next effective theory with some medium-mass particles omitted, and so on, obtaining a 'descending sequence of effective theories', each one with fewer fields and more interaction terms than the last.[70]

The above steps describe the construction of an EFT when a more complete theory is known. But they should nonetheless suffice to convey the relations that EFTs stand in independently of our knowledge of the relevant high-energy theory. Since the process of 'integrating out' that relates each pair of neighouring EFTs is irreversible, it is clear that one cannot simply run

negligible at low energy may possess symmetries which are absent from the full Lagrangian. See, e.g., Brading and Castellani [2008], Section 4.1.

[68] At least assuming the dimensionally regularized framework: see Manohar [1996], p18.

[69] See Pich [1998], Section 3.3.

[70] Georgi [1993], p6.

it backwards and recover a more complete theory from a given EFT.[71] In this effective picture, then, we apparently obtain a sequence of EFTs, each occupying a portion of energy space with boundaries set by particle masses, and such that a given (relatively) low-energy theory may be deduced from a high-energy theory but not *vice versa*. The claim regarding portions of energy space is, however, unfortunately a little hasty, because there remain certain technical obstacles that must be overcome if we are to sustain this picture of neatly-parceled out regions of energy space in which successive theories reign. The reason for this is predicated on the fact that EFTs *are* QFTs, and as such are not well-defined until a renormalization prescription has been given – at least for those terms that are regarded as non-negligible at the energy in question.[72] But the relationships between the various renormalization prescriptions are more complicated in the case of EFTs, and if not chosen correctly the intuitive and most distinctive feature of them – that they describe finite, parcelled-out regions of energy space – is threatened, since any such EFT will apparently receive contributions from all over energy space. Nonetheless, it appears that the problem may be resolved through the process of 'decoupling subtraction', though I will refer only to the literature on this issue here.[73]

Given that decoupling subtraction apparently succeeds in delimiting the energy range appropriate to each theory, the domains of the successive EFTs can after all be viewed as 'stacking up' into well-delineated layers. In doing so, they provide a *levels structure* formulated within the conceptual framework of QFT, and as such a levels structure that may be deduced with recourse only to an extant physical theory (or 'framework').[74] Moreover, since a

[71]It is the irreversiblilty of the integration, on account of the decrease in variables at each iteration of the process, that makes the Wilsonian 'renormalization group' not a group but at best a semi-group (cf. Peskin and Schroeder p401; Fisher [1998]).

[72]Manohar [1996] p.17: 'To use the effective Lagrangian beyond tree level, it is necessary to give a renormalization scheme as part of the definition of the effective field theory. Without this additional information, the effective Lagrangian is meaningless.' (Huggett and Weingard [1995] note that this undermines Cao and Schweber's argument for EFTS and anti-fundamentalism, which is largely predicated on their discomfiture with the renormalization procedure.)

[73]See e.g. Manohar [1996]; Kaplan [1995]; Burgess [2007]; Polchinski [2009].

[74]This has already been pointed out by Castellani, who writes: 'The EFT approach provides a level structure of theories... The basic question concerning the inter-level relationships... can here be addressed in a concrete and definite manner: we have formal and substantial tools for determining how successive effective theories are related to each other. Moreover, it is particularly advantageous from the viewpoint

lower-energy theory may be obtained from a higher-energy theory by a process that is in general *irreversible*, these relations of nomic derivation that relate neighbouring theories have an asymmetric structure. Putting all this together, and recalling the list of possible priority relations discussed in Chapter 2, we may say that theories that are related in this way stand in well-defined, asymmetric relations of *relative fundamentality*. Now, the Lorentz invariance assumed by QFT obviously implies the continuity of spacetime; the inhomogeneous Lorentz group, after all, is a Lie group, and this assumes that its parameters are continuous. Moreover, the Heisenberg uncertainty relations are also encoded in the formalism via the canonical commutation relations.[75] Putting these two together we then know – as is any case evident from the upper limits on the S-matrix integrals, such as (6.5) and (6.6), that these basic assumptions imply – that QFT implies the existence of an infinite energy range. We may therefore state with confidence that the energy range against which successive QFTs stack up must likewise be infinite. Whether that involves infinitely many *theories*, such that there is no one distinguished theory lying at the top of the hierarchy, is the crucial question to which I at long last turn.[76]

6.8 Where will it all end?

The preceding discussion has recounted how the renormalization principle underwent a radical change in status between the early days of QFT and today. It began by being viewed as a *sine qua non* of any empirical theory, and even at times as a fundamental principle of nature on a par with symmetry principles.

of the philosophical discussion that the conceptual framework always remains the same. All the theories are formulated in the same QFT language, thus allowing us to avoid the typical translation problems arising when discussing "heterogeneous" inter-theoretical relationships' (Castellani [2002], pp263-4).

[75]See e.g. Weinberg [1995b], p19.

[76]It is worth noting here that Cao and Schweber, in formulating their 'infinite tower' interpretation of QFT, disparage the method of dimensional regularization on the grounds that it is 'untenably formalistic' (cf. Huggett and Weingard [1995]) and thus favour the Wilsonian hard cut-off approach. But I do not see how we can deduce the fact – which is necessary for the interpretation they defend – that the energy range available for theories to stack up against is infinite without the assumption of Lorentz invariance, which the hard-cut off approach contradicts.

But that it need not after all be seen as a necessary requirement on empirical QFTS was revealed through simple dimensional analysis; whether it can still have a claim to nonetheless being a fundamental principle is something that I will consider presently. But in order to do so, it is crucial to recall that the same dimensional considerations that underwrite the empirical efficacy of theories that fail to satify the RP also imply the non-fundamental status of those theories. As we have seen, any EFT can be used consistently and predictively until the energy of the processes under study approaches the theory's cut-off, at which point the theory blows up, falls foul of unitarity, ceases to be consistent with the basic principles of quantum theory and must therefore by supplanted.[77] But this predicament that EFTs are inescapably enmeshed in naturally invites the question of what it is that happens *then*, since the train of thought that led us to this point has a distinctly regressive character. If we regard the new theory as an EFT, then it too must eventually be supplanted, and we are back with the question with which we started; but if, on the other hand, we regard the new theory as containing only renormalizable interactions, then one must ask why the reasons so far given as to why some ostensibly renormalizable theories ought in fact to be regarded as just the low-energy 'surfaces' of EFTs fail to apply in this case. It is this regress inherent within the EFT concept that has prompted claims that contemporary high-energy physics demands a radical revision of standard assumptions concerning the existence of a fundamental level, and that prompts a full discussion of the issue in this work.

In the absence of further arguments, however, such anti-fundamentalist claims on behalf of QFT are too quick – for there are in fact a variety of forks in the road currently lying before us.[78] In an oft-quoted passage in the literature on EFTs and their implications for fundamentality, the effective field

[77]To repeat, there is a theoretical possibility that a given EFT might 'saturate' through the phenomenon of asymptotic safety and avoid the conflict with unitarity/ But since in the main body of this chapter I assume a perturbative treatment of QFT, this isn't one that I will countenance here. I will however consider this issue briefly in Section 10.

[78]As Huggett and Weinbard have noted, it is too quick to move, as Cao and Schweber do, from the regressive situation outlined above to the idea that there is no fundamental theory but only a 'tower' of ever-more fundamental ones, for 'an EFTer cannot infer from the *possibility* of a tower of theories that the alternatives are logically or physically untenable' (Huggett and Weingard [1995], p187; italics added).

theorist Georgi lays out the possible alternatives that present themselves at this juncture.

> One can imagine, I suppose, that this tower of effective theories goes up to arbitrarily high energies in a kind of infinite regression. This is a peculiar scenario in which there is really no complete theory of physics, just a series of layers without end. More likely, the series does terminate, either because eventually we come to the final renormalizable theory of the world, or (most plausible) because at some very large energy (the Planck mass?) the laws of relativistic quantum field theory break down and an effective quantum field theory is no longer adequate to describe physics.[79]

There are thus *three* possibile states of affairs at sufficiently high energies, one and at most one of which must obtain. The first of these is

(i) the breakdown of the quantum field theoretical framework altogether, to be replaced by another framework of a qualitatively different sort, such as a string theory, most plausibly at the Planck scale.[80]

The other two, however, are each compatible with QFT. They are

(ii) the retention of a quantum field theoretical framework and its culmination in a final, perhaps unified, but in any case renormalizable theory;[81] and

(iii) an infinite tower of EFTs – that is, an infinite tower of theories each of which is superceded at some finite energy by a higher-energy theory *ad infinitum*.

While the existence of these three mutually exclusive possibilities has been noted in many places, not a great deal has been said on whether any one of them can be regarded as a better-supported resolution of the apparent regress than any of the others. By drawing on various themes surrounding the concept of fundamentality as understood in contemporary physics and

[79] Georgi [2009], p138; see also Georgi [1993], p6 and Georgi [1992], p456.

[80] There is in fact a sense in which string theory is a *kind* of quantum field theory, but not in 4 dimensions: see Weinberg [1997a].

[81] Georgi [1993] states that he regards this as in fact unlikely, given the difficulty with gravity, but there are now models of gravity which, though nonrenormalizable, show promise of being asymptotically safe. I will return to this again in Section 10.

metaphysics, and by considering the demands of the internal approach to fundamentality questions, I will now consider whether we have any reasons to favour any one of these possibilities over each of the others. I begin with the first possiblity on Georgi's list: the overthrow of QFT.

6.8.1 The Overthrow of QFT

Of all of these options, it is probably fair to say that this is the possibility favoured by most physicists. As Georgi suggests, it is perceived as very likely that attempts to quantize gravity will have to confront the prospect of a fundamental length as part of the structure of spacetime at the Planck scale defined by the three units G, h, c. If that is the case, then the idea that spacetime is continuous will presumably have to be jettisoned, which in turn implies that Lorentz invariance will likewise have to go by the wayside – not to mention the entire local operator formalism of QFT.[82] Planck-scale considerations therefore seem to portend the downfall of QFT, and it is common to find such considerations motivating interpretations of both the ultimate outcome of the regress mentioned a moment ago and the renormalization process itself.[83]

In spite of its seeming popularity, however, the idea that we can legitimately use Planck-scale considerations to settle issues posed within the framework of QFT has been questioned by Fraser. She objects that 'gravitational considerations are external to QFT', and that as such that the Planck length cannot be marshalled in settling the outcome of the regress of theories that QFT presents.[84] The 'externality' of Planck-scale considerations to QFT is reflected in the fact that the former are taken to involve a discrete structure to spacetime that conflicts with Lorentz invariance, and as such theories involving the Planck length may be taken to commit to 'different sets of theoretical principles' than QFT.[85] Now, as Wallace has pointed out, in general it is 'perfectly reasonable (and not at all *ad hoc*) to be motivated to believe in a discrete structure to spacetime because it is independently motivated by

[82]As Georgi puts it, cut-offs 'require giving up one or more of the cherished principles which led to local interaction in the first place' (Georgi [1992], p449).

[83]See e.g. Wallace [2011], Peskin and Schroeder [1995], Chapter 12.

[84]Fraser [2009], p552.

[85]See Fraser [2009], p561.

various strands in theoretical physics,' even if QFT isn't one of them, *and* to use that to motivate a view as to the high-energy fate of QFT.[86] Moreover, that is so in spite of the fact that it has the *prima facie* conspicuous consequence that QFT itself must break down. Wallace himself chooses to adopt precisely this line on the fate of QFT, and in support of his view he notes that this position is 'pretty commonly taken by particle physicists' – and indeed Georgi seems to be one of them.[87]

But two points cry out to be made regarding this strategy of using Planck-scale considerations to motivate a stance on the high-energy behaviour of QFT. The first of these points is of general significance and the second more *apropos* to the specific task in hand. The first of these points is that the significance of the Planck units to a future of theory of spacetime remains a 'worryingly unchallenged assumption' that is by no means certain.[88] While it is often simply taken for granted that the Planck length represents a physical joint in nature, it must be realized that this idea deserves to be met with a more critical attitude than is often the case. As Meschini reminds us, the Planck units were introduced simply to provide a less anthropocentric set of units than had been used hitherto, and one must provide reasons as to why they are of any greater significance than that. After all, 'the chances are that any combination of three dimensional constants chosen at random would allow the same procedure' of defining new dimensional scales, and we presumably do not think that a physical scale set by an arbitrary combination of constants necessarily indicates some physical joint in the world.[89] What is presumably needed before any significance can be assigned to scales defined in this way, such as the Planck length, is an overarching theory in which those constants appear – some *equations* whose other terms already have some appreciable physical meaning – which can be used to infer what, if anything, of physical significance it is that these newly-defined scales pick out. But it is precisely this that is lacking in the case of a quantum theory of gravity.[90] Meschini also argues, citing Baez, that inferring features of the short-distance structure of spacetime through considerations of these constants 'presupposes that in a

[86]Wallace [2011], p122.
[87]*Ibid.*
[88]Meschini [2007], p1.
[89]Bridgman [1963], p101; quoted in Meschini *ibid.*, p12.
[90]Meschini makes this point with reference to the Compton wavelength in the context of atomic physics; see *ibid.*, Section 3.

future theory of spacetime, and any observations related to it, the combination of already known physics – and nothing else – will prove to be signficant'.[91] But he uses an example involving atomic theory prior to Planck's resolution of the black-body problem to show that neglecting then-unknown constants in trying to define the scales relevant to atomic physics can generate wildly wrong results. Furthermore, he argues that there are reasons to be sceptical that all of the constants that define the Planck length will in fact be relevant in a quantum theory of gravity – as they must be if the Planck length is to be significant there.[92] For all these reasons, the idea that the fact that the Planck length can be defined provides compelling support for the idea that QFT must ultimately break down, as Georgi and Wallace apparently believe, must be taken with a large pinch of salt.[93]

That first point invites the second, which is that Georgi and Wallace's preferred stance on the high-energy fate of QFT does not issue from QFT itself, but from QFT conjoined with *another* theory – and one that, moreover, remains highly speculative at present. But what I am interested in in Part 1 of this thesis is whether robust *internal* arguments against fundamentality are possible – that is, whether arguments against fundamentality can be framed from within the persective of *extant* physical theories – and in particular, in this chapter, with whether QFT furnishes us with an example of one. Adopting the option outlined here, however, amounts to (1) giving up on investigating the fundamentality conclusions that may be drawn *from within QFT*, and instead (2) investigating the fundamentality conclusions that may be drawn from QFT

[91]*Ibid.*, p5.

[92]As he descusses, there are at least *conjectural* models in which quantum effects reside in the manifold 'before' the metric field is laid on, thus rendering G and c redundant to the fundamental theory.

[93]This is not of course to say that the Planck length, and the way in which the notion was arrived at, might not represent a useful *heuristic* when it comes to attempting to construct a quantum theory of gravity (a point on which Meschini concurs: see *ibid.*, p8). But it must be remembered that the Planck length is just that: a potential *heuristic*, and not by any means an established fact (on which Meschini likewise concurs; see *ibid.*). I should perhaps say, however, that I am unclear on whether the Planck length can be usefully regarded as a potential 'footprint', in Post's sense, of a future theory in a past theory, in the sense that the numerical equivalence of inertial and gravitional mass in Newton's theory may have been (cf. Post [1971], Section II (2)). Whether we can indeed regard it as such is clearly going to be a function of whether we in fact have what can be regarded as a working *theory* that contains all of these constants, but that does not seem to be the case here; see below.

conjoined with an *as-yet unknown theory*.[94] Citing the notion of the Planck length to settle the question of how the tower of QFTs ends is thus doubly objectionable from the present point of view. The internal arguments against fundamentality that I consider here must therefore be those that leave the basic principles of QFT intact, and I turn now to investigating those remaining two possibilities.

6.8.2 A Final Renormalizable Theory

The two remaining options constitute the dichotomy that the QFT framework – so far at least – apparently *underdetermines*, and what I want to do now is consider what support, if any, can be provided for breaking the underdetermination one way or the other. I will start off by considering what could motivate favouring the view that there exists a final, hence renormalizable quantum field theory lying atop the tower.[95]

In the earlier sections of this chapter, I described and defended the consistency of merely *effective* theories. I argued that EFTs are perfectly workable as low-

[94]As Wallace puts it, 'we have only the sketchiest idea of what X [the unknown quantum theory of gravity] will turn out to be' (Wallace [2006], p46.). Now, while theories may not, of course, have precisely defined boundaries – witness for example the debate over whether Gibb's paradox tells us something novel about classical particles or rather indicates that classical mechanics is simply inadequate to describe the relevant thermodynamic phenomena (cf. French and Krause [2006], Section 2.5) – I take it that is uncontroversial that we can regard QFT and this unknown theory as distinct. (After all, if we had reason *not* to regard this theory as distinct from the QFT that we already have to hand, we would presumably have more than 'the sketchiest idea' of what this theory in fact is!) I note finally that none of this implies that any eventual theory of quantum gravity may not itself have interesting anti-fundamentalist interpretations; any such implications are, however, completely beside the point for present purposes.

[95]At least one *bad* reason has been given, and it is that proferred by Cao and Schweber in favour of the anti-fundamentalist interpretation. As Huggett and Weinberg point out, 'the only positive argument [in Cao and Schweber's paper] is the suggesion that the renormalization involved in the GUT [i.e. final renormalizable theory] approach is untenably formalistic' (Huggett and Weingard [1995], p187). But even putting aside their claim that renormalization *is* reasonably well-understood, as pointed out in Section 7 the friend of EFTs must likewise renormalize their theories. Thus fresh reasons must be sought for defending the anti-fundamentalist horn of the dilemma, and I will consider some possible avenues that may lead to this end in the next section.

energy theories, so that it is consistent to regard our current theories, such as those of the Standard Model, as counting amongst them; I also pointed out that it is usual among contemporary physicists *to in fact* so regard them. But clearly none of these points have any bite in *this* context: all they show, after all, is that renormalizability is not a necessary property of theories *at currently accessible energies*. Here, however, I am considering whether there exists an (as-yet unknown) fundamental theory; since it remains that a final theory must be valid to arbitrarily high energies, that entails, against the backdrop of our perturbative assumptions, that it must be renormalizable. Therefore, since we do not regard our current theories as the end of the story, whatever justification we may have for assuming that *they* can be regarded as EFTs is not to the point here.[96]

I have, however, raised a point that could be invoked in this context, and that is the perceived lack of foundation for the satisfaction of the RP. Recall from Section 4 that physicists have mooted that there does not seem to be any clear physical reason why any law of nature should be found to respect the RP in addition to the other principles that must govern a Lagrangian in QFT and that one 'cannot imagine modifying' – namely, that they should respect the basic QFT principles and any symmetries that are assumed to govern the relevant interaction. Given the perception amongst sections of the QFT community that it is unclear *why* a theory should conform to the RP, we can legitimately ask whether we should expect *any* theory to in fact do so. We know that renormalizability is a necessary condition for a fundamental theory within (perturbative) QFT; but why should we think it is in fact ever satisfied? In other words, what would *explain* the existence of a theory, towering above all others, which happened to respect this otherwise inexplicated principle?

An obvious gambit at this point would be to say that, even though we might *ordinarily* feel under pressure to explain features of theories that are judged to be otherwise surprising, the need to explain the satisfaction of the RP by a fundamental theory is simply obviated by the fact that *that theory is fundamental*. Since the satisfaction of the principle in such a case would be an aspect of the fundamental basis, we might be happy to view it simply as part of the metaphysical bedrock of the world and, as such, not the sort of thing that could admit of explanation. Indeed, priority relations are often taken

[96]This point is also made in Castellani [2002], p264.

to map onto relations of explanation, so that the fundamental is frequently presented in metaphysics as that which is brute, primitive, and resistant to explanation.[97] Given that, it might therefore seem that we can relegate the satisfaction of the RP by a fundamental law to brute and inexplicable fact.

While that line of argument is a tempting one, I think we should in fact be hesitant about adopting it. While it may be very common *in metaphysics* to take the fundamental as by definition brute and inexplicable, it is increasingly less common for *physicists* to adopt this attitude. It seems, in fact, that physicists in the 20th century have grown increasingly uncomfortable with taking *anything* as brute and inexplicable – something testified to by the fact that there have by now been numerous attempts to explain things that physics was previously presumed to necessarily take for granted, such as that the universe appears ordered, had the initial conditions that it did or even that it exists at all.[98] Weinberg reflects on this broad change in outlook regarding the fundamental by reporting that, at present, 'the aim of physics at its most fundamental level is not just to describe the world, *but to explain why it is the way it is*'.[99] But if that is the aim when it comes to the physics of the fundamental level – should it exist – then merely citing that something is fundamental need not obviate the demand that some explanation be given of it. Indeed, one need only think of physicists' efforts to explain the values of the fundamental constants, which appear in the most fundamental laws of nature that we know of, in order to grasp the point. And while the idea that we might be able to explain the fundamental is, in itself, just a stated view from physics, we may note that Nozick has explicitly defended the logical and philosophical *coherence* of the idea that the fundamental may be amenable to explanation.[100] This should compound our unease about the idea that we are *a priori* licensed to take features of an assumed fundamental basis as simply

[97] See for example Jenkins [2011], deRossett [2010]. I have not explicitly discussed the relationship between fundamentality and explanation here, partly due to the complexity of the subject of explanation. Bringing explanation more explicitly into the fold thus represents a further avenue for development of this project.

[98] Concerning the explanation of why the universe appears ordered, see Wheeler [1983]. For an introduction to speculative cosmological explanations of why the universe exists at all and has the initial conditions that it has, see Davies [1996].

[99] Weinberg [1993], p175; italics added.

[100] See Nozick [1981], Chapter 2. This defence draws heavily on Kripke's theory of truth, a theory that – unlike Tarski's – permits reflexive truthmaking relations.

brute, and that in turn should discourage us from shrugging off the need to explain why any law should satisfy the RP.

One may put things in still stronger terms, however, since one may argue that the very fact that the satisfaction of this principle has been regarded by physicists as ultimately perplexing *positively counts against* the idea that any fundamental theory does in fact satisfy it. This is because it is often asserted – once again, at least within physics – that the mark of a truly fundamental principle of nature is that it somehow invokes in us the sense that nature *could not but* have satisfied it: it should present itself to us with a clarity and inevitability that makes it hard to understand how we failed to recognize previously that such a principle had to be satisfied. This sentiment that the fundamental will present itself to us as inevitable and natural may for example be found in Wigner, who notes that

> It is often said that the objective of physics is the explanation of nature, or at least of inanimate nature. What do we mean by explanation? It is the establishment of a few simple principles which describe the properties of what is to be explained. If we understand something, its behavior, that is the events which it presents, should not produce any surprises for us. *We should always have the impression that it could not be otherwise.*[101]

But if some quarters of the physics community regard it as the mark of a fundamental principle that it leaves us with the impression that somehow nature 'could not be otherwise' than respectful of it, the very fact that the satisfaction of the RP has been regarded as so physically unintuitive seems to make it a bad candidate for one. And of course, if we *do* reject on these grounds that fundamental nature in fact obeys this principle, then it follows – *modulo* our perturbative assumptions – that *there is no fundamental law.*

These considerations concerning how the fundamental is conceived of in physics – specifically, the ideas that it (i) may be explicable and (ii) ought to strike us as natural and inevitiable – have thus failed to furnish a defence

[101]Wigner [1963], italics added. I note that I am not *defending* the idea that the fundamental will in fact have this impact upon our psychology, merely at this point reporting that this view is held amongst influential members of the physics community. (Nor do I intend to convey that the notion of 'simplicity' of principles that Wigner cites here has any clear and unproblematic meaning.)

of fundamentalism in QFT. Indeed, they seem rather to have steered us more in the direction of the anti-fundamentalist horn. It is moreover unclear what other non-question-begging grounds we could adduce in support of fundamentalism. Certainly, if the only reason for holding that there exists a theory which satisfies the RP is to secure the existence of a fundamental basis, then that clearly cannot be appealed to in this context. Nonetheless, and as should be abundantly clear by this point, to say that the considerations I have just adduced in any way *suffice* for the anti-fundamentalist to declare a victory over her rival would be stretching things beyond breaking point. The arguments, after all, revolved around the properties that physicists *think*, or would *like*, the fundamental to have, but that does not in any way imply that the fundamental *will in fact have* any of these properties. Indeed, the anti-fundamentalist cannot herself consistently deny this: physicists often state, for example, that they *aim*, and thus *hope*, to discover the 'fundamental building blocks of nature', but the anti-fundamentalist will of course not take *that* to entail that there are in fact any out there to be discovered. Thus, while the above considerations certainly problematize the uncritical assumption that there is a fundamental theory, and should give the fundamentalist pause, I have not found any reason to rule out the existence of such a theory – a theory that, although otherwise unmotivated, after all seems perfectly consistent with the basic principles of QFT.

At the moment, then, we seem to lack a clear reason for either adopting the fundamentalist stance on QFT or for ruling it out. What I want to do now is see if any positive arguments can be given for the opposing point of view, and thus consider further whether QFT may be said to positively support – as opposed to merely permit – anti-fundamentalism about laws.

6.8.3 An Infinite Tower of Theories

It remains, so far, that QFT *underdetermines* whether there exists a fundamental law of nature or not. As discussed in Chapter 3, the virtuous features often cited in the face of underdetermination can in general only be taken to constitute rational grounds for making differential commitments if they constitute good *epistemic* grounds for doing so. And as I also argued there, one obvious way in which we can infer that one theory is less likely than another is if the former *places more constraints* on a given ontology than the latter does. After all, this would mean that it could be represented as the latter theory *conjoined with* some extra propositions expressing those constraints, and hence mean that it is is more difficult to satisfy.

But one may well wonder whether this observation might be of interest to those who want to defend anti-fundamentalism about QFT. The reason for this, of course, is that renormalizable theories must satisfy all the constraints imposed on effective theories, *as well as* conform to the RP. That is, while the only criteria governing whether a term ought to be included in an effective Lagrangian – recalling Donogue's list above – is that the resultant law (i) respects the principles of relativistic quantum mechanics, such as Lorentz invariance and unitarity, and (ii) respects any other symmetries of the domain in question, renormalizable theories must obey the additional – and highly stringent – constraint that they feature only renormalizable terms. One may therefore legitimately claim that anti-fundamentalist worlds have *fewer primitive constraints* governing their stock of laws than fundamentalist worlds do. The anti-fundamentalist might therefore be tempted to infer that the *theories* of anti-fundamentalist quantum field-theoretic worlds are therefore more likely to be true than the theories of fundamentalist worlds, since the former place *fewer demands* on their (nomic) ontology than the latter do. If that were correct, it seems that they would be vindicated in vouching for an anti-fundamentalist interpretation of *this* quantum field-theoretic world as a consequence.

Given that observation regarding the relative parsimony of primitive principles in anti-fundamentalist worlds, can we therefore say that the anti-fundamentalist interpretation of QFT is the one which is more likely to be true? Unfortunate as it may be from the anti-fundamentalist's perspective, I do not think that any such conclusion would be justified. In fact, I do not think that we can move from this sort of claim regarding parsimony of primitive principles to anti-fundamentalism about laws for the same sort of reasons that Sider's appeal to parsimony of primitive predicates failed to warrant fundamentalism about objects. The reason for this, of course, is that just as 'is a part of' is a predicate with ontological implications when it comes to objects, so is 'satisfies the RP' when it comes to quantum field-theoretic laws of nature. That is, just as two worlds cannot differ with respect to whether the parthood predicate is required to adequately describe their (objectual) ontologies without thereby also differing with respect to those ontologies, so two worlds cannot differ with respect to whether the RP must be satisfied by some element of their (nomic) ontologies without thereby also differing with respect to those ontologies.[102] Thus, just as in Sider's case, we do not have two different theories about a shared ontology – the contents of a world – one of which places additional demands on that ontology relative to the other, and as such is less likely to be true. Rather, we have two different theories describing two *different ontologies*, so that the extra constraint involved in the theory $T(w_f)$ of the fundamentalist world w_f cannot be represented simply by means of a proposition *conjoined onto* the anti-fundamentalist theory $T(w_a)$ of an anti-fundamentalist world w_a. But if the additional principle that must be respected in fundamentalist worlds cannot be represented as an additional demand on the ontology of an anti-fundamentalist world, then it is not at all clear how the anti-fundamentalist could exploit their relative parsimony of principles to their advantage – at least not in probabilistic terms. And without any obvious alternative means of exploiting it, it seems that we cannot say that the theories of worlds bereft of laws that are required to satisfy the RP are more likely to be true than their fundamentalist counterparts – or at least not on these sorts of simple logical grounds.

The fact that anti-fundamentalist QFT worlds require fewer in the way of fundamental principles thus does not seem to deliver – at least not in any obvious way – the conclusion that the actual world is more likely to be one. And since, as I have repeatedly insisted, realists may appeal to virtues such as parsimony to ground theory choice only if they can show that those virtues enjoy suitable epistemic support, it is not clear that there is anywhere else for this observation regarding parsimony of principles to go.

But it may be worth taking one more kick at the can. Recall that in Chapter 3, when discussing Cameron, I pointed out that some scientific realists believe that if one can show that appeals to virtues such as simplicity in theory construction have resulted in more empirically successful theories, then such appeals may sanction differential commitments among empirically equivalent theories after all. I pointed out too that if this *a posteriori* strategy can

[102]This point will be revised at the at the end, once the concept of *asymptotic safety* is brought back into the fold.

succeed in epistemically differentiating theories, it can do so only in the case of theories that themselves admit of confirmation (for otherwise there is no justification for invoking the strategy at all). *Here*, however, we are dealing with quantum field theory, which frames the best-confirmed theories of all time; if historical appeals to virtues such as simplicity or parsimony have a hope of being warranted anywhere, then, it seems that they should do so here.

Let us therefore consider whether the anti-fundamentalist can exploit this *a posteriori* strategy to their advantage. What needs to be established if this strategy is to work is that theories that have posited fewer in the way of fundamental principles have historically tended to do better than those theories that have posited more; that is, that theories that are more parsimonious when it comes to fundamental principles have been shown to ultimately be better confirmed. But unfortunately for the anti-fundamentalist, the idea that parsimony about principles has been confirmed by the history of science is plainly untenable. In fact, we need look no further than QFT itself to see that this is the case, since it is palpably obvious that our theories of high-energy processes are more successful given that they respect, say, the unitarity principle *in addition to* the other principles that QFT respects than they would be if they did not do so. Musgrave's strategy thus leads straight into a dead end here, and is therefore of no use in breaking the underdetermination in the anti-fundamentalist's favour.

By way of a last-ditch attempt to salvage things, the anti-fundamentalist might object that there is clearly a difference between principles such as the unitarity principle and the RP, since the former is *empirically fecund* while the latter, it may be claimed, is *empirically superfluous*. The reason that the RP can be claimed to be empirically superfluous is that, as we already know, at *any finite energy* one can find an EFT that is just as consistent and predictive a theory as a renormalizable theory at that energy, for one can consider any renormalizable theory to be an EFT with the non-renormalizable terms sufficiently suppressed. At any finite energy, then, we need not assume that our theory satisfies the RP in order to make all the predictions that a renormalizable theory can. But since *any* of our predictions will inevitably concern phenomena that are measured at a finite energy, that means that we *need never assume* that the RP is satisfied to have a well-confirmed description of reality, and hence that the latter can be regarded as empirically redundant. There is thus a marked distinction between it and the unitarity principle, and one that the anti-fundamentalist might be tempted to exploit. But while the distinction between the two principles is clear, it is equally clear that it is not a useful one for the task currently at hand. The reason for that, of course, is that if two theories differ only in that one adopts an empirically superfluous principle that the other does not, then *neither* theory will prove to be better confirmed than the other – with the result that Musgrave's strategy cannot even be invoked. And while there may be good *methodological* reasons to either favour parsimony about principles in general, or to banish non-empirical elements of whatever sort from our theories wherever we can – the latter, after all, is arguably simply a statement of 'Einstein's razor' (cf. Chapter 3) – it is the very fact that purely methodological considerations do not suffice for grounding theory choice that motivated the use of Musgrave's strategy in the first place.

It therefore appears that the *a posteriori* method of epistemically privileging one of a pair of underdetermined theories cannot be exploited here. But it may be worth flagging up that we should perhaps have reservations about the viability of Musgrave's strategy, quite independently of whether it does the work the anti-fundamentalist wants it to do here. After all, this strategy requires that, of two theories that possess virtue S to varying degrees, the one that exemplifies more in the way of S is ultimately better confirmed than the other, and that this pattern is repeated with at least some other pairs of theories (so that an induction may be cautiously made). It is clear, however, that the pairs of theories for which this claim can be made cannot themselves be underdetermined *by all possible evidence*, and hence in these cases we could simply have played the waiting game before finding good epistemic grounds for choosing between them. The only cases in which we will ultimately *need* to appeal to virtues to settle underdetermination disputes is therefore in the case of theories – such as the two interpretations of QFT at hand – that are tied with respect to *all possible* evidence. But it is then easy to see that the very existence of such cases where we can do *nothing but* use the strategy to decide between them undercuts the evidence for the strategy in the first place. This is because the very existence of theories that are empirically tied yet differ regarding S seems to undermine the claim, supposedly based on historical evidence, that theories with different quantities of S turn out to be differently confirmed in the long run. Thus the very fact that we may sometimes *have no option* but to appeal to Musgrave's strategy seems to undermine the support that the strategy supposedly enjoys. This unhappy situation casts doubt on the idea that the strategy can be used to settle underdetermination disputes in the very cases in which we most need to use it.

In sum, then, the *a posteriori* strategy outlined by Musgrave does not seem to support differential epistemic commitment in favour of the anti-fundamentalist interpretation of QFT – if, indeed, it can support differential commitments anywhere. And given that the *a priori* argument based on the idea that fundamentalist QFT worlds make additional demands on their nomic ontology also failed to deliver, if there is yet justification for favouring the anti-fundamentalist interpretation on epistemic grounds, we must admit that it so far eludes us.

6.9 First Conclusions

In contemplating the question of whether the QFT framework carries anti-fundamentalist implications, I have invoked a variety of considerations. In the light of them, I think that the most appropriate thing to say by way of answering the question is that, while QFT certainly *permits* anti-fundamentalism, in the sense of an infinitely-descending sequence of EFTs, it remains that it *underdetermines* whether there is a fundamental level of laws or not. While there may be good *methodological* reasons that could be given for not committing to the RP as a fundamental principle – such as that empirically superfluous principles that lack independent motivation should be banished from our theories – we nevertheless seem to lack any convincing *epistemic* reason for favouring the anti-fundamentalist interpretation. But while this might seem disappointing for those hoping to defend the idea that we can use physics to deny fundamentality, let it not go unrecognized that *this is in fact a highly non-trivial conclusion*. For one thing, the very fact that we currently have a set of physical principles that collectively *permit* an anti-fundamentalist interpretation runs counter to the picture that Schaffer presented us with, in which physicists make successive fundamentalist assumptions and it is only ever history that proves them wrong. Nor is it in any way obvious that such a set of principles could be found that have this property.[103] And let us also not forget that it is regarded as something of an open question as to whether *any* physical theory, even a putatively fundamental theory, may not turn out to be in exactly the same predicament of possessing an empirically equivalent rival that posits a radically different ontological picture of the world.[104] There are, furthermore, arguably some real examples of such theories, and indeed frameworks for theories, *that are in fact* in precisely this predicament. Lyre, for example, has argued that 'the plethora of rivalling quantum interpretations', such as the GRW, Everettian and collapse-by-consciousness interpretations, cannot be differentiated by any possible evidence in spite of their radical incompatibility.[105] One may therefore argue that the anti-fundamentalist about QFT stands in an analogous relationship to her fundamentalist rival as the Everettian stands to the advocate of GRW, and thus that she has just as much – and as little – a right to believe in her picture of reality as the latter has to believe in theirs. That is clearly a non-trivial achievement for the defender of naturalistic anti-fundamentalism.

Let me therefore sum up what has been shown so far in this chapter.

- *Theories – or theoretical frameworks – may be consistent with both anti-fundamentalist and fundamentalist interpretations.* Unlike in the case of S-matrix theory – in which democracy is unambiguously implied by its core principles, and most notably its 'principle of maximal analyticity

[103] As we have seen, QFT presents a picture of fields interacting in accordance with laws, where each law is related to a specific expression for amplitudes. We know that unless we supplement the principles of QFT with the principle that any given law must not only respect any symmetries applicable to the domain in question, but also contain only renormalizable interactions, then at some energy the amplitudes associated with that law will violate the unitarity principle that is basic to QFT. This implies that the law itself has broken down, and that a new law must govern the portion of spacetime bound from below at that energy. But at that point the argument recurs; since the principles of QFT themselves imply that the available energy space is infinite, the argument may be taken to recur without end. In other words, the specific principles of QFT play an essential role from start to finish in generating this anti-fundamentalist picture, and there is clearly no reason to suppose that *any* old set of physical principles will be capable of producing such an infinitely regressive structure.

[104] See, e.g., Hoefer and Rosenberg [1994].

[105] Lyre [2010].

of the second kind' – QFT does not *demand* anti-fundamentalism about laws. This sort of situation was in fact anticipated at the end of the last chapter, when I noted that it was unlikely that such a characteristically metaphysical claim as that concerning the existence of a fundamental level would in general carry such a determinate empirical signature as it did in the S-matrix context. Nonetheless, the argument shows that anti-fundamentalism *can be at least as well empirically supported* as fundamentalism within a particular theoretical framework, thus showing that physics may be in a position to deny fundamentality to just the same degree that it is able to affirm it. This is clearly a non-trivial achievement. If we want to go further in such cases, underdetermination-breaking strategies must of course be used. This leads on to the next point.

- *Anti-fundamentalism may, in some cases, be motivated only on methodological grounds.* In light of the above discussion, progress with our argument against fundamentality will rest on whether any strategies can be found for defending the truth-tracking nature of the methodological features that may be claimed to be present in (and only in) the anti-fundamentalist interpretation, and in particular, of its jettisoning of an empirically superfluous principle. While there has arguably been renewed interest in the truth-tracking import of theoretical virtues, whether any of it will be of any use in this context for now remains to be seen. (I for one do not see how we could use the methodological advantages gestured at above to confect a compelling *epistemic* argument in favour of anti-fundamentalism.) I will say, however, that the case just discussed demonstrates that baldly stated claims to the effect that fundamentalist worlds are 'simpler' in some obvious and unqualified sense just do not stand up in general.[106] Here, commitment to fewer primitive principles entails a commitment to anti-fundamentalism.

In addition, this argument once again shows that

- *Arguments against fundamentality need not be meta-inductions.* While QFT is best regarded less as a theory than as a 'framework' for theories capable of describing many theories, it remains that none of the strategies used to deny the existence of a fundamental theory were

[106]See, e.g., the works by Sider discussed in Chapter 3 (cf. his [2011a], [2011b]).

meta-inductive in character. Rather, the possibility of the infinite regression issued from a set of physical principles that are available for survey *now*, and the argument over fundamentality turned upon the status of a principle that only has meaning within the QFT context. Thus although we are dealing less with *a theory* than a *framework for theories*, it makes sense to call the anti-fundamentalist arguments that I have considered in this chapter to be thoroughly *internal* arguments.

A final feature, and one that is implicit in the very notion of the internal approach, is once again that

- *Internal arguments against fundamentality are limited.* As pointed out in the last chapter, this is an unavoidable feature of the internal approach. After all, this strategy must always at least *treat* something as fundamental – namely, the physical principles from which the relevant anti-fundamentalist conclusions are derived. And here, of course, what was assumed were the principles of quantum mechanics and relativity that lead to QFT in the first place.

These are the main conclusions that I believe we can draw from this episode. They represent non-trivial accomplishments for the anti-fundamentalist, and I think that they may be regarded as illuminating with respect to how we might deny fundamentality on naturalistic grounds. Nonetheless, they were drawn against the background of *perturbatively analysed* QFT. What I want to discuss, by way of a coda to this chapter, is the significance of this assumption for the anti-fundamentalist's case.

6.10 Coda: Beyond perturbative analysis

So far in this chapter, I have discussed the issue of whether a law can be regarded as fundamental or not in terms of whether or not it satisfies the RP. In particular, I presented this satisfaction as a necessary condition on fundamental laws, since (as discussed in Section 6) the perturbative expansion of the amplitude suggests that nonrenormalizable theories would blow up and thus violate unitarity at some finite energy – hence the designation of such theories as merely 'effective'. Renormalizable theories, by contrast, seem to carry no outward indications that they are doomed to collapse at some point, so that satisfaction of the RP seems to be necessary for consistency in the infinite energy limit. There is, however, another way in which one could choose to contemplate the fundamentality of theories, and one that is in principle independent of perturbation theory. This is through a study of the *β-functions* associated with the theory's couplings. Introduced in above in Section 2, these functions β_i for the ith coupling λ_i are given by (6.8):

$$t\frac{\partial \lambda_i(t)}{\partial t} = \beta_i(\lambda_i, \lambda_j, ...t), \tag{6.8}$$

where t is a momentum scale-up factor. It can thus be seen that the β-functions for all the couplings of a theory will comprise a set of coupled differential equations that are in general highly non-trivial to solve.[107] But there is very good motivation to at least attempt to try to solve them, since these functions are very informative of the high-energy properties of the theory. The reason that a theory's β-functions can yield so much insight in this respect is at root very simple, and it is based upon the fact that generically, if a theory's couplings are finite then its observable quantities – such as its cross-sections, decay rates and amplitudes – can likewise be expected to be finite.[108] For example, one can show that QFT cross-sections σ are expressible as $\sigma = k^{-2}\tilde{\sigma}(X, \tilde{\lambda}_i)$, where X denotes dimensionless kinematical variables (such as scattering angles and ratios of energies), the $\tilde{\lambda}_i$ denote the couplings of the theory expressed in dimensionless units, and k is an external momentum. Thus if some of the couplings of the theory diverge as t – the momentum scale-up parameter – goes to infinity, then this quantity can likewise be expected to diverge, and thus for the unitarity and hence consistency of the theory to also be destroyed in that limit. Conversely, if a theory's couplings remain finite up to arbitrarily high energy, then there is every reason to expect the theory to produce finite observable quantities and thus to be consistent to arbitrarily short distances. The simplest way to achieve the latter scenario is to assume that in the limit that $t \to \infty$, the couplings *stop* evolving as t increases and thus may be expected to retain a finite value.[109] Reference to (6.8) shows that that is just to say that each of

[107]See, e.g., Percacci [ms].
[108]Percacci, [2008], p5; Huggett and Weingard [1995], p178.
[109]See e.g. Percacci [2009]. Though one often reads that having the couplings flow

the β-functions of the theory *vanish* as t increases.

In this alternative approach to the assessment of theories – an approach in which the evolution of the couplings lies centre-stage – it therefore makes sense to view theories as inhabiting a *space* parameterized by all possible QFT couplings. Partly on account of the fact (mentioned above in Section 5) that there are infinitely many possible combinations of fields with any given symmetry, each of which requires its own coupling, this space will be infinite dimensional and may be thought of as 'containing' all possible QFT Lagrangians.[110] The investigation of a theory through the evolution of its couplings is then translated, in this picture, into the study of the *trajectory* that the theory takes through coupling space, where the trajectory is parameterized by the scale-up factor t. The points in coupling space $P = (\lambda_1^*(t), ...\lambda_j^*(t), ...)$ for which $\beta_i = 0$ for all i – indicating that high-energy consistency may be possible – are called *fixed points* for the theory, since they represent points at which the trajectories *terminate*.

We can expect such fixed points to be extremely rare features of the space.[111] But if we do manage to discover a fixed point, we can study the behaviour of the couplings in the region around it where the β-functions change smoothly from zero, since this region provides crucial information about the high-energy properties of the theory. If the fixed point is such that the β-functions drive the couplings *up* towards the fixed point as t increases from below, but also *down* into the fixed point as t increases from above – so that the β-functions are positive below the fixed point as negative above it, as t increases – then we say that the fixed point is 'UV-stable'.[112] The reason for this is that in such a case, the couplings are continually attracted towards the fixed point as the energy increases and thus never escape to diverge at some energy. If, on the other hand, the β-functions exhibit the opposite behaviour with respect to increasing t, then we say that the fixed point is 'UV-unstable'. Putting everything together, then, we may say that a sufficient condition for a theory to be well-defined at all energies is if all its couplings are driven

towards to fixed point is 'one way', or the 'simplest way', to secure consistency at high energies, I do not know what the alternatives are.

[110]Note that couplings that 'start off' at zero will in general become non-zero as the energy increases, unless they are forbidden by symmetries.

[111]Huggett and Weingard [1995], p181.

[112]See e.g. Maggiore [2005], p237.

into a UV-stable fixed point as $t \to \infty$. Theories that meet this criterion are said to be 'asymptotically safe'.[113] Asymptotically safe theories may therefore be said to produce 'a self-consistent description of a certain set of physical phenomena which is valid to arbitrarily high energy scales and does not need to refer to anything else outside of it. In this case the theory is said to be "fundamental"'.[114] Huggett and Weingard refer to such theories as 'continuum compatible'.[115]

This alternative method of addressing questions of fundamentality does not – or at least not obviously – attach any significance to the *mass dimensions* associated with those couplings. But these, as we saw, are crucial to fundamentality considerations in the perturbative context. What therefore needs to be addressed at this point is what the relationship is between the 'power counting'-based approach to fundamentality presented until this coda, and the approach currently under consideration. What, then, *is* the relationship between asymptotic safety and the perturbative concepts of renormalizability and nonrenormalizability – and indeed between asymptotic safety and the use of perturbation theory at all?

Let us first of all consider the relationship between renormalizability and asymptotic safety. The most important point to note in this connection is that it is now regarded as having been conclusively shown that renormalizable theories may lack UV-stable fixed points, and thus to fail to be asymptotically safe.[116] Indeed – although there are caveats of various sorts – it now seems that both standard QED and the standard electroweak theory (and indeed the simple theory ϕ^4 that we looked at as our example) fail to possess a UV stable fixed point, and thus not to be asymptotically safe. We may therefore say that renormalizability is not *sufficient* for asymptotic safety, and thus that it is possible that even a renormalizable theory may break down somewhere in energy space. However, given that my primary motivation for discussing QFT was to investigate whether it supports an anti-fundamentalist ontology, and given that that discussion was framed in terms of the properties of

[113] I am glossing over some subtleties here, since it turns out that this condition needs to be satisfied by only a subset of the couplings; see Percacci [2008] p5-6 for more detail.
[114] Percacci [2008], p5.
[115] Huggett and Weingard [1995], p179.
[116] See Huggett [2002], p264.

nonrenormalizable theories, the more pertinent question for present purposes is whether a theory's power-counting nonrenormalizability may still be taken to be sufficient for its breakdown at some finite energy. But here things are rather less clear in the nonrenormalizable case, and the best answer to this question, at least for now, seems to be: *probably not.* I will now try to explain why this is.

What we *do* know for certain is that there are models of QFT *in two and three dimensions* that are nonrenormalizable and yet possess a UV-stable fixed point. What is not yet known is whether there are any realistic examples of such theories.[117] It is now acknowledged as likely, however, that it is only technical obstacles that stand in the way of us ascertaining that there could exist such a theory.[118] Indeed, there are now real hopes that there may be a fundamental quantum field-theoretic description of gravity – despite it being in principle nonrenormalizable – and looking for such a theory now represents an active area of research.[119]

These technical obstacles confronting the study of continuum-compatible yet nonrenormalizable theories are nonetheless fairly formidable. One of the fundamental challenges facing any such analysis is that the use of perturbative techniques is ruled out. The reason for this is that if such techniques are to be used to study the high-energy behaviour of a theory, and thus its behaviour around its fixed points (should it have any), then it must have couplings that remain sufficiently small in the region around the fixed points. It turns out that this implies that the only fixed point we can study through such techniques is the so-called 'Gaussian' fixed point, the characteristic feature of which is that all of the theory's couplings vanish at it, and thus are guaranteed to be small in the region around it. Theories whose UV-stable fixed point is the Gaussian are known as *asymptotically free*; QCD (again, as standardly written down) is a famous real example of such a theory. However, it is easy to show (though I shall not do so here) that asymptotic freedom is equivalent

[117]See Percacci [2009].

[118]Percacci [2008].

[119]A theory of gravity is necessarily nonrenormalizable since the requirement of general covariance brings in its wake a high number of spatial derivatives. Since the ∂_x operator increases the mass dimension of a term containing it by 1 unit, these terms need to multipled by couplings with highly negative mass dimensions in order to keep the action dimensionless.

to asymptotic safety *plus power-counting renormalizability*: that is, those theories whose fixed point is the Gaussian *must contain only renormalizable interactions*.[120] Thus, while it may turn out that there do exist realistic examples of asymptotically safe *nonrenormalizable* theories, such theories cannot be investigated through perturbative techniques, and this compounds the already formidable challenges facing any attempt to find them.[121] So while it does seem intuitively clear that 'randomly chosen' QFTs – including, of course, EFTs – will be very *unlikely* to exhibit asymptotic safety, it is not clear that we can say a great deal more than that about such theories at present.[122]

What has thus emerged in this coda is that the discussion I offered in the main body of this chapter about whether QFT supports an anti-fundamentalist ontology was not only inconclusive insofar as the matter was left *underdetermined* – though that was, I argued, non-trivial in itself – but that it also approached the matter in too restrictive a manner. This is on account of the fact that it took nonrenormalizability to be sufficient for denying a theory's fundamentality. In light of the above considerations, however, the idea that effective theories necessarily 'contain the seeds of their own destruction' in the way that their perturbative expansion seems to suggest they do can no longer be taken for granted. What it seems we *can* say is that, were amenability to perturbative treatment regarded as an *essential* element of any acceptable QFT, then our argument would still go through as before; it would remain the case that an EFT could not qualify as a fundamental theory as it could not be asymptotically free, hence could not be treated by means of these techniques. However, the path integral approach to computing the S-matrix – though not, it turns out, that based upon canonical quantization – furnishes us with a definition of field theory that is in principle non-perturbative; that *we* can only systematically get a handle on QFT by means of perturbative techniques should be regarded as our problem and not Nature's.[123] I will therefore close

[120]Percacci [2009]; Percacci [ms].

[121]See e.g. Weinberg [1997a], p11; Rothstein [2004], p64.

[122]Thanks to Nazim Bouatta at the University of Cambridge for an illuminating discussion on these matters.

[123]The basic difference between the two formalisms in this connection is that the canonical quantization route contains operators in exponentials. Since the exponential of an operator is defined by its Taylor expansion, expansion techniques must always be used when making calculations within this formalism (see Maggiore

by saying that the argument I adduced in the main body of this chapter as to whether QFT has anti-fundamentalist implications is conditional upon the assumption that QFT was to be analyzed perturbatively, and stands or falls with that assumption. And that in turn invites a final conclusion.

- *Internal arguments against fundamentality may be sensitive not only to the physical principles underpinning the theory through which fundamentality is denied, but also to the mathematical framework that is adoped to present, and compute with, that theory.*

Since the metaphysical structure of the world is presumably invariant under changes in the computational approach that we take to our physics, and impervious to our limitations when it comes our ability to perform the requisite calculations, this shows that in pursuing the internal approach to answering fundamentality questions we must refrain from uncritically projecting figments of our mathematical representations and pragmatic limitations into our metaphysical conclusions. Nevertheless, it remains that whether one ought to believe in a fundamental level or not will turn on questions of what principles, what formalisms, and what methods we elect to adopt in physics. Questions of fundamentality are thus shown to be continuous with all the other questions we might ask when interpreting physical theories, and no less difficult to answer. What is certain, however, is that the idea that those questions can be fruitfully approached by *a priori* speculation simply cannot be taken to stand up.

[2005], p219). Nonetheless, one *can* find in the literature claims to the effect that QFT is 'intrinsically perturbative': see, e.g., Anselmi [2003] and Valente [2011], each of whom cites Dyson's view that QED 'is in its nature a perturbation theory'. But given the growing – and increasingly successful – use of non-perturbative techniques in extant QFT practice, it is entirely unclear to me what convincing defence could be given of this position.

Part II

Arguing for the Fundamentality of Structure

Chapter 7

The Limits of the Internal Approach: Implications and Interpretations

7.1 Taking Stock

The previous two chapters demonstrated that it is indeed possible to mount naturalistic arguments against the existence of a fundamental level by utilizing the internal approach. In Chapter 5, I argued that we can use the principles underlying Analytic S-matrix theory to deny that there exists a fundamental level of objects, in the sense of a set of objects that lack proper parts and that are sufficient to compose everything else. In Chapter 6, I argued that the basic principles of quantum field theory, at least when viewed through the lens of perturbation theory, imply the existence of an infinitely descending ladder of laws unless we stipulate that one of those laws satisfies an additional principle for which we could find no physical motivation that did not presuppose fundamentality. And while I claimed that that in itself means that perturbative QFT ultimately *underdetermines* whether there is a fundamental level or not, I argued that it was non-trivial that one could find a set of principles that even permit such an anti-fundamentalist interpretation.

Given that, as pointed out in Chapter 1, it is a standard assumption in metaphysics that there exists a fundamental level to reality that physics will one day describe, this demonstration that we can use physics to deny the existence of a fundamental level represents a significant achievement. It means, among other things, that the widespread belief in the existence of a fundamental level is not one that the naturalized metaphysician need share. Note, however, that I did not conclude from either of the case studies I looked at that the world *does in fact* lack a fundamental level. We obviously cannot draw any such conclusions from the S-matrix case, since this theory has long been relegated to the dustbin of scientific history. And while QFT remains our most fundamental testable physical framework to date, even the qualified conclusion regarding anti-fundamentalism that it permits was drawn against the backdrop of perturbative assumptions and thus may have to be abandoned when those assumptions are supplanted. As such, Chapter 6 remains at best a first stab at discussing the relationship between QFT and anti-fundamentalism, and one that awaits a more exact treatment (elusive though that may be at present). However, the fact that I did not secure that this world does in fact lack a fundamental level is not actually of great relevance for present purposes. After all, the task that I set myself was that of investigating whether it is possible to deny the existence of a fundamental level through internal means, and the case studies amply demonstrated that it is. They showed how fundamentality questions can be continuous with other questions in the interpretation of physical theories, and that we can have as much right to draw conclusions supportive of anti-fundamentalism from them as we have to draw other conclusions that transcend their empirical content.

While the fact that we can use the internal approach to mount defensible arguments against the existence of a fundamental level should certainly be regarded as significant, it must nevertheless be acknowledged that this approach brings with it a profound limitation on the kind of infinitely-descending worldviews that can be justified through its means. This was something that was flagged up at the end of both of the case studies, and the reason is that the use of the internal approach to denying to existence of a fundamental level inevitably commits us to certain *principles* which must at least be *treated* as fundamental within the context of that approach, since they constitute the basic assumptions from which our anti-fundamentalist conclusions are derived. Thus while we may be able to deny the existence of fundamental particles through S-matrix theory, those particles must obey the principles of S-matrix theory at each and every mereological level that those principles imply. Likewise, while we may have grounds to deny the existence of a fundamental law lying at the bottom rung of the ladder of laws that the principles of QFT imply, every law in that ladder must accord with those principles (whether it has a last rung or not). It is in this sense that the internal approach can only ever present us with a sort of 'half-way house' form of anti-fundamentalism in which the principles governing the physics of a world must stay the same even as its levels unfurl without end.

But this clearly represents a non-trivial limitation on the sorts of anti-fundamentalist conclusions that the approach can be used to draw. It implies, for example, that we cannot use the approach to argue for a picture in which the world unfolds *ad infinitum* into regimes as dynamically and ontologically opposed as those of classical and quantum physics. But since it was part of Bohm's view of the 'qualitative infinity of nature', mentioned in Chapter 4, Section 1, that worlds could coherently – even profitably – be regarded as decomposing into successively deterministic and indeterministic layers, we will never use the internal approach to argue for the existence of the sort of worlds that Bohm envisaged.[1] More generally, we cannot use the internal approach to argue for the existence of worlds that unfold endlessly into regimes governed by ever more fundamental physical principles. But since Popper held that the world that is studied by science should be taken to be describable by theories of 'greater and greater depth' – or, as Weinberg has characterized his view, in terms of a chain of 'more and more fundamental principles' – this is not a view that we can argue for by means of the approach either.[2] However, I think we have to agree there is nothing *incoherent* in the concepts of the sort of worlds that Bohm and Popper envisaged. We ought, I think, to acknowledge that they constitute genuine possibilities, and that it is even possible that *this* world is just such a world. But since, as was argued in Chapter 4, the only acceptably naturalistic way we could hope to deny the existence of a fundamental level is through the internal approach, I believe that there is no avoiding the conclusion that, although *it is possible* that our world is one such world, *whether it is in fact that way* is simply something that must forever escape our grasp. And that is something that I think we must just accept.

I hold, therefore, that since the internal approach is the only acceptable route

[1]Bohm [1957], Chapter 4, Section 6.
[2]Popper [1972]; Weinberg [1993], p184.

we can take toward denying the existence of a fundamental level, and in spite of the fact that the approach *can* generate non-trivial anti-fundamentalist conclusions, there is a substantial limitation on the sort of fundamentality that we will ever be in a position to deny. Though we can deny the existence of fundamental *level* – in the sense of a set of particles or a fundamental law – *we cannot argue against the existence of fundamental physical principles*. That is a straightforward but nonetheless profound consequence of the position taken so far. But it is also a consequence that suggests we now ask a new fundamentality question, and one whose answer will help us to more fully comprehend the conclusions that have so far been reached. That question is whether the physical principles that must be *treated* as fundamental in the context of the internal approach ought to be regarded as somehow more *ontologically* fundamental than anything else that we have been discussing so far – more fundamental, that is, than either laws or particles, including even any putatively fundamental examples of each. This, after all, is a natural question to ask once the above limitation is acknowledged, since exactly *how* limited the internal approach is as a means of denying fundamentality is going to depend on whether we ought to have regarded principles, and not particles or laws, as that which is somehow 'truly' fundamental all along.

Before we can make sense of this question of whether we ought to regard principles as that which is ontologically fundamental, however, it seems we face a number of challenges. Three such challenges spring immediately to mind. First of all, one might argue that we have not yet been given a sufficiently good reason for even asking the question, since all that the above considerations have shown is that it is a product of the *dialectical method* represented by the internal approach that one cannot deny the existence of fundamental principles. It follows that the most that has been shown so far is that such principles have to be regarded as *dialectically* or *methodologically* fundamental in the context of arguments against the existence of a fundamental level. By contrast, when we talk about the fundamental level, we mean to denote something that is *ontologically* fundamental (be it occupied by particles, laws or anything else); but, of course, the fact that something is methodologically fundamental need not imply, at least not without further argument, that it ought to be regarded as ontologically fundamental too. Secondly, even if the above challenge can be met so that the question *can* be regarded as well-motivated, it is not clear that it would even *make any sense* to ask it, since the very concept of a 'principle' connotes something that is propositional in character. But propositions seem to belong to the realm of concepts and representation, and thus to be in an altogether different ontological ball park from such physically efficacious entities as particles or laws. And thirdly, even if we *could* construe principles in appropriately ontological terms, it is far from obvious at this point how we could construe their conjectured fundamentality. The reason is we have so far conceived of the fundamentality of objects or laws in terms of their inhabiting the fundamental level, but it is clear by now that we cannot construe these principles as being fundamental in that way. After all, what was shown above is that we can use these very principles to *deny* the existence of a fundamental level; we clearly cannot then go on to say by way of articulating their fundamentality that those same principles inhabit it.

There are therefore considerable challenges facing any attempt to orientate physical principles within the concept of ontological fundamentality. But they are challenges it will nonetheless pay to face up to. Regarding the first of the above challenges, not leaving the discussion of the fundamentality of principles at the methodological level surely *must* be regarded as well-motivated. For one thing, and as already pointed out, only once we have made at least some attempt to understand where principles sit on the fundamentality hierarchy will we be in a position to fully understand the limitations of the internal approach to denying fundamentality. But for another thing, the idea that it is principles – especially symmetry principles – that are somehow to be regarded as that which is truly fundamental in particle physics reflects a view that seems to be increasingly held by particle physicists themselves. Largely due to the success of the gauge principle in (to a great extent) determining both the laws of the Standard Model and the associated fundamental particles, one can now find physicists explicitly alluding to the idea that it is 'the *principles* of elementary particle physics [that] are fundamental to all of nature', not particles, forces or laws.[3] By taking the time to make sense of our question regarding the fundamentality of principles, we will be assisting in further articulating how certain particle physicists who have reflected on this issue themselves conceive of fundamentality – a project that is, I take it, of

[3]Weinberg [1993], p44; see also, e.g., Salam [1979], p528.

some philosophical interest in itself.

Given that doing so has clear motivations, what I want to do now is take some steps toward understanding what it might mean to call a physical principle 'ontologically fundamental', and then begin to consider whether we in fact ought to regard principles in that way. I will therefore now try to address the second and third challenges that were raised a moment ago. As will be seen presently, doing so will reveal rich connections between Part 1 of this thesis and the central tenets of *ontic structural realism.*

7.2 Understanding the Fundamentality of Principles: The Touchstone to Ontic Structuralism

The second of the challenges laid out above concerned the fact that it is not at all obvious at a first pass how we could regard physical principles as being fundamental in anything like the way that we take, say, elementary particles to be – the reason being that principles seem to belong more to the realm of representation than to that of the physical. But there is an obvious and natural way to try to resolve this quandary, and that is to consider whatever it is that these principles *refer to* as the proper subject of the fundamentality attribution. Now, I will not here be hubristic enough to attempt to anticipate every sort of physical principle that might be proposed in fundamental physics. Rather, I will try to provide a characterization of principles that is representative of the principles we have looked at so far at least, and try to ascertain a candidate for their referents.

Looking back again the case studies, and thus considering once again the principles underpinning S-matrix theory and QFT, we find principles such as the principle of superposition, the unitarity principle and the principle of Lorentz invariance; I have also just mentioned the gauge principle as another example that has motivated claims regarding the fundamentality of principles. But all of these principles may be naturally construed as *constraints upon the dynamics*. The superposition principle implies, for example, that the dynamics must be linear; the unitarity principle amounts to the condition that the dynamics must representable by a unitary operator; the principle of Lorentz invariance obviously implies that the dynamics must be invariant under the transformations of the (inhomogeneous) Lorentz group, and the gauge principle demands that they must be invariant under a group of local gauge transformations (though, of course, which such group is not pinned down by this principle). Now, clearly each of these constraints refers to the *mathematical form* of the dynamics, and as such it seems that we can characterize each of these key principles as principles that impose a *constraint upon the structure* of the dynamics. But if we are looking for an ontological correlate of these principles, then it seems that the most natural candidate is just the *structure of the dynamics*. It therefore seems right to say that, in these (important) cases at least, principles refer to *dynamical structures*. It is therefore the latter that I propose we take to be the proper subject of the fundamentality claim made on behalf of principles.

If that is the case, then the question of whether we should regard these principles as in some sense ontologically fundamental becomes the question of whether we should regard the relevant dynamical structures as ontologically fundamental. But it is, of course, precisely that question that *ontic structural realists* are centrally concerned with – and take to be answered in the affirmative. The task of considering whether we ought to regard the physical principles that the internal approach treats as methodologically fundamental as also being *ontologically* fundamental thus translates into the task of adjudicating on whether the distinctive thesis of ontic structuralism can be rigorously shown to hold up. *It is therefore precisely this question that I will be concerned with in Part 2 of this essay.*

Before I attend to that question, however, it will be helpful to get clearer on exactly what is meant by calling structures 'ontologically fundamental', and thus to address the third of the challenges that were outlined above. We have seen that we cannot conceive of the fundamentality of principles — where I now take the latter to be an elliptic way of expressing the fundamentality of dynamical *structures* – in terms of their occupation of a fundamental level. The question is then how we *should* understand any claim that they are ontologically fundamental. The key here, however, is to realize that when we talk about a levels hierarchy – that is, a hierarchy in which some level of objects or laws is more fundamental than another – we make *intra*-categorical fundamentality claims. Thus, to say that there exists a fundamental level of objects, for example, is to say that there is a set of objects that are privileged with respect to all the other entities *within the category of objects*. Likewise, to say that there exists a fundamental law is to say that there is a law that is privileged *with respect to all other laws*. However, given that what originally motivated asking the question of whether principles – and hence structures – ought to be regarded as ontologically fundamental was the fact that we seemed to be able to argue against the existence of fundamental particles or laws, but not of fundamental principles, it seems clear that what we are concerned with investigating now is an *inter*-categorical claim. In particular, we are concerned with a claim about the relative fundamentality of the *category* of dynamical structures compared to the *category* of objects, or the *category* of laws. Thus, to be clear, when the radical ontic structuralists Ladyman and Ross write that 'structure is more ontologically fundamental than objects', for example, this should not be interpreted as a claim about structure lying on a lower *level* than some level composed of objects. Rather, we should understand it as a claim about the relative fundamentality of those *categories*.[4] It follows that we can expect the answer to this question of whether structure is a fundamental category to be neutral on the question of whether or not there exists a fundamental level.[5]

In the second part of this thesis, then, I want to assess whether the flagship claim of ontic structuralism – that the category of dynamical structure is an ontologically fundamental category – can be rigorously defended. In doing so, it will be helpful to be clear that structuralists accuse most contemporary metaphysics of being overly 'object-oriented' as a result of its disengagement from physics, and that consequently it has failed to recognize that it is *structure* that comprises either *the*, or at least *a*, fundamental category.[6] Though I take it that the view has a certain amount of *intuitive* force, I will not attempt to argue

[4]Ladyman and Ross [2007], p145. The precise meaning of 'radical' versus 'moderate' structuralist terminology will be clarified in the next section.

[5]Indeed, James Ladyman has pressed, in a number of recent talks, the compatability of ontic structuralism with both the existence and the absence of a fundamental level.

[6]See, e.g., French [2006] for a statement of the 'object-oriented' accusation. (As we will see, the use of the definite and the indefinite article here separates the radical and moderate views, respectively.)

for that accusation here.[7] But it is clear that if structuralists *are* correct in that view then examining whether structure should be regarded as a fundamental category will represent another attempt to use physics to scrutinize and challenge a received view in metaphysics regarding fundamentality. Indeed, when radical ontic structuralists state that 'relational structure is ontologically fundamental, and individual objects are not', they are clearly making an explicitly anti-fundamentalist claim on behalf of particles.[8] However, I have argued that it is a different such claim than that which was the topic of Part 1. While there I considered whether one might deny that there exist fundamental particles in the sense of a fundamental *level* comprised of them, ontic structuralists attempt to deny that particles are fundamental in the sense of comprising a fundamental *category*. Although this latter claim is clearly a different anti-fundamentalist claim on behalf of particles, it is an anti-fundamentalist claim nonetheless, and one that will be our principal focus of attention from this point on. The discussion of Part 2 of this thesis may thus be seen as complementary to that of Part 1, as well as interpretative of it.

Before I begin contemplating the ontological fundamentality of structures, however, it will help to make some clarifications at the outset. First of all, while structuralists have, in places, come close to claiming that the structures they are concerned with are more fundamental than laws, laws are more often seen in the structuralist literature as themselves representative of dynamical structures.[9] Furthermore, the vast majority of the ontic structuralist literature, and the most characteristic examples of it, are concerned with structure's ontological standing relative to *objects*. On account of that, I will focus

[7]Alas, detailed defence of the idea that contemporary metaphysics typically does regard objects, and only objects, as fundamental appears to be lacking in the current structuralist literature. While I take it that it is reasonably *intuitive* that much familiar contemporary metaphysics does privilege the category of objects, one clearly cannot be too facetious with this claim since, for example, Armstrong holds that states of affairs constitute the fundamental ontological category (Armstrong [1997]), and Russell and Wittgenstein took facts to be fundamental (see e.g. Russell [1918]; Wittgenstein [1961]).

[8]Ladyman and Ross *op. cit.*, p148.

[9]For example, Cei and French [forthcoming] discuss whether we can view symmetries as constraints on laws (as opposed to merely features of them), and thus as in some sense 'prior' to laws. On the other hand, Worrall [1989] presents laws as paradigmatic examples of structures.

exclusively on the relationship of structures to objects in particular, and thus examine whether we may say that the category of structure is more ontologically fundamental than that of objects. Secondly, I will assume throughout that there are only two categories in play. If we can show that structure is prior to objects and not *vice versa*, then structure will be *the* most fundamental category (and *mutatis mutandis* with 'objects' replaced by 'structure'). Should it turn out that each is prior to the other, then they will, of course, be equally fundamental. It seems correct to me to say in this context that if the latter were to be the case, then it is better to regard *each* as a fundamental category rather than *neither* as a fundamental category, since both (and by hypothesis no others) are required to build the world. If we find that the priority relations are indeed reciprocated then that is what I shall say, though it is, of course, somewhat a semantic matter which stance we choose to take on this. In any case, what is most important is the structure of the priority relations, and not whether the event of their reciprocation is or is not taken to undermine fundamentality attributions. Finally, I note that I will not provide much in the way of a survey of the literature on ontic structuralism here. Rather, I will give merely the briefest presentation of what I take its core claims to be, before moving on to assess them.

With those clarifications in place, let me therefore introduce ontic structuralism.

Chapter 8

Structure as a Fundamental Category 1: Structuralism as a Supervenience Thesis

8.1 Introducing Ontic Structuralism

I have already pointed out that ontic structuralists hold that structure should be regarded as a fundamental ontological category. We may, in fact, take this position to be largely definitive of ontic structural realism. In his survey article for the *Stanford Encyclopedia of Philosophy*, for example, Ladyman introduces ontic structuralism on its 'broadest construal' as 'any form of structural realism based on an ontological or metaphysical thesis that inflates the ontological priority of structure and relations', suggesting that it is a claim concerning the priority status of structures relative to objects that we may take to be most characteristic of structuralist metaphysics.[1] Indeed, statements attesting to this view abound in the structuralist literature.[2]

[1]Ladyman [2007], Section 4.

[2]French, for example, 'take[s] as a core feature of [ontic structuralism] the claim that the putative "objects" are dependent in some manner upon the relevant relations' ([2010], p104); Ladyman and Ross state that 'ontic structural realism is the view that the world has an objective modal structure that is ontologically fundamental' ([2007], p130); Wolff states that 'ontic structural realists hold that structure is all there is, or at least all there is fundamentally' (Wolff [2011], p1); and so on.

As also mentioned in the last chapter, ontic structuralists – hereafter, simply 'structuralists' – typically additionally hold that mainstream metaphysics is highly 'object-orientated', since it presents the category of objects as though it has a uniquely privileged ontological status (and mistakenly so in their view).[3] Whatever exactly the evidence is for this claim, if it *is* true then given that structuralism is about 'inflating' the priority of structures relative to objects, it follows that there are *two* distinct forms of structuralism that may be discerned.[4] This is because a position that fell short of imparting a *superior* status to structure, but simply raised it to the status of objects, would qualify as just as legitimate a form of structuralism on Ladyman's construal as one that held the stronger 'superiority' view. One therefore finds two positions, of differing strengths, being defended in the structuralist literature. On the one hand, there is the 'radical' position in which structures enjoy an unreciprocated, one-way priority over objects. This more revisionary of theses has been associated primarily with French and Ladyman – both of whom even go so far as to recommend the outright elimination of objects from our ontology as a result of their analyses.[5] There is, on the other hand, a less radical and so-called *moderate* position, which is at present associated primarily with Esfeld and Lam. According to this position, the two categories should be taken to be 'ontologically on a par' with one another, so that – to the extent that is makes sense to speak of 'priority' at all – these priority relations are reciprocated.[6] Thus while the radical view takes it that 'relational

[3]Though there is also a purely epistemic variant of structuralism (see, for example, Section 3 of Ladyman [2007]), as this is a thesis in ontology I will focus on the ontic version alone.

[4]As noted in the last chapter, I will not attempt to defend or rebut this claim regarding the 'object-oriented' nature of contemporary metaphysics here. All that is needed for present purposes is the perception that structure has been neglected as a category in comparison to objects; in that case, either of the positions about to be adduced will imply a suitable 'inflation' of the priority of structures.

[5]See, e.g., Ladyman [1998], French and Ladyman [2003a]. For statements on how the secondary status of objects prompts their elimination, see French [2010]; Ladyman and Ross [2007], Chapter 3. The issue of elimination will be returned to at the end of this chapter.

[6]See e.g. Esfeld and Lam [2008], [2009]; Esfeld [2004]. This position has also been associated with Eddington (see e.g. his [1939] pp230-231 and French [2003]). (It may well be helpful for me to recall at this point my remarks in Chapter 1, Section 2 that while it is awkward to use the word 'priority' when symmetrical relations are permitted, I nonetheless use the currently accepted terminology. And once again, I do not take x's being prior to y to be sufficient for x's being *more fundamental than* y;

structure is more ontologically fundamental than objects', according to the moderate view objects and structure 'are both on the same footing, belonging both to the ontological ground floor'.[7] As such, structure and objects are to be regarded as *equally* fundamental categories. Since – as mentioned in the last chapter – I (i) assume that these are the only categories in play, and (ii) take it that, in a case of reciprocated priority between categories, it is better to say that *both* are fundamental (as opposed to saying that *neither* are), it follows that *both* structure and objects comprise fundamental categories according to the moderate view.

Let me therefore call the claim that structures are prior to objects the 'core claim' of structuralism, since this is shared by both of the positions; the positions may then be differentiated from each other in terms of whether they assert or deny that objects are likewise prior to structures, and thus over whether objects comprise a fundamental category in addition to that of structure.[8] Since my purpose in Part 2 of this essay is to adjudicate on whether there is any case to be made as to whether structure is an ontologically fundamental category, I will therefore assess in what follows whether either of these positions can be defended. However, in spite of the fact that there is already a large body of literature dedicated to defending each view – something that certainly cannot be said regarding the existence of a fundamental level – I think that more than a mere survey of the extant literature is required in order to assess whether structure is indeed more fundamental. The reasons for this are twofold. First of all, there is a case to be made that the structuralist understanding of priority has in many cases yet to be made fully precise, and different and inequivalent characterizations of priority are often used interchangeably.[9] As a result, Hawley has complained of radical structuralism that 'the ways in which structures are somehow prior to objects', and thus

I take x to be more fundamental only if, in addition, y is not prior to x; failing that, x will be *as fundamental as* y.

[7]Esfeld and Lam [2008], p5.

[8]Thus note that the 'moderate' position is not – at least not on my rendering – to be understood as a *logically* weaker claim than that made by that radical view. That is, the radical view should not be thought of as *entailing* the moderate position. Rather, the two are incompatible with one another since each makes an assertion regarding the priority of objects over structures that the other explicitly denies.

[9]For example, Ladyman and Ross [2007] (see, e.g., p130) and Kantorovich [2003] both slide between determination- and dependence-based characterizations of priority. (Kantorovich's paper will be discussed below.)

what it means for structure to be ontologically fundamental and for objects by contrast to fail to be, are as it stands 'deeply unclear'.[10] When ascertaining whether one or other structuralist thesis can be defended, we will therefore have to try to be clearer on the nature of the priority relation involved than has often been the case so far.

A further reason why it is problematic to simply assess the extant arguments for one or other of the structuralist positions is that they are arguably just *inconclusive* as they stand (and that is so even putting the ambiguity over priority aside). Thus on the one hand, while radical structuralists have in many cases presented intuitively compelling grounds for suspecting that structures are prior to objects, they generally do not consider the converse question of whether there is also a case to be made for the reciprocated priority of objects over structures. As such, their arguments do not yet establish that it is the radical position, and not the moderate position, that is best recommended to us by physics. On the other hand, while moderate structuralists *have* explicitly attempted to make a case for the reciprocated priority of objects over structures, that case is arguably just not compelling as it stands, and thus radical structuralists have not bothered to spill much ink on it. The extant major argument for the moderate position hinges on the idea that 'for relations to be instantiated, there has to be something that instantiates them, that is, that stands in the relations', and that these relata can only be construed as objects.[11] They call this objection that structures cannot be conceived of as instantiated without objects the 'intelligibility objection' to radical structuralism.[12] But the nature of this requirement that structures place on objects is not spelled out, and, in any case, it seems perfectly consistent to construe the requirement in terms of relations necessitating the existence of relata, but only as an *ontologically secondary* phenomenon.[13]

[10]Hawley [2008].

[11]Esfeld and Lam [2008], p5; see also their [2009].

[12]Esfeld and Lam [2008].

[13]It is, after all, presumably this circumstance that French and Ladyman have in mind when they invoke Cassirer's conception of objects as the 'points of intersection' of relations, and thus *implied by* those relations but only as a derivative phenomenon (see French and Ladyman [2003a]; see also Ladyman and Ross [2007], Section 3.5, Point 1). (Note that I do not mean to communicate that this talk is sufficiently perspicuous as it stands, only that it is a coherent idea that Esfeld and Lam fail to engage with.) It may also be pointed out that Paul [forthcoming] and Mertz [1996] defend the coherence of the view that relations can have other relata as

Thus, insofar as we want to go further than establishing structuralism's core claim and hence adjudicate between its two rival positions, we need to explicitly consider possible justifications for the priority of objects that are more compelling than have been offered so far.

I therefore suggest that, rather than simply review the extant literature, we try to view the issue of priority in structuralism with fresh eyes. However, to put ourselves in a position to assess any claim regarding the relative priority of structures and objects there is some obvious preliminary groundwork that must be done first. In particular, we must first of all (i) identify what it is that we mean by 'structures', (ii) identify what it is that we mean by 'objects', and (iii) identify an appropriate priority relation that we take to connect these two categories. Regarding the first point, it should be noted right away that how exactly structuralists ought to define structure in general has proved to be a controversial matter. In fact, even providing a loose characterization of structure has proved to be somewhat problematic: while structuralists often informally characterize structures as 'nexuses of relations', this is arguably insufficiently general to capture the all-important notion of group structure (since the latter is better expressed as 'pattern of interrelatedness of relations').[14] But rather than offer any very precise *general* definition of structure and establish their claims with respect to that, structuralists are usually content to work with a rough-and ready characterization of structure and establish their claims with reference to specific examples of (what they take to be) paradigmatic structures in physics. Likewise, while there is no general definition of 'object' to be found, to my knowledge, in the structuralist literature, again paradigmatic examples of objects are either chosen or entailed by an antecedent choice of structure, and the priority claims then argued for. Structuralists thus tend to establish their claims not with respect to some general characterization of structures and objects, but rather on a case-by-case basis. (Indeed, it is difficult to imagine how a naturalistic approach to the structuralist question could proceed in any other way.) Therefore, in order to assess structuralism's priority claims, I will choose among *their* examples and see if those claims may be argued to go through in those cases.

their relations, thus challenging Esfeld and Law's 'intelligibility objection' even more directly.

[14]See e.g. Ladyman and Ross [2007], p138; French [2012], p10.

To do that, the third of the above points must be addressed, and thus an appropriate priority relation be chosen. What is immediately clear in this regard is that any such relation, if it is to be appropriate to structuralism, must be capable of relating entities of different categories (since it must be the case that it is able to relate both structures and objects). Thus the priority relations that we focused on in Chapters 5 and 6 – namely, those of parthood and nomic derivation – will be unsuitable here, since the latter obviously obtains only between laws and the former – at least standardly – only between objects. Something more general is therefore required. Looking again Chapter 2, where a number of priority relations are described, two obvious candidates stand out. Recall that it was stated there that we may split priority relations into two broad categories, which I called 'relations of determination' and 'relations of dependence'. As was also mentioned there, while the relation of nomic derivation was taken to be a member of the first category, supervenience was taken to be a more general sort of determination relation since it could apply to entities of different categories (see Chapter 2, Section 1). Likewise, although parthood was presented as a type of dependence relation, it was noted that there are much more general approaches to dependence that may be appealed to in more general cases.[15]

It therefore seems that, *prima facie* at least, either of supervenience or some suitably general notion of dependence may be taken to be candidate relations for expressing priority in structuralism. Indeed, structuralists have, at different points, utilized both.[16] In order to get things going, I suggest that we pick one and run with it, and I propose that we start with *supervenience*.[17] In addition to the reasons just adduced as to why this is a suitable candidate, this relation is arguably a natural choice given structuralism's subversive intentions. After all, since supervenience is so commonly invoked to express fundamentality

[15]Much more on Fine's notion of dependence – one proposal for a general dependence relation – may be found in Chapter 6.

[16]Examples of uses of each will be presented *anon.*

[17]To recap from Chapter 2, by saying that A supervenes on B I will mean that things that are B-indiscernible are A-indiscernible. This rendering is equivalent to saying that B-properties *determine* (or 'settle') A-properties, since it entails that sameness with respect to B-properties implies sameness with respect to A-properties. In thinking about whether objects supervene on structures, then, we will be thinking about whether the objects could be different without the structures somehow being different.

theses in contemporary metaphysics, if structuralists can show that objects supervene on structures then they will be in a very strong position to claim that the overly 'object-oriented' view that they detect in that metaphysics is a deeply misguided one. That seems like motivation enough to be getting along with for now. (Whether it is in fact the best relation for structuralism is something that it will be easier to address in due course.)

What is now needed to proceed further is a choice of structure and objects. Since the focus in this thesis lies squarely on particle physics, I suggest that we choose *group structure* as the example of structure that we will use to get started.[18] The reasons for this will be obvious. The importance of symmetries in contemporary particle physics simply cannot be overstated. It is indeed the unrivalled methodological centrality of symmetry principles in contemporary theories that has led particle physicists to hazard that it is symmetries that are 'fundamental to all of nature', as was mentioned in the last chapter. Partly as a result of this, the notion of group structure has enjoyed a centre-stage position in structuralism, though one can find symmetries at the forefront of even the earliest of structuralist works.[19] Still, talk of group structure in general is a little too abstract to be helpful in this context and, since we are going on a case-by-case basis, what we need is a concrete example of group structure that is relevant to particle physics. I therefore propose that the global SU(3) flavour symmetry – the structure underpinning the 'Eightfold Way' classification of hadrons by Gell-Mann and Ne'eman – represents a good place to start.

This specific example is again recommended to us for a number of reasons. For one thing, the SU(3) flavour symmetry is an undisputed advocate for the power of symmetry considerations in particle physics. Almost half a century since its inception, particle physicists have described it as 'probably the most successful and fruitful idea for the systemization of elementary particles', and

[18]This structure is to be understood as somehow physically, and not purely mathematically, interpreted; the question of how this type of structure can be considered as such will be considered in the next chapter.

[19]See, e.g., Cassirer [1956]; Eddington [1939]. Thus, while Roberts has dubbed ontic structuralism that focuses on symmetry 'group structural realism' (see Roberts [2011], French points out that 'group structure is so bound up with ontic structural realism in the works of myself and Ladyman that one may wonder whether the view really deserves a separate designation' (French [2012], p13).

one that has served as a prototype for particle physics in the time since.[20] Redhead and Debs have likewise described it as 'perhaps the most celebrated example of symmetry considerations being put to heuristic use', and the avowed structuralist Kantorovich has likewise dubbed it 'the most successful and historically most influential' of the internal (i.e. non-spatiotemporal) symmetries.[21] The latter has also stated that 'one of the best ways' to establish the thesis of ontic structuralism is to examine its implications for structure-object relations, and given its paradigmatic status in particle physics it does indeed seem right to say that if group-based structuralism is to be thought to work anywhere, it should be shown to work here.[22]

Since this symmetry pertains to the strong interaction, the choice of this as our structure entails that the objects of interest will be the strongly interacting particles – that is, the quarks and hadrons. Given that I have already singled out supervenience as the priority relation in question, what I want to do in this chapter is closely study the question of whether – and *modulo* what assumptions – one can say that these particles supervene on this structure. Now, something like this issue has already been discussed by Kantorovich, since he claims that the global SU(3) symmetry 'dictate[s] via its representations the hadron spectrum (i.e., the variety of charges and other quantum numbers, such as total isospin) and determine[s] the possible outcomes of hadron interactions'.[23] Since supervenience amounts to determination, I take

[20]Guzey and Polyakov [2004], pp673-4.

[21]Redhead and Debs [2007], pp39-40; Kantorovich [2003], p660.

[22]Kantorovich [2009], p79. One hesitation one might have about this choice is that the global SU(3) symmetry is no longer regarded as a fundamental symmetry, being seen instead as an 'accidental' consequence of the fact that the strong interaction has a local SU(3) colour gauge symmetry in tandem with the fact that the three lightest quarks have masses within 10% of each other (plus, as is often forgotten, the fact that the energy scale associated with QCD is so high as to make that 10% difference insignificant). But this is not a relevant point for our purposes: I am not here setting out to establish *which structure* out of the category of structures used in particle physics is more fundamental than another or more fundamental than the rest (if any), but rather whether structure *as a category* is more fundamental than objects *as a category*, and hence more fundamental than any of the objects in that category. Thus, if the strongly-interacting particles – including the fundamental strongly-interacting particles – can be shown to supervene upon the global SU(3) flavour structure, then the core structuralist claim will go through, *regardless* of whether this structure is or is not the most fundamental structure one could cite in this connection.

[23]Kantorovich [2003], p663 *et passim*. Kantorovich's work is also recounted approvingly in Ladyman [2007], Section 4.1 and Ladyman and Ross [2007], Section

this language of 'dictation' to amount to a supervenience claim. However, he has precious little to say about how exactly this 'dictation' is supposed to work, at least not in any detail. Furthermore, as we will see, the idea that this symmetry alone suffices to determine the hadrons cannot be regarded as correct as it stands.[24] This is in fact something that Kantorovich himself seems to concede, for he states that

> [o]f course, we do not have here an absolute dictate; once we choose a representation for a family of hadrons that have some common properties, the classification of the rest of the family is determined. However, this kind of 'dictate' is weaker than the dynamical dictate that will be discussed when we turn to gauge dynamics.[25]

But one would like to know what this lack of an 'absolute dictate' amounts to in less metaphorical terms. Certainly, if structuralism is to present itself as a viable and compelling alternative to standard 'object-oriented' metaphysics, then what exactly is going on with such priority claims will have to be presented more sharply.[26]

I propose, therefore, that we try to elucidate in more explicit terms the extent to which the strongly interacting particles – hereafter for brevity 'SIPs' – may be said to supervene on dynamical symmetries. As already implicated, the claim that symmetry 'dictates' the SIPs is more complex than it has been

3.3.

[24]There are in fact a number of other problems with Kantorovich's argument in addition to the lack of clarity regarding how it is that 'symmetry dictates hadrons'. For one thing, as well as presenting the fundamentality of structure along supervenience lines, he also adopts something closer to a dependence-based account to express his priority views and slips between the two as though they are interchangeable. This is problematic in itself, but a second problem is that his argument that the hadrons depend upon the symmetries fares even more poorly than his supervenience-based argument, since it patently will not convince anyone who does not already share his conclusion. I will explain why this is in the next chapter.

[25]Kantorovich [2003], p664.

[26]If it helps avoid confusion at this point, I think that the contrast with the gauge symmetries that Kantorovich is referring to is that in the latter case – at least in his opinion – the specific representations that are entailed is determined, not just the rest of a representation given the existence of a subset of the particles in it. Whether or not that is right, what this talk of 'specific representations' being determined means will be explained imminently.

presented in the literature thus far, and it will be helpful to extend at the outset the structure we will be concerned with from SU(3) to SU(3)⊗SU(2)⊗U(1). (Of course, it will not matter from the point of view of structuralism's main thesis that a *different* structure may have to be used to make its claims most plausible; all that structuralism demands is that *some* physical structure can be used to establish them.) Here – and as will be gone into in more detail immanently – the SU(3) structure relates the internal 'flavour' properties *isospin* and *hypercharge*, SU(2) pertains to *spin* and U(1) to *baryon number*. The reason that this extended structure will be used is because the latter two groups play a pivotal role in the *constituent quark model* that followed on from, and immeasurably improved, the original Eightfold Way hypothesis that was based on SU(3) alone, and do so in a way that is highly relevant to the supervenience claim. Thus, and to be clear at the outset, I shall take the first three quarks – that is, the up, down and strange quarks – and all the hadrons that can be built up from them as the objects relevant to this structure, where each of these particles is defined in terms of the relevant specific determinate values of isospin, hypercharge, spin and baryon number.[27] Furthermore, note that when I talk about 'objects' in what follows, I will remain entirely neutral – at least initially – on how they are to be conceived of ontologically, beyond that they instantiate the determinate properties that I take to define their kind. That is, I will remain neutral on whether we ought to consider them in terms of the 'bundle' view, the 'substratum' view and so on. *Any* metaphysical conception of a particle will, after all, have to consider it to possess the appropriate determinates of these properties; whether the supervenience claims that I will attempt to derive conflict with any of the received metaphysical conceptions of objects is a matter I will return to, albeit briefly, below.

With those clarifications of structure, objects, and the choice of priority relation in place, I will now move on to present the Eightfold Way hypothesis

[27]Thus, when I purport to consider the question of whether the SIPs supervene on the symmetries, I am really being elliptical since there are other SIPs that do not involve these quarks but that are ignored here. Likewise, the SIPs that I do consider have more properties than those I present as definitive of them here (such as their weak-interaction properties), but these will be abstracted from them for present purposes. (The structuralist will of course hope that, if a structuralist story can be told about these particles, and considered only with these properties, a similarly structuralist story can likewise be told about neglected other particles and properties.)

and from there the constituent quark model. As is so often the case, it will prove most straightforward to begin by more or less just recounting the episode historically. While my presentation of how the relevant events unfolded will be rather airbrushed and simplified, it will hopefully not be *too* simplified to put us in a good position to assess in the closing sections what, if any, priority claims it furnishes for supervenience-based structuralism.

8.2 From Hadrons to Quarks (and Back to Hadrons)

8.2.1 The Eightfold Way Hypothesis: Identifying the Structure of Multiplets

What inspired the postulation of the Eightfold Way hypothesis was the observation that the then-known SIPs of the same spin and baryon number, and approximately the same mass fell into striking patterns when arranged according to their isospin and hypercharge (see Figure 8.1).

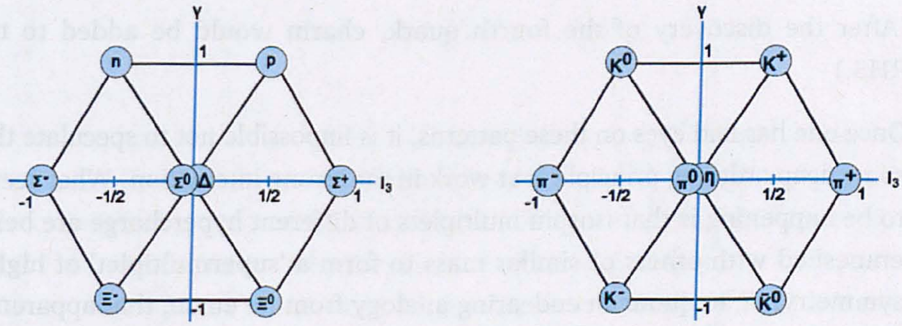

Figure 8.1: Baryons and Mesons: Octets of SU(3)

The diagram on the left of Figure 8.1 is the 'octet' of the then-known baryons ('heavy particles'): strongly interacting particles defined as having baryon number $B = 1$. All of these particles are fermions. The masses here are all around the 1GeV mark and to within 30% of one another, with the differences between the isospin multiplets falling into a definite pattern.[28] The diagram

[28]This may seem to stretch the term 'approximately' to breaking point, but it is encouraged by the fact that the mass differences are significantly less than the masses of the other known baryons (together with the fact that the differences between the

on the right belongs to the mesons ('medium-mass' particles): SIPs defined as having $B = 0$. All of these particles are bosons. Owing to the conspicuously small mass of the pions, the mass differences here are much greater than in the fermionic cases, but are nevertheless smaller than the typical masses of hadrons, and they again the differences exhibit a definite pattern. (If the pions are ignored, however, the meson masses are much closer – to within 10% of one another in this case.) In both cases, particles with like charge are arranged down the diagonal lines sloping upwards from right to left. This relation between charge, isospin and hypercharge is in accordance with the Gell-Mann–Nishijima formula, which had been empirically established in the 1950s:[29]

$$Q = I_z + Y/2$$

Here, I_z is the third component of the *isospin* and Y is the *hypercharge*. The latter encodes the strangeness S of the particle via

$$Y = S + B. \tag{8.1}$$

(After the discovery of the fourth quark, charm would be added to the RHS.)

Once one has laid eyes on these patterns, it is impossible not to speculate that some deep ordering principle is at work in the strong interaction. What seems to be happening is that isospin multiplets of different hypercharge are being enmeshed with others of similar mass to form a 'supermultiplet' of higher symmetry; or, to quote an endearing analogy from Ne'eman, that apparently unrelated sets of particle brothers are being revealed as first cousins.[30] Exactly how it is that the concept of a 'higher symmetry' can explain this striking phenomenon is something I will try to explain in a moment, but in order to do so it will be helpful to get a grip on the symmetry concepts already implicit in these diagrams and upon which the Eighfold Way would be built.

The patterns lying along to horizonal axes constitute *isospin multiplets*. The concept of isospin, and its group SU(2), had been introduced by Heisenberg

isospin multiplets fall into a definite pattern).

[29]Nakano and Nishijima [1955]; Gell-Mann [1956].

[30]See Pais [1986], p519.

in 1932 as a device to simplify nuclear calculations (though the term 'isospin' itself was coined by Wigner).[31] Gell-Mann subsequently postulated that isospin was a property of all SIPs, and also that strangeness should be common to all members of an isospin multiplet. Given the relationship between strangeness and hypercharge in (8.1), the isospin multiplets may then be arranged vertically in order of increasing Y. Strangeness had been introduced as the parameter that, roughly speaking, encodes the peculiarly long particle lifetimes observed in the new particles produced as the first accelerators came online, and since it manifested itself in the strong interaction simply as an additive quantum number with integer eigenvalues, its governing group was taken as the 'circle group' U(1).[32] However, instead of S the quantity Y was used for convenience, since it permitted a unified treatment of the mesons and baryons given the Gell-Mann–Nishijima relation. Baryon number, like strangeness, was also manifested simply as an additive and integral quantum number, so the operator for baryon number, and hence that for Y as well, were likewise taken to be generators of U(1) symmetry. However, given the evidently tight connection between baryon number and spin, it was argued that B invariance should not be treated as part of the 'internal' symmetry group but should feature instead as a part of the 'external' (spacetime coordinate-dependent) group. This permits the baryon number to be fixed independently of the internal symmetries. The external symmetry was taken include at least the inhomogeneous Lorentz group, or 'Poincaré group', of which the spin group is a subgroup, and also the space-inversion group. And since the particles in the above multiplets all have the same spin and baryon number (as well as parity), it was inferred that the associated groups commute with the internal symmetry group (for by Schur's lemma,

[31]Heisenberg [1932]; Wigner [1937].

[32]Cornwell [1984], p431. U(1) is the group consisting of all complex numbers with modulus 1 under the relation of multiplication. The reason that additive integer eigenvalues are the signature of U(1) symmetry is because there is an irreducible representation $\Gamma_n[e^{i\theta}]$ of U(1) given by $\Gamma_n[e^{i\theta}] = e^{in\theta}$ for every $n \in \mathbb{Z}$, where the restriction to integers follows from the fact that $e^{in\theta}$ must equal $e^{in(\theta+2\pi)}$; these representations obviously have the property that $\Gamma_{n_1}.\Gamma_{n_2} = \Gamma_{n_1+n_2}$. Thus additive, integral quantum numbers imply the presence of a U(1) symmetry. (Note, however, that the fact that the irreducible representations of U(1) are labelled by integers does not entail that the Hermitian operators that generate the U(1) transformation them must *only* have integer eigenvalues; rather, all that is required is that they have eigenvalues y such that $ny \in \mathbb{Z}$, so that y may be a rational fraction (see Cornwell *ibid.*)). This was, of course, to turn out to be the case with quark charge.

this means that the eigenvalues of these external group operators can be used to label the representations of the internal group). The full group of the strongly interacting particles was then taken to have the direct-product form

$$\text{Ext} \otimes \text{Int} \supseteq \text{Poincaré} \otimes \text{Parity} \otimes \text{B} \otimes \text{Int} \supset \text{SU}(2)_S \otimes \text{U}(1)_B \otimes \text{Int}$$

where the internal group must subsume the $\text{SU}(2)_I$ and $\text{U}(1)_Y$ groups. Our focus for considering the structuralist priority claims that may be made in this context will be on the $\text{SU}(2)_S \otimes \text{U}(1)_B \otimes$ Int subgroup that appears on the right-most equation here (where here I have included subscripts to keep the various symmetries clearly distinguished). The next step in the process was to ascertain the as-yet unidentified internal group that features here, considered in the limit of perfect symmetry. Before I discuss how that was achieved, however, I want to make plausible that the Eightfold Way's postulation of a 'higher symmetry' transcending the $\text{SU}(2)_I$ and $\text{U}(1)_Y$ symmetries can explain the occurence of these patterns.[33]

Explaining Particle Patterns through 'Higher Symmetry'

Two things have to be accounted for in the particle patterns above. First of all, and most saliently, there is the striking geometric pattern in the distribution of I_z and Y eigenvalues. Secondly, there is the approximate equality of the masses, which – as a first approximation – we will take as an exact equality. What I will try to show now is that postulating a *symmetry in the dynamics*, and interpreting the particles as the *basis vectors of an irreducible representation of that symmetry group*, suffices to explain both phenomena.

Let us begin with the second phenomenon – namely, that of the fact that all the states in each diagram have (approximately) equal masses. To say that there is a *symmetry in the dynamics* of the strong interaction that produces these particles is to say that there is a group, G, of transformations that leaves the Hamiltonian invariant. Now, since we are dealing with quantum mechanics, we need transformations that leave inner products invariant and, as Wigner showed us, that means that we can restrict our attention to unitary

[33] Of course, much more detail may be found in the references, or any textbook on particle physics. Here, however, I want just to bring out the basic concepts.

and antiunitary operators; we will restrict our attention here to the unitary case.[34] Thus, if we let unitary operators in G be U_i and the strong interaction Hamiltonian be H_s, the statement that H_s is invariant under G is the statement that

$$[H_s, U_i] = 0 \text{ for all } U_i \in G;$$

hence we have

$$H'_s \equiv U_i H_s U_i^{-1} = H_s \tag{8.2}$$

for all U_i. Now consider the eigenvectors ψ_j of H_s,

$$H_s \psi_j = E_j \psi_j. \tag{8.3}$$

Suppose now that the action of a U_i on some ψ_j is given by $U_i \psi_j = \psi_k$, so that the ψ_k form a basis for a representation of the U_i.[35] From (8.2) and (8.3) we have

$$\begin{aligned} U_i H_s U_i^{-1} U_i \psi_j &= E_j U_i \psi_j \\ \Rightarrow H_s \psi_k &= E_j \psi_k \end{aligned}$$

and thus we can deduce from the invariance of H_s under the action of U_i the existence of another eigenstate, ψ_k, of H_s with the same energy as ψ_j. Thus the existence of a dynamical symmetry means that we can expect the eigenvectors of H_s to resolve themselves into sets of states that all have the same energy. However, there is no guarantee yet that *all* the states that are connected via the U_i will have the same energy eigenvalue, as opposed to there merely being a *number* of distinct sets of states, each whose members share some one eigenvalue but possibly different such eigenvalues across different sets. Indeed, the latter may well be the case if the matrices U_i form a *reducible representation* of the group G. To see this, note that in such a representation each matrix is expressible in block-diagonal form, and suppose that the U_i are $n \times n$ matrices expressible by two such blocks. Suppose further that these blocks take up rows 1 to j and $j+1$ to n respectively, so that the basis vectors for the representation are n-row column vectors. If the U_i were

[34] Unitary operators have the property that $U^\dagger = U^{-1}$. Antiunitary such operators are needed to deal with discrete transformations, which will not be considered here.

[35] See McVoy [1965]. A *representation* of a group is a homomorphism between the group elements and a set of operators which act on a linear vector space.

so expressible, then we could consistently have $H_s\phi_{1,j} = E_1\phi_{1,j}$, where $\phi_{1,j}$ is a column vector of j rows, and also $H_s\phi_{j+1,n} = E_2\phi_{j+1,n}$, where $\phi_{j+1,n}$ is a column vector of $n-j$ rows. By operating with the U_i and turning the handle as we did before, we will find one set of vectors all with energy E_1, and another set of vectors all with E_2. But there is no requirement that $E_1 = E_2$, in contrast to the phenomenon we want to explain. The key to avoiding the latter scenario is therefore to find an *irreducible* representation of the group G: that is, a representation that is *not* convertible to block-diagonal form by any similarity transformation.[36] The states of the spaces such representations act in will still be closed under the action of the group operators, but will be such that they contain no smaller subspaces that are similarly closed (in contrast to the case above). Such a space is called an 'irreducible invariant subspace', but it is common in physics parlance to refer to the spaces that the irreducible representations act in as the irreducible representations themselves.[37] The net result is that we can now see that postulating a *symmetry in the dynamics*, and then conceiving of particles as the *basis states of irreducible representations* of the corresponding symmetry group, can explain the existence of particles all of which have the same mass.[38] The basis states of such a representation are said to constitute a *multiplet*.

Considering sets of particles, such as those shown in Figure 8.1, as the multiplets of a symmetry group can therefore account for why they have been produced with (approximately) the same energy. But what is so striking about these particles is less the relationships between their masses than the pronounced geometric symmetry in the (graphical) distribution of their properties, and this too may be explained by postulating a symmetry in the dynamics. In order to get a sense of why this is so, the first thing to note is that the quantities I_z and Y that label the axes of these diagrams obviously

[36]See Lichtenberg [1978], p38 for further details on all of this.

[37]Or indeed the corresponding weight diagram, to be introduced below.

[38]Note that the simple picture we will describe runs into difficulty when symmetry breaking – manifested by the only *approximate* equality of masses in a multiplet – is considered, owing to O'Rafertaigh's theorem; see, e.g., Marshak [1993], pp200-202. Therefore the internal symmetry was initially treated as though it were a perfect symmetry, with the symmetry breaking and the patterns within it to be dealt with perturbatively once the underlying group was discerned. Both Gell-Mann and Zweig's original papers on the quark model were in fact largely devoted to ascertaining the pattern in the symmetry breaking, but I will not discuss it here.

correspond to observable quantities. Given this, they are to be represented by Hermitian operators. Since it may be shown that any unitary operator U may be written as $U = e^{i\theta A}$, where θ is a parameter and A a 'generator' that can be shown to be Hermitian, the observable quantities must be taken to correspond to the generators of the unitary operators U_i in G. It is therefore useful from an operational point of view to work directly with the generators and this is, in fact, the approach that particle physics normally takes. That we can in large part work with the generators alone in studying the group follows from the work of Lie, for Lie's great insight was that all but the global properties of a group may be deduced by studying the elements of it that differ infinitesimally from the identity, and that all the relevant information about their behaviour in this region is contained within the algebra of the generators.[39] In fact, since the study of particle multiplets is usually impervious to the global properties of the group, in particle physics 'the term "group-theoretical" almost always means "Lie-algebra–theoretical"', and, insofar as we are primarily interested in particle multiplets, there is no loss of generality in working directly with the algebra.[40]

To see how a study of the algebra of a symmetry group can reveal why particles materialize in such highly structured sets as those shown above, it is most useful to consider an irreducible representation of that algebra and place it in 'standard form' – that is, in the *Cartan-Weyl basis*. Such a basis is always available for a semi-simple (hence also a simple) Lie algebra, and since there is no known systematic method for extracting the representations of non-semi-simple Lie groups, we will here follow standard physics practice in restricting our attention to semi-simple Lie groups and their algebras (with the exception of the circle group U(1), whose representation theory is straightforward).[41]

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[39]The algebra of the generators X_i is the set of equations $[X_i, X_j] = i\Sigma_{k=1}^{r} c_{ij}^{k} X_k$, where the group is characterized by r real parameters (e.g. two rotation angles).

[40]Slansky [1981], p6; Lichtenberg [1978], p58; see also p36. As Slansky also notes, in particle physics literature the same *symbol* is also often used to denote both the group and its algebra; since it is almost always the local properties that are of interest, the 'notation is not as sloppy as it first appears' (*ibid.*).

[41]The simplicity or semi-simplicity of a Lie group is defined with respect to its algebra as follows. Consider a subset of operators B of a Lie algebra A. If the commutator of any operator in A with any operator in B also lies in B, then B is said to be an *ideal*. A Lie algebra is simple if it is not Abelian and its only nonzero ideal is A itself; a Lie algebra is semisimple if it has no nonzero Abelian ideals. A Lie group is then defined as simple or semisimple iff its associated algebra is simple

To regiment the algebra in this way, we must first compute the maximum number of commuting generators H_i that can be formed in the algebra. The number, r, of such generators is called the algebra's *rank*. The aim then is to find linear combinations, E_α, of the remaining generators such that

$$[H_i, E_\alpha] \propto E_\alpha$$

– that is, a basis such that the remaining operators may be viewed as 'step operators' with respect to all the H_i.[42] Such a basis can always be found, and we then obtain the following relations between the operators:

$$[H_i, H_j] = 0;$$

$$[H_i, E_\alpha] = \rho(\alpha)_i E_\alpha, \tag{8.4}$$

which defines the components of the *root vectors*, $\rho(\alpha)$;

$$[E_\alpha, E_\beta] = \rho(\alpha).H$$

if $\beta = -\alpha$; if $\beta \neq -\alpha$ but $\rho(\alpha) + \rho(\beta)$ is a non-vanishing root vector, we have

$$[E_\alpha, E_\beta] = N_{\alpha\beta} E_{\alpha+\beta}$$

where $N_{\alpha\beta}$ is a constant, and otherwise

$$[E_\alpha, E_\beta] = 0.$$

The root vectors defined in (8.4) may then be plotted in an r-dimensional root space to produce the *root diagram* associated with the algebra. This root diagram is unique to the algebra and may be used to deduce all information about it. Crucially, the root diagram may be shown to contain a great deal of symmetry – such as that, for any root $\rho(\alpha)$ contained in the diagram, $\rho(-\alpha) = -\rho(\alpha)$ is also contained in it, and for any two roots $\rho(\alpha)$ and $\rho(\beta)$, there is a third root $\rho(\gamma)$ obtained by reflecting $\rho(\alpha)$ in the hyperplane

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or semisimple respectively. Semi-simple Lie groups (algebras) are direct products (sums) of simple groups (algebras), which is why Cartan's classification of the simple Lie algebras was simultaneously a classification of the semi-simple ones.

[42] A familiar example is the linear combinations J^+ and J^- of the Pauli matrices in SU(2).

perpindicular to $\rho(\beta)$. This latter is said to constitute a 'Weyl reflection' of $\rho(\alpha)$.

So far so abstract, but it is this symmetry in the root diagram which can be used to furnish an explanation of the striking patterns such as those in Figure 8.1. As noted, the commuting generators correspond to the simultaneously observable properties that may be used to define particles (such as the properties I_z and Y above). We can therefore elect to identify a given kind of particle in terms of a *weight vector*, $|\mu\rangle$, defined by

$$H_i|\psi\rangle = \mu_i|\psi\rangle. \tag{8.5}$$

Here $|\psi\rangle$ is a common eigenvector of the H_i and a basis vector of the (irreducible) representation. The other weights can be obtained from any other by repeated action of the operators E_α, the effect of which may be derived from the commutation relations between the H_i and E_α in the 'standard form' listed above:

$$H_i(E_\alpha|\mu\rangle) = (\mu + \rho(\alpha))_i(E_\alpha|\mu\rangle). \tag{8.6}$$

This equation establishes that E_α shifts the weight $|\mu\rangle$ to $|\mu = \rho(\alpha)\rangle$:

$$E_\alpha|\mu\rangle \propto |\mu + \rho(\alpha)\rangle. \tag{8.7}$$

Thus we can see through (8.7) that the effect of these operators is to shift a given weight to make a new weight with eigenvalues $\mu + \rho(\alpha)$, justifying their appellation of 'step operators'. This action of the E_α on a given weight defines the *weight diagram* associated with a given irreducible representation. The $\rho(\alpha)$ here are, as before, the roots of the algebra, so that we can see that the weights of any representation will be displaced from one another in the same way as the roots themselves, and thus that *the geometric structure of the algebra's root diagram is imported into its weight diagrams*. One may discern, for example, that sets of weights ('equivalent weights') may be obtained from one another by Weyl reflections in planes perpendicular to the roots, so that this 'reflection' symmetry of the algebra's root diagram is preserved in the weight diagrams of its representations. A theorem that succinctly conveys the high degree of symmetry in these diagrams that results from the symmetry in the root diagram is that the weights of any irreducible representation must sum to zero.[43] While the deduction of the structure of the full weight diagram of a given representation from its algebra needs a patient and systematic treatment, through (8.7) we can, when we recall the symmetry of the root diagram, at least get the sense that that not only does the postulation of a symmetry explain the (approximately) degenerate masses of the particles in Figure 8.1, but also the striking geometric pattern obtained when they are plotted on the I_z and Y axes. At this point, then, *we interpret the diagrams in Figure 8.1 as the weight diagrams of a semi-simple Lie algebra*, and thus the particles themselves as the ***basis vectors of an irreducible representation of that albegra*** – or, in other words, as the members of a *multiplet*. The next task is to ascertain which symmetry group they are the basis vectors of, and let us now consider how this was done.

Identifying the 'Higher Symmetry'

As already pointed out, the rank of a group describes the maximal number of mutually commuting generators, and since the observed baryons and mesons in Figure 8.1 have determinate values of I_z and Y simultaneously, each of which is conserved during interaction, the group sought was of rank two. And given the observed enmeshing of the various isospin multiplets into patterns in the $I_z - Y$ plane, what was needed was a group that contains both the corresponding groups SU(2) and U(1) as proper subgroups, but such that its structure does not just contain those subgroups simply as a product. This is because any semi-simple Lie group is expressible as a direct product of simple groups; if the sought rank-two group was semi-simple, it would be a direct product of rank-one groups, and hence a direct product of two groups, each of which contains only one of I_z or Y. Being a direct product, all the operators in the group containing I_z would commute with Y, so that every possible isospin multiplet would occur with every possible hypercharge. Were that the case, the observed restrictions on possible Y values for each multiplet, such that each isospin multiplet aligns with only one Y-value, would not be manifest. For this to happen, a more 'intimate' way of conjoining the two groups had therefore to be found.

[43]Cornwell [1984], p564. For a fuller treatment of this issue, see Cornwell *ibid.* and Lichtenberg [1978], Chapters 5 and 6.

In seeking a higher-symmetry scheme that would take the various isospin multiplets and enlarge them into a 'supermultiplet', what was thus needed was a *simple* Lie group.[44] Given that the (compact) simple Lie algebras had been exhaustively classified by Cartan, what remained to be done was to identify the groups meeting the above criteria and compare how well their algebras fitted with the observed features of the strong interaction. This task is made vastly easier than one might have intitially assumed by the fact that there are a finite number of algebras of any given rank – a number that is, moreover, small for small rank.[45] It turns out that the algebras of all the rank 2 simple Lie groups contain the algebra of U(1) $\otimes$SU(2) as a subalgebra, which means that if any of these groups is a symmetry of the hadrons, hypercharge and isospin will be conserved.[46] In order to move forward, what was then needed was a comparison of the implications for particle properties associated with each algebra to find which agreed best with experiment, and this task was undertaken by Ne'eman.[47] On the grounds that the other algebras ruled out certain known transitions and/or forbade a nuclear magnetic moment for the neutron, for example, the successful candidate was declared to be a group from the same stable as isospin – namely, the group SU(3). It thus fell to the eight generators of the SU(3) group to 'tie together strongly interacting particle multiplets with different values of I_z and Y (but same spin and parity) in approximately degenerate supermultiplets', and hence reproduce the striking patterns above.[48]

These, then, were the steps that were taken to ascertain that there was an SU(3) symmetry afoot in the strong interaction. In the wake of this realization, Gell-Mann and Ne'eman hypothesized that *all strongly interacting particles occur in SU(3) multiplets*: that is, the SIPs are given by basis states of the irreducible representations of SU(3), so that they will arrange themselves into weight diagrams corresponding to these irreducible representations.[49] This

[44]Lichtenberg [1978] p77; p68. It is in large part due to the fact that the gauge group associated with the Standard Model also has this 'pasted-together' structure that it is regarded as so imperfect.

[45]McVoy [1965], p89.

[46]Lichtenberg [1978], p77.

[47]Ne'eman [1963].

[48]Gell-Mann [1964b], p7.

[49]This oversimplifies the history a bit, since the original group of the Eightfold Way was in fact the adjoint group SU(3)/Z(3). This modification was made to screen out the representations of 'non-zero triality' – that is, representations containing particles

proposal constitutes the *Eightfold Way hypothesis*.[50] To put us in a position to predict which particles are realized in accordance with this new hypothesis, what these irreducible representations are had to be ascertained. Group theory supplies the answer, and the representations of SU(3) may be deduced to form an (infinite) series with dimensionalities given by 1, 3, 6, 8, 10, 15, 27... The 8 forms the adjoint representation – that is, the representation whose dimensionality equals the number of generators in the group; since the then-known baryons and mesons were assigned to this representation, it was from this that the approach drew its name.

The Eightfold Way hypothesis of Gell-Mann and Ne'eman was famously corroborated almost immediately with a triumphant success in the shape of the Ω^-, and it will be useful to go through how this happened. By the end of 1961, 9 new particles were known, including a full isospin multiplet of delta particles. But there is no 9 in the above series, and the only nearby candidates were the 10, 15 and 27. The 15 was ruled out as it did not contain a multiplet with the values of the Δ particle.[51] And, at a conference in Geneva in 1962, it was announced that two $I = 3$, $Y = 2$ particles had failed to materialize in scattering experiments designed to produce them – particles that would be needed to fill up the 27. Gell-Mann and Ne'eman, both present at the conference, saw 'the pyramid being completed before their very eyes' and the Ω^- was at that moment predicted as the tip of the triangular 10 multiplet. It was observed at Brookhaven in November the next year, complete with all the properties – including its mass – that had been predicted the year before.

Although the symmetry approach had already been used in successfully predicting particles, this was its most jubilant celebration yet.[52] It was therefore

with non-integer charges, which at the time were regarded as abhorrent (which seems rather quaint by today's lights!). All of these can be built up from products of the 8; hence initially the 8 was taken as fundamental, and only one-third of the SU(3) representations were initially taken as candidates for multiplets. However, once quarks had been accepted, the restriction to zero-triality representations was dropped.

[50]The classic reference on this is Gell-Mann and Ne'eman [1964].

[51]Well, it is more accurate to say that it *would* have been ruled out. Given the restriction at this point to zero-triality representations alluded to in the previous note, the 15 wasn't even on the table.

[52]Isospin symmetry had been used to predict, for example, the neutral pion, kaon and hyperon: see Pais [1986], p520.

the prediction of this particle on the basis of group-theoretic considerations that firmly consolidated opinion on the power of the symmetry approach as a tool of particle physics.[53] Nonetheless, and in spite of this success, the Eightfold Way could not be regarded as wholly satisfactory as it stood. While it had demonstrated, as the Ω^- example makes clear, its ability to predict the missing particles in a multiplet that was almost full, it could not predict *which* of the infinite number of possible SU(3) multiplets would be partially filled in the first place. This was clearly a major shortcoming. Addressing it would lead physics to the *constituent quark model.*

8.2.2 The Constituent Quark Model: Predicting Specific Multiplets

Working independently, Murray Gell-Mann and G. Zweig realized that the natural strategy for transcending the predictive limitations inherent in the Eightfold Way was to start thinking seriously about the *fundamental,* or *defining,* representations of SU(3) – the fact that the particles in them had not been observed notwithstanding.[54] In the context of the semi-simple Lie groups and its representation theory, the fundamental representations are the lowest-dimensional, non-trivial irreducible representations – or, alternatively, any representation whose highest weight is a *fundamental weight.*[55] What is distinctive about the fundamental representations is that all the other representations of the group can be constructed from them by taking their tensor products. In this sense, the fundamental representations may be regarded as the 'building blocks' of all the others and it is from this feature that they derive their name. (Coupling 1-dimensional representations together, by contrast, will never produce anything but the 1-dimensional representation and it is in

[53] See Bangu [2008] for more on the Ω^- prediction and its reception.

[54] Gell-Mann [1964a]; Zweig [1964].

[55] The fundamental weights, Λ_i, will be defined below. But to get a sense of what makes these weights 'fundamental', it may be helpful to note that a property of the Λ_i is that the highest weight, $|\mu\rangle'$, of any other irreducible representation can be expressed as $|\mu\rangle' = \Sigma n_i \Lambda_i$, where n_i is a non-negative integer. Since it turns out the whole of a weight diagram is, in turn, determined by its highest weight, we can view these fundamental weights as being determinative of the rest of the group's multiplets and in this sense 'fundamental'. I will have more to say on fundamental weights below.

this sense that it is trivial.) *Since a rank l simple Lie group has l* fundamental representations, SU(3) has two; these turn out to conjugate to one another and to each contain three states.[56] They are therefore designated as the '3' and '3*', with the latter containing the anti-particles corresponding to the former; the full set of states is labelled [anti-] 'up', 'down' and 'strange'. The particles corresponding to these representations can be displayed by means of the associated weight diagrams, which are shown in Figure 8.2. In general,

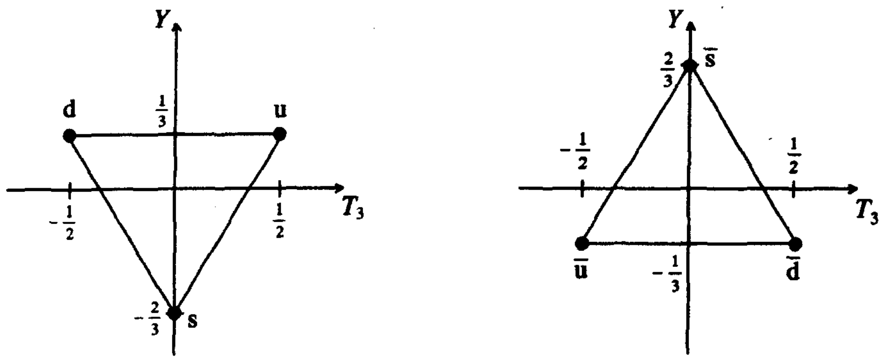

Figure 8.2: Quarks and Antiquarks: Triplets of SU(3)

the irreducible representations that are obtained by taking products of other irreducible representation produce a *Clebsch-Gordan* series.[57] Some examples of this series, obtained by taking products of fundamental representations, are

$$3 \otimes 3^* = 8 \oplus 1, \text{ and}$$

$$3 \otimes 3 \otimes 3^* = 15 \oplus 3 \oplus 3 \oplus 6^*.$$

Since from combinations of a finite number of fundamental representations only finitely many other representations are produced, the hope was that some means of predicting which of the infinitely-many possible representa-

[56]Lichtenberg [1978]. Mathematically only one of the two fundamental representations of $SU(3)$ is needed since $3 \otimes 3 = 6 \oplus 3^*$, $3^* \otimes 3^* = 6^* \oplus 3$. But, for reasons of physics (specifically the physics of particles and antiparticles), we keep both.

[57]The Peter-Weyl theorem establishes that every finite dimensional invariant space of a semisimple Lie group is completely decomposable into irreducible invariant subspaces, so that the space of any finite dimensional representation of a semi-simple Lie group can be parcelled out into non-overlapping 'multiplets'. This guarantees the existence of Clebsch-Gordan expansions.

tions of SU(3) are actually realized would be furnished through considering which combinations of fundamental representations might be appropriate on physical grounds. This was the basis of the *constituent quark model.*

The key insight that led to the correct combinations of the fundamental representations was that idea that, by analogy with the role these representations themselves play in the group representation theory, the particles in these representations could be thought of as the 'building blocks' for the hadrons in the higher-dimensional multiplets. On these grounds, any particle contained within the fundamental multiplets has a claim to be considered as a *fundamental particle*, and those featuring in the higher-dimensional multiplets constructed from them could in consequence be regarded as composite. These particles Gell-Mann baptized the *quarks*.[58] It is therefore clear to see that the symmetry approach that had been taken toward the hadrons through the Eightfold Way hypothesis led naturally to a consideration of prospective fundamental particles and the principles that might govern their combination. What Gell-Mann and Zweig discovered upon pursuing that thought was that the internal (SU(3)) and external ($SU(2)_S \otimes U(1)_B$) group structures that were flagged up at the outset could be used to determine (i) the properties of the quarks, and (ii) the composite hadrons we can expect the quarks to compose – at least to a very great extent.[59] This extent was not total, however; as well as some initial conditions (something of course to be expected), considerations of *simplicity* had to be invoked in order to complete the determination as well. Let us now see how this works.

The internal group properties are the most straightforward properties of the quarks to deduce, as this can be done directly from the SU(3) structure plus an initial condition to fix the normalization.[60] This can be seen as follows. Each representation is three-dimensional and can therefore decompose into isospin multiplets in only three ways: three singlets, one triplet, or a singlet

[58]Zweig dubbed them the 'aces' but it was Gell-Mann's nomenclature that stuck. Both Gell-Mann and Zweig were initially skeptical that the particles in the fundamental representations were real particles. Thus Gell-Mann spoke for a time of 'mathematical quarks' and Zweig wrote, for example, that 'it is quite possible that aces are completely ficticious, merely proving a convenient way of expressing a symmetry' ([1964], p2). This hesitancy did not last long in either case.

[59]Note, however, that it was not until QCD that a *mechanism* to keep the composite together was on the table.

[60]The following is adapted from Coleman [1966].

and a doublet. The first case is mathematically impossible, for it implies, amongst other things, that the representation is degenerate on the boundary – something that is provably forbidden.[61] The second is mathematically possible, but physically unacceptable as it implies that only integral isospins are possible in the higher multiplets – thus ruling out the neutron and proton (amongst others). This leaves only the third possibility, and this uniquely determines the I_z properties of the quark triplet (namely as one $I_z = 0$ particle and an $I = 1/2$ isospin doublet). This leaves the hypercharge to be assigned. In accordance with the observation that the isospin multiplets of the observed higher hadrons are separated by one unit of Y – which we may take as an initial condition – they must be so here; so if the Y assignments of the singlet is y, that of the doublet is $y + 1$ (or $y - 1$; the choice serves to differentiate the conjugate or 'anti-particle' representation). But since Y is one of the two additive quantum numbers measureable simultaneously with the energy, it must be representable by a diagonal matrix, and since it is a generator of a special unitary group, it must be traceless. Together these imply that $\mathrm{Tr}Y = 3y + 2 => y = -2/3$.[62] This fixes the Y of every quark. And with the hypercharge and isospin for the quarks now in place, then via the phenomenological Gell-Mann–Nishijima formula – which now just corresponds to a rotation in $I_z - Y$ space – we can also retrieve their charges. Therefore, the SU(3) structure alone, plus some initial conditions provided by the obervation of certain hadrons, suffices to settle the internal (I_z and Y) properties of the fundamental SIPs, the quarks.

With the 'internal' properties of the quarks now established, we can contemplate the labels the fundamental representations as a whole, and hence the quarks in them, should receive from the spin and baryon groups as well as the combinations that the quarks should occur in, in order to produce hadrons. (As we will see, these are not independent questions.) Here things are less determined, and both Gell-Mann and Zweig had to deploy a number of as-

[61] It would also imply that all the operators of the isospin group were the identity. But this results in an inconsistency, for unless the other operators in the SU(3) group were the identity, then the group structure would be lost; and if they *were* all also the identity, then we would have the trivial (singlet) SU(3) representation, not a three-dimensional one as originally assumed.

[62] The 'special unitary groups' SU(N) are the groups of unitary representations that have unit determinant. The latter condition implies that the corresponding generators must be traceless.

sumptions at points where possible alternatives presented themselves along the way. However, out of a number of possible schemes devised by both Gell-Mann and Zweig, the 'simpler and more elegant' was in each case plumped for and, indeed, all of the assumptions that each of them made in arriving at a determinate model could be placed under a banner of *simplicity*.[63] Now, given that the baryons – which are fermions – are by this point hypothesized to be composites of quarks, it is clear that quarks themselves must be fermions. For it follows from the familiar 'vector sum rule' – itself just a statement of the Clebsch-Gordan decomposition for SU(2) – that only if the quarks have half-integral spin will their products be able to produce half-integral representations. And since the quarks are being understood as fundamental particles, then just as it is natural to put them in the fundamental representation of SU(3) it is natural to put them in the fundamental representation of SU(2), and thus the natural choice for this half-integer is 1/2 (since this is the value of that representation). Call this the *lowest eigenvalue* assumption.[64] Furthermore, since even-numbered products of SU(2) representations produce only integral-spin composites, baryons must be composed of an odd number of quarks. And the lowest odd number compatible with compositeness is obviously 3. This then plausibly represents the simplest choice, and thus it was this that was favoured by both Gell-Mann and Zweig.[65] Call this the *fewest parts* assumption. From here, we can have a guess at their baryon number. Since the baryon number is governed by the U(1) group with its

[63]Gell-Mann [1964a], p214 (in Gell-Mann and Ne'eman [1964], p168.)

[64]In fact, this assumption is so natural neither Gell-Mann or Zweig seem to have flagged it explicitly, but it is, of course, possible to make spin-1/2 baryons from products of, say, spin 3/2 particles. Perhaps this is because the $S = 1/2$ assumption accounts for spin 1/2 and 3/2 baryons only, which is all that had been observed. But it would have been hasty at this point to suppose that higher-spin baryons would never be seen (as well as question-begging to appeal to it here). Note as well, however, that not all of the currently known or hypothesized fundamental particles belong to the fundamental spin representation, or even have the lowest eigenvalue consistent with their being fermions or bosons – such as the spin-1 weak bosons or hypothetical spin-2 graviton.

[65]As Zweig put it, 'to narrow the field and give our problem a more explicit formulation, we will insist on picking a theory which contains the minimum number of units consistent with the observed strongly interacting particles and known conservation laws' ([1965], p192); as Gell-Mann put it, 'baryons can now be constructed from quarks by using the combinations (qqq), $(qqqq\bar{q})$, etc... It is assuming that the lowest baryon configurations (qqq) gives just the representations that have been observed' (*op. cit.*).

additive eigenvalues, the baryon numbers of these three quarks must add up to 1; likewise, the baryon numbers of the three quarks in an anti-baryon must add up to -1. The most efficient way to achieve this is to simply divide this number evenly between the three constituent quarks, giving them each baryon number 1/3. This is equivalent to saying that baryons contain only quarks and anti-baryons only anti-quarks.[66] Call this the *uniform division* assumption. (The only other alternative choice that is consistent with 'fewest parts' is $qq\bar{q}$, which would produce $B(q) = 1$.)

To summarize what has been achieved regarding the baryons by this point, the assumptions of *fewest parts* and *uniform division*, plus the fact that baryons are spin-1/2 particles, result in the $3 \otimes 3 \otimes 3$ composition for baryons and $3^* \otimes 3^* \otimes 3^*$ for anti-baryons. This in turn fixes the B assignment for the quarks as 1/3, which together with the deduced hypercharge also serves to fix their strangeness; the value for their remaining external quantum number, that of spin, is fixed by assuming *lowest eigenvalue*. Since one can find these values repeated today in the most up-to-date lists of particle data we have, it seems that we can only agree with Zweig when he says that 'simplicity, combined with the known complexity of particle physics, leads us to unique spin, isospin and strangeness assignments to the units [i.e. the quarks]', where we may take it that the 'known complexity' refers to the SU(3) structure that had already been deduced for the known hadrons.[67]

Moving on now to the mesons, it is clear that we need make no further assumptions in order to deduce which of *them* we can expect to find in nature. Since we have established that the quarks are spin 1/2 fermions, the smallest number of quarks compatible with a composite meson is 2. In accordance with the *fewest parts* assumption, then, that is the number that was chosen. But it is clear that the quark and anti-quarks must collaborate here if their baryon numbers are to give us back zero, since we will need baryon numbers that are equal and opposite.[68] Thus the only combination of quarks in mesons compatible with fewest parts is $q\bar{q}$, and it is this that was proposed.

[66]Mesons must contain both quarks and anti-quarks to produce the value $B = 0$.

[67]Zweig [1965], p193.

[68]That is, 'uniform division' can only apply in the case of baryons. Perhaps a better name for this principle is thus 'uniform division *wherever possible*'; but I shall just leave the name as it is.

Having established on symmetry and simplicity grounds the quark composition of baryons and mesons, we can deploy the SU(3) group theory once again to produce the resultant Clebsch-Gordan series:

$$3 \otimes 3^* = 8 \oplus 1;$$

$$3 \otimes 3 \otimes 3 = 1 \oplus 8 \oplus 8 \oplus 10,$$

$$3^* \otimes 3^* \otimes 3^* = 1 \oplus 8 \oplus 8 \oplus 10^*.$$

Thus the 'constituent quark model' predicts that baryons and anti-baryons will occur in singlets, octets, decuplets and anti-decuplets only, and that mesons only in singlets and octets. Exactly which hadrons this implicates – that is, exactly which *weights*, or combinations of I_z and Y, that we can expect to find in nature – can then easily be deduced using just the SU(3) representation theory and the corresponding theory of weight diagrams. (I note too that the exact quark content of each of the hadrons housed in these representations may be deduced in the process.)[69] Doing so reproduces both the low-lying meson and baryons multiplets known at the time beautifully – recovering, amongst others, the baryon decuplet topped off by the Ω^-. Indeed, forty years on from the postulation of the quark model, it remains true to say that 'all the states predicted by the quark model (at least with the quarks u, d and s) have been found, and no others'.[70] In terms of its ambitions to predict the SU(3) multiplets that are actually realized in nature, then, it seems that we can only affirm that the constituent quark model was an audacious success. It is therefore fair to say that the problem of multiplet prediction that was left to us by the Eightfold Way hypothesis was in this way essentially solved.

[69]See, e.g., Ho-Kim and Yem [1998], Chapter 7 for a detailed discussion, utilizing the Clebsch-Gordan coefficients, of how this is done.

[70]Ryder [1996], p11 . Likewise, a recent paper on the continued fruitfulness of contemporary uses of SU3 states that 'today we can group all experimentally known baryons into singlets, octets, decuplets and anti-decuplets ' (Guzey and Polyakov [2004]). To be clear, the current consensus is that the supposed 'evidence' for the existence of pentaquarks is little more than a statistical flunctuation; see Yao *et al.* [2006].

8.3 Lessons for a Structuralist Metaphysics

With this historical reconstruction behind us, let me sum up what has just been shown before moving on to consider the conclusions regarding the relations between symmetry structure and fundamental particles that may be drawn from it. After the SU(3) symmetry in the strong interaction was recognized, the 'Eightfold Way' hypothesis that all hadrons would occur in SU(3) multiplets was proposed. While that in itself was somewhat predictive insofar as it could predict the existence of new particles given prior knowledge of enough particles to almost fill up a multiplet, it could not in itself predict which multiplets would be realized in the first place. The 'constituent quark model' was then deployed to this end, which exploited the potential analogy with the 'building-block' role of the fundamental representations with respect to the other representations. The aim of this analogy was to allow us to infer which of the latter representations would actually be realized through considerations of which combinations of the former could be expected to obtain. This approach resulted in the postulated existence of new fundamental particles, which were denoted 'quarks'. We saw that the knowledge of the internal SU(3) group (interpreted as encoding the isospin and hypercharge) sufficed to determine the 'intrinsic properties' of isospin and hypercharge of these fundamental particles, but that the three 'simplicity' assumptions of *lowest eigenvalue*, *fewest parts* and *uniform division* had to be used alongside the $SU(2)_S$ and $U(1)_Y$ structures in order to determine their spin and baryon numbers. The combinations that these particles would occur in were simultaneously determined in the process, and thereafter precisely what hadrons one could expect to be observed was deducible through the SU(3) representation theory.

While *that* is clear enough, what is less clear at this point is whether, and how, this episode in physics supports structuralism and its signature priority claim. It is therefore to that matter that I want to turn now. Recall, first of all, that in this chapter I have elected to understand priority via supervenience and to understand the relevant structure in terms of symmetry structure. On this conception, to say that the structure is prior to the objects is to say that there can be no difference in the SIPs that exist without there being a difference in the symmetry structure; or, in other words, that the symmetry *determines* those particles. Now, the first and most obvious thing to say at this point is that it is not *purely* the SU(3) symmetry that determines all the relevant properties of the fundamental particles, nor of the composites they can create – in spite of Kantorovich's claims, noted above, that this 'symmetry dicate[s] via its representations the hadron spectrum'. While the internal group suffices to determine the internal properties of the fundamental particles, and determines that all the remaining particles must occur in SU(3) multiplets, it does *not* itself determine which hadrons those quarks will form, hence nor which multiplets will be realized in nature. (This, no doubt, is what Kantorovich means by his otherwise somewhat oblique remark that the SU(3) 'dictate is not absolute'.) However, and more relevantly for our purposes, even the extended group structure *SU(3)⊗SU(2)⊗ U(1) fails to determine* what hadrons we can expect to find in nature, for the simplicity assumptions we made at the various points were also crucial in determining these.[71] Had the same structure held but it not been the case, for example, that *fewest parts* held – so that baryons contained, say four quarks and an anti-quark instead of just three quarks – we would not expect to find only the 1, 8 and 10 representations. Rather, since

$$3 \otimes 3 \otimes 3 \otimes 3 \otimes 3^* = 35 \oplus 3(27) \oplus 4(10) \oplus 2(10^*) \oplus 8(8) \oplus 3(1),$$

many, many more particles would be expected to make themselves known in this case. Likewise, were it not the case that *uniform division* held, so that quarks could have $B = 1$, we would have for baryons

$$3 \otimes 3 \otimes 3^* = 15 \oplus 3 \oplus 3 \oplus 6^*,$$

so that we would again expect to find different hadrons from those we in fact do. It therefore seems right to say that the spectrum of SIPs supervenes not on the structure, *but on the structure supplemented by the simplicity assumptions*.

[71]Wolff makes a similar point when she states that it is unclear how one could say that relativistic particles, *qua* representations of the Poincaré group, supervene on the latter since the existence of the symmetry group is compatible with the existence of infinitely many different combinations of relativistic particles (see her [2011], p11). However, here – unlike there – we can try to append the supervenience claim to some simplicity principles in order to determine the particles. What the structuralist should say in the Poincaré group context needs, I think, a rather different treatment.

What should the structuralist say in the face of this fact? That is, to what extent does the fact that simplicity assumptions have to be made *in addition* to postulating the relevant symmetry if the spectrum of SIPs is to be determined undermine the core structuralist claim that 'objects supervene on structure'? It seems that there are two questions implicit in this. The first is whether these principles may be regarded as something sufficiently 'structural' as to be subsumable under the banner of structure, and thus to pose no threat to the claim. Should this be answered in the negative, there is then the further question of whether the fact that they cannot be so regarded is enough to deny structuralists of their thesis. Let us look at these questions in turn.

Regarding the first question, it seems correct to say that while it is admittedly rather difficult to spell out how these simplicity principles ought to ultimately be conceptualized – it is, for example, somewhat unclear whether to class them as merely methodological as opposed to ontological in character – it is nonetheless obvious that they cannot easily be regarded as *structure* in this context.[72] The reason, of course, is that in this chapter I have been taking the relevant structure to be a specific symmetry structure. But the principles of 'fewest parts' and 'uniform division' do not themselves make *any* reference to structure thus conceived, and we can understand them perfectly well in isolation from this structure. Thus, while it is palpably clear that such principles cannot be construed as *objects*, extending the structure so as to include elements that seemingly have *nothing to do* with group structure will have a distinct air of 'moving the goal posts'. I therefore do not think that this move represents a viable structuralist strategy.

Let us turn, therefore, to the second question – that of whether the need to appeal to these principles is damaging to structuralism, given that they cannot (or at least cannot obviously) themselves be regarded as structure. One response that has been made in the face of this question is that *any* physicalistic supervenience claim is going to be vulnerable to the objection

[72]Thus while it is true that Ockham's razor, for example, is usually construed as a methodological principle, it seems wrong to regard it *only* as such if we believe that nature somehow respects it (and thus is presumably of ontological import). (Here I am again thinking of the discussion of Musgrave in Chapter 3, which concerned his thought that simplicity principles – or at least those employed in empirical theories – could enjoy confirmation and thus plausibly be regarded as staking out features of the world, despite anti-realist claims to the contrary.)

that simplicity principles must be smuggled in somewhere if the supervenience claim is to go through, so that it is unduly harsh to hold this fact against structuralism.[73] Consider, for example, Lewis' claim that all supervenes on the fundamental level – a level that, moreover, physics will one day describe. Now let us grant that for any fundamental physical theory there will be countless empirically equivalent alternatives, and let us further grant, for argument's sake, that in the face of this fact the 'simplest' will in all cases be plumped for and subsequently canonized by physics. It therefore appears that Lewis' thesis that the non-fundamental supervenes on the fundamental level also invokes a simplicity principle, albeit implicitly, insofar as the *content* of that level is partially identified in terms of such principles. Thus, the claim goes, all physicalistic supervenience theses must make some appeal to simplicity assumptions, insofar as physics itself does. But if that is right, then this cannot be an objection to structuralism's supervenience thesis in particular.

But it seems that something altogether different is going on in the structuralist case in comparison with Lewis' – and that the differences are more problematic in the structuralist case. The first thing to note is that simplicity was not invoked in the case study discussed above to allow us to choose between empirically underdetermined theories, as it arguably may have to be in cases such as Lewis'. After all, as we just saw, if the simplicity assumptions had been different in this case, then different hadrons would have been *observed* in experiments. Simplicity is therefore not invoked by the structuralist in the above case study merely to choose between empirically underdetermined alternatives; rather, it is chosen to determine in advance what *will* be observed. Secondly, and furthermore, it is not at all clear that Lewis' supervenience claim is compromised by the fact that simplicity may have to be appealed to in order to identify the content of the supervenience base, since Lewis' claim is principally just that *given* that base, the rest of the world follows. Therefore how the base is identified in the first place, or what content it is identified as having, is of secondary interest for Lewis' purposes. But this is not the case for the structuralist, because the structuralist is not *just* making a physicalistic supervenience claim to the effect that the non-fundamental supervenes on whatever is fundamental in physics. Rather, they are trying to go further than that and *identify* what is fundamental in physics, and to do

[73]French, private communication.

so by showing that a supervenience relation holds. But in order to establish the existence of such a relation, we must invoke simplicity assumptions *in addition* to structures in order to determine those objects; and then we are back at the problem we started with – namely, that of deciding whether or not the inclusion of such non-structural assumptions undermines the structuralist claim. It therefore does not seem that the fact that physics may have to deploy simplicity principles when characterizing the fundamental is a problem for *all* supervenience theses that one might put forward in the metaphysics of physics, but it *does* appear to remain a problem for the structuralist thesis in particular. The difference, again, is that structuralism is trying to *identify* what is fundamental in physics by means of a supervenience claim, not merely state that *given* the fundamental, all supervenes on that. Since the simplicity principles are in each case involved in characterizing the supervenience base, the fact that we may have to enlist them seems to be a problem for structuralist supervenience only.

So let us think again about how the structuralist might deal with the presence of the simplicity principles in the analysis, given that this presence cannot simply be dismissed as a problem for everyone (and hence, in a sense, for no-one). One response that the structuralist might be tempted to make is that the core structuralist claim – at least when interpreted in supervenience terms – should be changed to the claim that it is structure *plus simplicity principles* that objects supervene on. However, I think that would I think not only be somewhat *ad hoc*, but undesirable in other respects too. Structuralism is, after all, supposed to be a general metaphysical thesis about the fundamental category (or categories) of the world, and one that will be established by reference to specific, paradigmatic cases. However, the simplicity assumptions governing the construction of hadrons apply only to a proper subset of reality, and so building these principles into the core claim of structualism *in general* thus seems very unwise. It seems to me that it is better that structuralists keep the core claim sufficiently general, but append it with the qualification that other elements that do not fit neatly into the category of structure may have to be added in specific contexts, keeping the specification of those extra elements for the discussions of those contexts (though we should insist that if the doctrine is not to be utterly trivialized, those extra elements cannot be the objects themselves).[74]

It therefore seems that the fairest assessment of this episode's lessons for structuralism is that the claim that the SU(3)⊗SU(2)⊗U(1) structure determines the strongly-interacting particles *must be carefully stated and qualified* if it is to go through at all. My instinct at this point, however, is that the fact that these qualifications must be made does not significantly denigrate the basic structuralist proposal – namely, its 'core claim' that objects supervene on structures. After all, it seems right to say that were the 'object-oriented' realist to dismiss the idea that there is a profound sense in which structure is prior to objects on the grounds that we must also assume that the number of parts in a composite is minimal or that matter particles do not contain unnecessary anti-matter, etc., then they would be shirking from the structuralist's challenge.[75] At the moment, however, I will not offer any very principled reason as to why we should deny that the fact that the claim does not go through without those qualifications means that it does not go through *simpliciter*. While this is, of course, to some extent a merely terminological question, I will leave the general significance for structuralism of the fact that we may always have to make similar but context-dependent qualifications as a matter

[74]Picking up Wolff's criticism mentioned in passing a moment ago, another example here may be Wigner's group-theoretic construction of relativistic particles through the representation theory of the Poincaré group. The problem here is that while Wigner's classification succeeds in predicting what (combinations of) *properties* relativistic particles must have, it permits infinitely many such particles and does not determine which of these will actually be realized in nature. But in this case, it seems that the appropriate thing for the structuralist to do is to regard the extra information that must be added as a sort of *initial condition*, and to deny that the fact that we need to add initial conditions to the structure in itself denigrates the claim that structure is ontologically fundamental – any more than the fact that we have to add initial conditions to Newtonian mechanics denigrates the idea that it determines the trajectory of classical particles.

[75]It is, after all, not as if the simplicity assumptions that must be added to the structure have an air of being suspiciously *ad hoc* or convoluted. As one textbook said of the idea that baryons are qqq composites and mesons $q\bar{q}$, and thus of (what I have called) 'fewest parts' and 'uniform division': 'The simplicity of [these assumptions] is attractive; a puzzle arises because the assumptions seem oversimplified, yet to correspond to what is observed experimentally (Cheng and O'Neil [1979], p334). But that the assumptions that must be added to the model in addition to the group structure seem *so* uncomplicated is something that the structuralist will instinctively draw strength from. Presumably the *more* convoluted the principles that have to be added to the structure for it to determine what they want, the less convincing is the proposal that structure has a claim to being a fundamental category.

for another day.[76] I therefore close this section by simply reiterating that I believe that the core claim should be taken to go through in this instance, *modulo* the above qualifications.

8.3.1 Radical or Moderate Structuralism?

In the previous section, we saw that there are good grounds for a qualified version of the core structuralist claim that strong-interaction symmetry structure determines the strongly-interacting particles. If one understands priority in terms of supervenience, then, there are strong grounds for saying that structure is *ontologically prior* to the particles that the structuralists protests that metaphysics presents as (uniquely) fundamental. But since I hold that priority relations may be symmetric, we have not yet thereby shown that this structure is *more ontologically fundamental* than these objects.[77] If it should turn out that the determination is reciprocated, the best the structuralist can claim is that the category of structure is *as* fundamental as that of objects. In such a case, of course, the *moderate* position would be vindicated at the expense of the radical one. By way of closing this chapter, I now want to consider whether or not these determination relations may be said to be reciprocated, and thus which of the radical and moderate refinements is recommended to us by this episode.

The question we are asking is whether we can say that the SIPs determine the symmetry structure – or at least, since we saw that the determination of SIPs

[76]I note that it is difficult to find any particularly detailed discussion of this issue in the structuralist literature. French, for example, notes in a survey article that 'whether the introduction of "non-structural elements" undermines the structuralist tendency is a tricky issue, depending, of course, on both the nature of the element and the form of structuralism adopted' ([2006], p176); but I can find nothing in the literature that discusses this 'tricky issue' at any length. Indeed, while French and Saatsi discuss some specific allegations regarding the imposition of allegedly non-structural elements into structuralist theories – such as the postulation of a linguistically specified natural kind structure – they close by noting that 'how exactly such a notion [of ' "extra" content going over and above pure structure'] is to be developed is still an open question' ([2004], p26). That seems to me to still hold true today.

[77]To recap yet again, I do not take x's being prior to y to be sufficient for x's being *more fundamental than* y; I take x to be more fundamental only if, in addition, y is not prior to x. If y *is* also prior to x, I say x is *as fundamental as* y.

by symmetry was not total, that the SIPs determine the symmetry structure to the same degree as was the case there. Recall from Section 2 above that when we say we are interested in symmetry structure in particle physics, what we are usually interested in is the Lie algebra corresponding to that symmetry (since it is this that does most of the work in defining particles in particle physics). Recall too that we were restricting our attention to the semi-simple Lie algebras (with the exception of U(1)), but that what was sought from the outset of the Eightfold Way was a *simple* Lie algebra, since it is only simple Lie algebras that correspond to 'higher' symmetry schemes that can appropriately enmesh the multiplets corresponding to lower symmetries. The question we are interested in is whether we can use the properties of the SIPs to determine the simple algebra that governs their internal properties, and those algebras that govern their external properties. As before, we regard the SIPs to be defined in terms of their I_z, Y, B and S alone.

So let us suppose, for argument's sake, that we somehow had epistemic access to the SIPs, including the quarks, quite independently of any group structure.[78] That is, let us assume that we can take them out of the box and examine them, as it were, and thus come to know their I_z Y, B and S properties, their total number, as well as the fact that the hadrons are composed of the quarks (and thus that the latter are fundamental relative to all the other strongly-interacting particles). Suppose we consider the quarks alone first of all. They all have spin 1/2, but the quarks and antiquarks will differ in their values of B. So suppose we then separate them into two sets, one composed of the three quarks, the second of the three anti-quarks, and plot the I_z and Y values of each member of each set on one of two I_z and Y axes. In so doing, we reproduce the iconic quark diagrams reproduced Figure 8.2. Being highly symmetric, these will (of course!) look like the weight diagrams corresponding to irreducible representations of a symmetry group, and so suppose we then understand them as such. Furthermore, given that we have agreed that we have 'access' to the fact that these particles are fundamental particles, then for the same reasons as offered before in Section 2 we have some justification to understand these to be the weight diagrams of the *fundamental* representations of some algebra. So the question we must ask

[78]After all, I am considering structuralism as an *ontological* thesis; even if we could not have any such access, even in principle, that is not to the point here.

ourselves now becomes: may we deduce the simple Lie algebra corresponding to the internal symmetry that – as we saw – could determine the I_z and Y of these particles, *from* the set of these particles?

But it is straightforward to show that the answer to this is *yes*: given the same particles that the SU(3) structure determines, we can work backwards to recover the SU(3) algebra. This may be seen as follows. We have agreed to interpret the particles featuring in Figure 8.2 as the weight diagrams of some algebra's fundamental representations. Thus each quark is construed as a *weight* $|\mu\rangle$, given by

$$H_i|\psi\rangle = \mu_i|\psi\rangle,$$

where the H_i represent the I_z and Y operators. We also know that weights of any representation are related to one another by the action of the *step operators* via (8.6):

$$H_i(E_\alpha|\mu\rangle) = (\mu + \rho(\alpha))_i(E_\alpha|\mu\rangle),$$

where the *root vectors* $\rho(\alpha)_i$ are defined by (8.4) above. It was also mentioned in Section 2 that the set of these root vectors can be plotted to obtain the root diagram, which (i) consists of all the roots of the algebra, (ii) is unique to the algebra and (iii) suffices to determine it. However, it turns out that the algebra may be classified in a more compact way – namely, in terms of the *simple* roots. The simple roots, ρ'_i, have the property that their linear combinations can produce any other root of the algebra, and for a simple rank l algebra there are l simple roots. It turns out that all the information about the root diagram of a simple Lie algebra as a whole may be deduced from these simple roots and the relationships between them. In particular, all information about the algebra may be deduced from the relative magnitudes of the simple roots and the angles between them – both of which turn out to be severely constrained. Thus, all the essential information about an algebra is captured in its *Cartan matrix*:[79]

$$C_{ij} = \frac{2\rho'_i.\rho'_j}{\rho'^2_i}. \tag{8.8}$$

It is this matrix that forms the basis of Cartan's classification of all the semi-

[79]See, e.g., Cornwell p221; Jones p182.

simple Lie algebras.[80]

The relevance of these facts to the structuralist priority claim is as follows. As already mentioned in Section 2, the *fundamental representations* (such as the 3 and 3* of SU(3)) are defined as those representations whose highest weight is a *fundamental weight*.[81] A fundamental weight, Λ_i, is defined via its orthogonality to the simple roots – that is, via

$$2\frac{\Lambda_i . \rho'_j}{{\rho'_j}^2} = \delta_{ij} \tag{8.9}$$

where ρ'_j is a simple root. Since a simple rank l group has l fundamental representations, it has l fundamental weights. But it is now clear that from a knowledge of the fundamental particles – which we are viewing as the fundamental *representations* – we can recover the algebra. For, given these l fundamental representations (so in the case of SU(3), all the quarks and antiquarks as depicted in Figure 8.2), we can plot the particles, read off the highest weight in each of the l representations and plug them into the equations (8.9) for the simple roots.[82] These are l equations in l unknowns, which can thus be solved for the simple roots; these simple roots then determine the Cartan matrix, which in turn determines the algebra. Therefore given a complete set of fundamental particles – construed as a set of fundamental irreducible representations corresponding to some simple algebra – we may work backwards from it to reconstruct the corresponding Cartan matrix which in turns determines that algebra. It therefore appears that just as the SU(3) structure unambiguously determined the I_z and Y properties of these particles, they in turn determine *it*.[83]

Let us now consider whether the spin and baryon properties can be used to determine the corresponding SU(2) and U(1) structures. Now, we know the quarks have $S=1/2$, and *lowest eigenvalue* postulated that the quarks should

[80] Cornwell [1984] p523. It may be useful to note that is the Cartan matrix that is expressed in a Dynkin diagram, which is often used in particle physics to represent an algebra (where 'represent' is not meant in the homomorphism-onto-operators sense).

[81] See Cornwell [1997], p242. (Note that Lichtenberg [1978], p86 refers to these as the 'fundamental dominant weights'.)

[82] Though I haven't mentioned it, both the weights and the roots of the algebra can be ordered, and must be ordered by the same convention for this formula to work.

[83] See also McVoy [1965], p103 for an alternative deduction of the SU(3) algebra from the two triplet representations.

be put in their fundamental representation on account of their fundamental nature. But then since that representation is one-dimensional – being composed of just the S_z=1/2 and S_z=−1/2 weights – then we know that the algebra sought is rank one. But there is only one such semi-simple Lie algebra, and that is the algebra of SU(2). Thus all we need to consider now is whether we can deduce that their baryon number group is given by U(1). Recall that before, we assigned $B = 1/3$ to the particles by assuming *fewest parts* and *uniform division*, together with the fact that the U(1) group has additive eigenvalues. What we have to go on here are just the fact that the (anti-)quarks have $B = 1/3$ ($B = -1/3$), (anti-)baryons $B = 1$ ($B = -1$), and the fact that there are three (anti-)quarks in a (anti-)baryon. In looking for the relevant group, then, we need a unitary group whose irreducible representations have the property that $\Gamma_b.\Gamma_b.\Gamma_b = \Gamma_{3b}$, and hence whose representations have the form $e^{iB\theta}$. But these are just the representations of U(1).

By surveying the properties of the SIPs and the same compositional relationships that the simplicity assumptions were designed to capture, then, we can show that the determination *goes both ways*. That is, just as the SIPs cannot vary without varying the structure and the simplicity principles, so that structure cannot vary without also varying that SIPs and those principles. Thus while the prediction of the properties of the quarks and hadrons through symmetry structure should be taken to be a triumph of structuralism in philosophy of physics, we should be clear that it is a triumph for the *moderate* stance, not the radical stance. Thus, insofar as priority is understood in supervenience terms, it is *moderate structuralism* that represents the right philosophy for this beautiful episode in particle physics.[84]

8.3.2 A Reply by the Radical Structuralist

At least one radical structuralist has, however, objected to the idea that the above argument shows that the moderate position is vindicated, and thus de-

[84]To be clear, however, the route taken here *is not* that taken in the moderate structuralism of Esfeld and Lam (*op. cit.*), in which the reciprocated priority of objects over structures was argued for just on the basis of an insistence that 'relations require relata' on intelligibility grounds (and unsuccessfully in consequence). Rather, this reciprocated claim was established by attending to how objects and structures are conceived *in the physics itself*.

nied that the radical position undermined through the above considerations.[85] As French quite rightly points out, the above demonstration that particles can determine structure rests on the assumption that we should conceive of particles in terms of the representations of symmetry groups – namely, as basis vectors in their irreducible representations, or equivalently as members of *multiplets*. After all, had we *not* made that assumption, we would have no idea how to deploy the representation-theoretic machinery needed to determine the relevant algebras. But according to French, once we have made that assumption, we have conceded exactly what the radical structuralist wanted us to concede all along, since group-theoretic representations are *themselves* to be regarded as part of the structure. Thus, the claim goes, by conceiving of particles in such terms we have implicitly embraced the radical stance; the conclusion that it is the *moderate* stance that is vindicated therefore cannot be right, since the two contradict one another.

There is clearly a fair bit to unpick here, so let us try to see what is going on. This view that the representations are themselves to be regarded as part of the structure is certainly a view that French has expressed elsewhere. For example, he writes in a recent collection on the present state of structuralism that metaphysical reflection upon the role of symmetries in quantum physics reveals that

> the putative objects are presented and re-conceptualised (and hence metaphysically eliminated *qua* objects) via group-theory and it is the particularities of the latter's representations (in the technical sense) that reveal, represent and present to us the concrete features of the structure of the world.[86]

French thus seems to be saying here that the group-theoretic representations are to be regarded as part of the structure, and since we may take the categories of objects and structures to exclude one another, this then precludes us from regarding them as objects. The principal relevance of this to present concerns is that it is therefore not particularly *surprising* that we should get the two-way priority relation between symmetries and representations that I derived above, since identity is, of course, a symmetrical relation. The space is thus cleared for the claim that the existence of symmetrical priority relations

[85] French, personal communication.
[86] French [2012], p26.

between symmetry structures and particles does not in fact undermine the radical point of view, on the grounds the symmetry of priority arises precisely because there *are* no objects. Since the latter is a big part of the radical package, this symmetry of determination in fact supports it.

But this claim that particles cannot be regarded as objects on account of the fact that we understand them in group-theoretic terms is, to my mind, a very puzzling move. Note first of all that, if we make the assumption (as in the case study outlined above) that there exists a symmetry in the dynamics pertaining to some particles, *it is just a fact about the quantum-mechanical formalism, and the mathematics of symmetry,* that those particles will fall into multiplets. As such, no real *choice* was involved in conceiving of particles in terms of representations. Thus were French to be right about the idea that there are no objects in (this regime of) particle physics on the grounds that they are conceived of in such terms, then it would follow only from (i) incontrovertible facts about the mathematics of quantum mechanics that no naturalistic metaphysician could ever be in a position to deny, plus (ii) what is, at least so far, a purely semantic decision not to regard particles as objects once they are connected with the concept of representations. But if that is the case, it is not clear what there is left for naturalistic metaphysicians to argue about – something that seems very suspicious, since I think we can safely assume that Esfeld and Lam are likewise committed to naturalism. Some argument must therefore be given for the second assumption that we cannot categorize an entity as an object once we conceive of it in terms of representations. But even if that argument were to succeed (which I do not think it can), it remains hard to see how French could consistently maintain, in the face of my argument above, that particles are thereby to be regarded as aspects of *structure*. After all, my argument set out to determine whether the radical structuralist claim that objects are ontologically secondary to structures could be defended, and in order to do that I had, of course, to *identify* examples of each at the outset. In this instance, I selected the first three quarks as our examples of objects and the global SU(3) flavour structure, supplemented with the spin and baryon number groups, as the example of structure; I then showed how the SIPs were determined via this structure and *vice versa*, in a way that seemed to undermine radical structuralism. How, then, can a radical structuralist ask me to renege on the idea that these particles are objects in the face of this conclusion, without being guilty of special pleading?[87] And even if they were to find a way to do so in good conscience, how could they then possibly claim that objects are secondary to structures, *if we don't even have any objects left in the picture?*[88]

French's claim that being conceived of in group-theoretic terms is incompatible with being regarded as an object is thus deeply confusing from all sorts of dialectical angles, and seems – on the face of it at least – to render radical structuralism incoherent. However, French's view appears to be an idiosyncratic one, since many other prominent structuralists do not seem to take this line. While it *is* commonplace for other structuralists to similarly claim that objects are 'eliminated', that claim more usually issues from the assertion that objects are *not metaphysically fundamental* than from the view that the category has simply been wiped out of the picture. As Ladyman and Ross put it, for example, 'this is the sense in which our view is eliminative; there are objects in our metaphysics but they have been purged of their intrinsic natures, identity, and individuality, *and they are not metaphysically fundamental*'.[89] Thus, in the particle physics context, they point out that 'objects are very often identified in terms of group theoretic structure' and go on to note approvingly that, as a result, both of the structuralists Kantorovich and Lyre have held that particles are ontologically secondary to those structures.[90] Thus it is seemingly not the case that all structuralists take the fact that particles are conceived of in terms of group-theoretic representations to rob them of their status as objects. Rather, the claim more usually offered is that objects *are to be identified* in such terms, and regarded as ontologically secondary in consequence. And on Ladyman and Ross' view of eliminativism, it is only on that account that they are to be 'eliminated'.

Thus, while the word is the same, it is crucial to note that this is a very different sort of eliminativism claim than that which French invokes above,

[87]To put it differently, given that we started off with the assumption that the relevant structure is the SU(3) group – or better, the algebra of its generators – how can we say that the vectors in the *weight diagrams* are also part of the structure without being accused of 'moving the goal posts'?

[88]This point has also been made, amongst others, by Psillos. As he writes, 'if structure is all there is, what are they said to be ontically prior to?' (Psillos [2012], p171).

[89]Ladyman and Ross [2007], p131; italics added.

[90]*Ibid.*, p145.

and we must be careful to keep them separate.[91] There is, on the one hand, the eliminativist view seemingly expressed by French above in which 'object' turns out to be a failed natural kind term – perhaps akin to 'phlogiston'. On the other hand, there is the view that there *are* objects in particle physics but only in a metaphysically secondary sense – perhaps akin to the way that what were previously thought to be *sui generis* chemical forces are now understood to be reduced to physical forces. I have argued that the first notion of eliminativism is dialectically problematic and – on the face of it at least – even incoherent from the point of view of radical structuralism, and have noted that other radical structuralists adopt instead the second point of view. I thus propose that we *reject* the view that particles cannot be regarded as objects on the grounds that they are conceived of in terms of the representations of symmetry groups, and instead retain the view that we *do* have objects in particle physics, whilst acknowledging that they are 'constituted' group theoretically.[92] We can then coherently ask the question whether they can be 'reductively' eliminated *qua* ontologically secondary entity.

Now, on the basis of the considerations I have adduced in this chapter, the answer to that question of course appears to be *no*, since – from the point of view of supervenience at least – the representations, and hence the particles, seem to be on a par with the symmetries that the structuralist takes as fundamental. Whether that ultimately vindicates the moderate form of structuralism, however, is going to depend on whether supervenience really is the right relation with which to express priority in structuralism. That is something that I want to consider now.

[91]Though, confusingly, French also invokes the 'elimination *qua* secondary' position in places! Analyzing priority in dependence terms, he writes elsewhere that 'our putative objects only exist, in a sense, if the relevant structure exists and the dependence is such that there is nothing to them – intrinsic properties, identity, constitution, whatever – that is not cashed out, metaphysically speaking, in terms of this structure... This yields what has sometimes been called the more 'radical', but as I would prefer to call 'strong' eliminativist form of OSR' ([2010], p106).

[92]This terminology is from Castellani [1998]. Some further implications of the fact the objects are conceived of in particle physics in group-theoretic terms will be developed in the next chapter.

Chapter 9

Structure as a Fundamental Category 2: Structuralism as a Dependence Thesis

9.1 The Right Priority Relation for Structuralism: Supervenience or Dependence?

In the last chapter, I claimed that it is the moderate position that is best recommended to us by the particle theories based around the SU(3) flavour symmetry, and justified this claim by means of an argument in which priority was interpreted in supervenience terms. Insofar as the structure involved in this episode is held up as paradigm example of structure in physics, the natural conclusion to draw from that argument is that the radical position is untenable as a general thesis in the metaphysics of physics. However, showing that radical structuralism is false on one construal of priority need not be taken as showing that it is false *simpliciter*, provided there is scope to argue that some other priority relation is somehow more apt for structuralism. But we are now in a better position to consider which, if any, is the best-suited relation for the priority claims that structuralism proposes than we were when I introduced structuralism in the beginning of the last chapter. There, my characterization of structuralism was very shorn-down, and the position was presented just in terms of its commitment to certain priority claims. But clearly such a characterization of structuralism cannot itself *adjudicate between* what is and is not the best priority relation for it (beyond, of course, that it must be able to relate different categories). It is therefore worth considering, with the benefit of hindsight, whether there is a better choice for expressing the priority claims definitive of structuralism than relations of supervenience. So let us consider that now.

One thing that became clear in the last chapter was that, in order to infer either of the supervenience claims that were established there, it was necessary to conceive of objects in group-theoretic terms – namely, in terms of their being basis vectors in irreducible representations of symmetry groups, or in other words members of multiplets. Without that assumption, there is no fathomable way to get the objects and structures 'talking' to each other so as to be able to deduce the one from the other. Furthermore, as was noted toward the end of that chapter, the act of conceiving of particles in terms of group-theoretic representations was not something that involved any real element of choice (given the assumption that there is a symmetry in the relevant dynamics); nor did it seem to warrant ceasing to regard the particles *as objects*. Nonetheless, it seems incontrovertible that the concept of object *qua* basis vector of a group-theoretic representation is very *intimately related* to the corresponding notion of structure, since the former notion cannot even be *defined* without reference to that structure. But given that objecthood now appears to be (at least partially) *defined in structural terms*, it also seems intuitively right to say that this conception of objecthood has implications that render it markedly different from those more typically presented in metaphysics, and as such that this conception of objects in modern physics is potentially highly *revisionary* with respect to received views on the nature of objects.

To appreciate the latter point, consider for example either of the 'bundle', or 'substratum'-based views of objects familiar from analytic metaphysics.[1] Simply speaking, we may say that each of these characterizes what it is to be an object in terms of how it is that it possesses properties (namely, in terms of their being 'bundled' together by a *sui generis* bundling relation or in terms of their inherence in a substratum). Thus presumably what it is to be a *physical*

[1]Whether the 'bundles' are taken to be of universals or tropes is of no consequence here.

object on either construal is conceived of in terms of how it is that physical properties are possessed. But what those properties are, beyond of course the fact that they are physical, is of no consequence to either account. As such, neither has anything to say on how the properties of an object might have to *relate* to one another in order to constitute a physical object – something on which the group-theoretic approach, by contrast, has a great deal to say.[2] Furthermore, each of the more familiar characterizations articulates what it is to be an object without in any way implicating the existence of *other* objects.[3] But since conceiving of an object in terms of its being a member of a multiplet will, in the general (i.e. non-trivial) case, imply the existence of other objects – recall here the prediction of the Ω^- – this again seems to stake out a profound difference with the more familiar views.[4] In sum, then, the group-theoretic conception seems to involve certain inter-object relationships, and constraints on combinations of properties, that are simply not present in the more familiar conceptions.

As a result of these apparently profound differences between the group-theoretic conception of objects and the more common-or-garden varieties, one often finds structuralism's priority claims being accompanied by a claim regarding the *reconceptualization of objects* that the shift to a structuralist perspective involves.[5] To take just one example, French and Ladyman state

[2]For example, the fact that particles are regarded as basis vectors in an irreducible representation of rank l algebra implies that each has l determinable properties that can be observed simultaneously, and has very specific implications regarding the corresponding determinates that may be observed in each particle, via the weight diagram structure.

[3]Thus while Paul's conception of an object as a 'bundle of n-adic properties' (see Paul [forthcoming]) might imply the existence of $n-1$ other objects in certain cases (if, for example, there are *irreflexive* n-adic properties involved), her general conception is in no way *committed* to the existence of other objects.

[4]More precisely, in *some* contexts we take an object to correspond to a basis vector in an irreducible representation, and in others we take an object to correspond to the whole representation. The 'Eightfold Way' case was clearly an instance of the former; the case of the constitution of objects via the representations of the Poincaré group, to be discussed below, will be a case of the latter.

[5]For further discussion of how the group-theoretic conception of objects differs from conceptions more standardly assumed, focussing on how modal debates are thereby changed, see McKenzie [forthcoming]. I should, however, perhaps underline that here I am most interested in considering which priority relation is most appropriate for structuralism, as structuralists themselves see the latter. Insofar as structuralists evidently *do* regard objects to be 'reconceptualized', and insofar

that as structuralists they 'are not "anti-ontology" in the sense of urging a move away from electrons, elementary particles etc. [...] rather, [they] urge the reconceptualization of electrons, elementary particles and so forth in structural instead of individualistic terms.'[6] What I therefore want to reflect on now is how structuralists ought to conceive of priority, given that a novel priority claim *and* a claim regarding the 'reconceptualization' of objects are both parts of the structuralist package.

As mentioned in the last chapter, in addition to supervenience a suitably general notion of dependence is a *prima facie* good candidate for expressing structuralism's priority claims, given its ability to relate entities of different categories. And as was also mentioned there, structuralists have used both, at different points, to trace out their metaphysical views. Ladyman and Ross, for example, write that

> OSR is the view that the world has an objective modal structure that is ontologically fundamental, in the sense of *not supervening* on the intrinsic properties of a set of individuals.[7]

This demonstrates that structuralists sometimes vouch for supervenience tò express their priority claims. French, on the other hand, more often alludes to relations of dependence to articulate his structuralism, stating that he

> take[s] it that a core feature of OSR is the claim that putative 'objects' are *dependent* in some manner upon the relevant relations (and hence these putative objects can be reconceptualized as mere nodes in the relevant structure).[8]

(Note another allusion to 'reconceptualization' here.) Given that each of these are *prima facie* good candidates and both in play in the literature, it is therefore natural to construe the question of which relation is most apt for analyzing structuralist priority as that of which, if either, of these two

as the latter may be argued to be relevant to the priority analysis, precisely *how* structuralists think this reconceptualization should best be cashed out is a somewhat peripheral concern for my purposes. But I take it that structuralists will agree that the above sorts of considerations are at least important for what they mean by 'reconceptualization'.

[6] French and Ladyman [2003], p37. See also e.g. French [2006]; Bokulich [2011], p.xiv; Pooley [2006]; Brading and Skiles [2012].

[7] Ladyman and Ross [2007], p130; my italics.

[8] French [2010], p104; my italics.

relations fares better. And since – as was pointed out in Chapter 1 – not only are the two conceptually quite distinct but also arguably not co-extensive, this question is certainly one that can be meaningfully asked.

Let us therefore contemplate, for a moment, how the two relations compare. I have already mentioned that supervenience is very familiar in metaphysics as a means of cashing out priority, and it is also deemed to be amply clear (at least insofar as modal concepts are deemed to be clear – a big qualification, to be sure). But the idea that supervenience can be used to cash out priority has, of course, also had its critics. Such critics often focus on the idea that supervenience is not at all *explanatory* of any relationship between the sub- and supervenient relata, so that supervenience is often regarded as at best an indication that it is worth *looking* for an explanation of the evident connection between them, while not itself explanatory of it.[9] That need not be a criticism in itself, of course – one can in fact imagine situations in which it might be positively advantageous, such as when it is believed that there is no such explanation to be had.[10] But insofar as it posits a relation between things without giving us any indication of why it holds, it may be difficult to stomach the idea that supervenience gives us the last word on fundamentality.

On the other hand, it is fair to say that the predicament with dependence has been almost the diametric opposite: while dependence is taken to have some deep connection with explanation, it has frequently not been seen as sufficiently clear. As evidence for this, one could cite the fact that Lewis – in one of his many papers attempting to define intrinsicality – commented that 'if we had a clear enough understanding of "in virtue of" [i.e. dependence], we would need no further definition of intrinsic'.[11] But it seems to be the case that philosophers have grown more sanguine about dependence in recent years, and that it is increasingly regarded as something of which we have a good enough working grasp. (This is indeed seemingly evidenced by the number of new dependence-based analyses of intrinsicality that are now on the market.)[12] Whatever it is that accounts for this change, if there *are* good reasons to be more sanguine, it appears that dependence – unlike

[9]See Kim [1993], especially pages 146, 148 and 156, and works cited therein.
[10]Emergentists of various stripes believe that there are such cases.
[11]Lewis [2001], p384. See also Lewis [1983], p29.
[12]On the defence of dependence, see Jenkins [forthcoming]. On dependence-based analyses of intrinsicality, see Rosen [2010], Witmer *et al.* [2005].

supervenience – gets 'a tick in both boxes'. Given, then, that it is increasingly regarded as both sufficiently clear and appropriately explanatory, we might want to endorse it on these grounds.

Whatever virtues either relation might have in the abstract, however, the principled question we should be asking in this context is which is the more pertinent relation *for structuralism*; and I think a strong case can be made that it is *dependence* that represents the analytically more fundamental relation for structuralism's priority claims.[13] I will argue for this in two steps. Step 1 will be to motivate the idea that claims that express any priority facts embodied in the very fact that objects are structurally reconceptualized should be regarded as more basic to structuralism's priority theses than those that must simply *assume* that that reconceptualization has taken place. Step 2 will be to argue that the priority relation most appropriate for capturing any such priority facts is a version of dependence.

Let me therefore start with step 1. Recall first of all that, as pressed at the start of this section, structuralism comprises more than just a priority claim: it is also an invocation for us to *reconceptualize* object-based ontology in some way that integrates structures. It is in that sense revisionary with respect to the *nature* of objects as well as to their supposed priority. Now consider once again the reciprocated supervenience claim that was obtained in the last chapter – namely, that of the supervenience of symmetry structures on objects. As already pointed out, that claim could go through only given the antecedent assumption that the objects *were conceived of in group-theoretic terms*. But it seems eminently plausible that there may be priority facts *embodied in* the very fact of this 'reconceptualization' that are distinct from the priority facts that *follow from* this reconceptualization – in much the same way as there may be modal assumptions that are embodied in a particular account of lawhood that are distinct from, but entailing of, modal facts regarding the behaviour of objects. So suppose we provisionally accept that possibility for now.[14] It furthermore seems plausible that any priority claims that may be 'embodied in' the fact of reconceptualization should be regarded as *more basic* to the structuralist analysis than the supervenience claims that were

[13]This conclusion is in addition to the claim, noted in Chapter 2, Section 2, that supervenience is increasingly viewed as insufficiently fine-grained capture concepts related to priority and fundamentality.

[14]This assumption will be justified in due course.

derived in the last chapter, since the latter *must take for granted* that such a reconceptualization has taken place in order to even get off the ground. Let us therefore accept that as well.

Now let us turn to step 2. To recap, step 2 is to make plausible that it is a version of dependence that is the most appropriate candidate for articulating any priority claims that might be embodied in the fact of reconceptualization. As noted, this reconceptualization is intimately related to the fact that the objects of particle physics seem to be (at least partially) *defined* in structural terms. As a first stab at making this plausible, we might approach the issue negatively: since it is only supervenience and (some version of) dependence that we have in the running, and since it seems that supervenience claims regarding the relationship between objects and laws must already *assume* that objects are reconceptualized group-theoretically, if there are priority facts *embodied in* the very fact of reconceptualization then it seems we can only conclude that supervenience does not have the resources to analyze them (as opposed to merely encode some of their consequences). But, more positively, there is an approach to dependence that seems to be tailor-made for analyzing such priority facts, and it is the approach to ontological dependence that is taken by Kit Fine. To see this, recall first of all that the 'reconceptualization' at issue here consists of a shift in perspective with regard to the *nature* of objects in comparison with more familiar metaphysical analyses: after all, what divides these analyses is not the properties that such objects have, but what their nature is *qua objects*. Recall secondly that what motivated the idea that objects are to be reconceived structurally was the fact that they are now *defined*, at least in part, in terms that make reference to symmetry. What we therefore want is an analysis of priority that can locate novel priority facts in novel facts about nature, where these facts about nature are intimately related to facts about definition. But if that is the case, then Fine's analysis of ontological dependence presents itself as ideal on both counts. Fine's analysis is, for one thing, *explicitly predicated* upon the idea that priority attributions should in all cases 'be appropriately tied to the nature of the dependent item', and thus that they should go deeper than, for example, the modal profile of the item alone.[15] But secondly, for Fine the question of what an object's nature is *just is* the question how it should best be defined, so that questions

[15] Fine [1995a], p272.

of priority will, on this account, be automatically invited by questions of definition. For both of these reasons, then, Fine's analysis appears to be an ideal resource for capturing any priority claims that might be embodied within structuralism's stated reconceptualization of objects.

If that two-step argument is cogent, then there is a strong case to be made that it is Fine's conception of ontological dependence that represents the most fundamental priority relation, analytically speaking, for structuralism's priority claims. And if *that* is correct, then the radical structuralist is, of course, back in with a fighting chance, since they can claim that the priority claims deduced previously had to *take for granted* certain facts which may yet play into their hands.[16] What I therefore think we ought to do at this point is put the talk of supervenience behind us, and instead take a closer look at what exactly is involved in the account of dependence to which I just alluded. With that in place, I will address how structuralists might explicitly invoke Fine's account to promote into real *demonstrations* what in many cases have, in the absence of a clear and unambiguous understanding of what is meant by 'priority' (or even 'dependence'), largely just been plausibility arguments for the priority of structure over objects (and, where appropriate, *vice versa*). In the second of these case studies I will resume the discussion of group-theoretic structuralism, and see how the priority claims that can be made in that context look once we conceive of priority *a la* Fine. But to broaden things out a little, the first case study concerns a different sort of structure that is also highly prominent within the structuralist literature, and it is that pertaining to *entangled quantum particles*. The latter is in fact perhaps the most vaunted and most discussed case of priority in structuralism, but complaints have nevertheless been lodged that it is unclear how exactly it is the relevant priority claims are ultimately supposed to work in this context.[17]

[16]Indeed, this observation may provide us with a more convincing reading of the claims made by French described late in the last chapter. While we can (and I believe should) deny that the act of conceiving objects group-theoretically warrants ceasing to consider them as genuine objects, it may be that this reconceptualization introduces dependence relations that exhibit the radical structuralist's supposed asymmetric structure. *If* that is the case, then the very fact that the moderate structuralist's supervenience claims presuppose this reconceptualization means that the radical can happily accept that moderate's supervenience claims but nonetheless deny that they express the most fundamental facts about priority in structuralism.

[17]Here I recall Hawley's criticisms mentioned in the last chapter.

Showing how those claims can be clarified when viewed through the prism of Fine's analysis will thus do nothing if not testify to the advantages of explicitly adopting this approach. So with that, let me now expand a little more on ontological dependence and, in particular, Fine's account of it.

9.2 Introducing Ontological Dependence

If we go with our first intuitions on the matter, we will want to say that an entity x is ontologically dependent upon an entity y just if x *exists only if* y *exists*. It seems clear that this should also hold with metaphysical necessity (for if there were a world in which x could after all exist without y, we would presumably want to retract that x was dependent upon it). To a first approximation, then, we may say that an entity x is ontologically dependent upon an entity y just if, necessarily, x exists only if y does, where the force of the necessity is metaphysical. This indeed represents the core of the analysis of ontological dependence to be found in Simons and Husserl.[18]

Intuitive though it may seem at a first pass, the starting point for Fine's analysis is that such a simple and purely modal construal of ontological dependence in fact turns out to be hopeless; it is simply not fine-grained enough to exclude some patently spurious cases.[19] As already indicated, Fine proposes in its place an analysis in which

> the necessity of the conditional 'x exists only if y does' should be appropriately tied to the nature of the dependent item x.[20]

It follows that such conditionals are 'not necessary simpliciter' but are in addition 'tied' in some sense to the nature of their relata. Now, the 'natures' involved in this 'tying' are to be understood in terms that are at least close to what has traditionally been associated with *essence* – for Fine's analysis is indeed explicitly essentialist. This raises an immediate concern, however, for we probably do have to grant to French that 'essentialism has not typically been viewed all that favourably in the context of modern physics.'[21] Hence we

[18]See Fine [1995a] for discussion of and references to these purely modal accounts.
[19]See however Wildman [ms].
[20]Fine [1995a], p272.
[21]French [2010], p106. This is in part because essentialist doctrines in the philoso-

face the worry that by invoking Fine's theory in discussions of structuralism in physics we are attempting to shoe-horn a very contemporary ontology into a hallowed framework that was not designed to accommodate it. It is perhaps partly for this reason that structuralist discussions of priority have tended to focus on the notion of *identity* without any mention of essence – the former being deemed to connote something altogether more innocuous than the latter.[22]

However, I would argue that keeping the discussion confined to identity considerations to the exclusion of this thing called 'essence' is neither realistic nor necessary. Talk of ontological dependence is after all metaphysical talk, and thus the fact that *physics* never speaks explicitly of essence need not constitute any reason to shun it. Furthermore, and as Lowe has argued, talk of the 'identity dependence' with which structuralists seem more comfortable apparently still requires us to ultimately invoke something close to essence if we are to put constraints (as we must) on what sort of properties should feature in the analysis of the identity of an entity.[23] But more positively, we should, I think, be open to the possibility that essence too may be relatively innocuous in the sorts of structuralist contexts that we will be working within here. It may turn out, in fact, that what Fine has in mind by 'essence' is just the sort of thing that we are used to dealing with *all the time* in particle physics. But before I can assert any of that, of course, I need to clarify what commitment to essence involves. To do so, I will defer to a recent and useful survey article on contemporary approaches to ontological dependence. Here, Correia writes that

> the conception of essence Fine has in mind is a traditional conception according to which what is essential to an object pertains to *what the object is*, or *defines the object* (at least in part).[24]

phy of science have tended to be tied up in the language of dispositions, and hence of cause and effect – concepts that are problematic to say the least in the context of fundamental physics.

[22]Thus while French's discussion of dependence in structuralism cites the notion of 'essential dependence', he has little more to say than that 'the essence of an object is closely bound up with its identity', and thus he effectively reverts to discussing identity dependence (French [2010], p101).

[23]Lowe [1998], p148.

[24]Correia [2008], p1018; italics in original. Note that it is better to speak of 'entities' than 'objects' here; more on this below.

This we will take as our starting point. Now, perhaps the two conceptions of essence just alluded to are intended in a somewhat technical sense in which they come out as synonymous, but it doesn't seem so, and at a first pass they do seem to invoke different things – both of which we intuitively want to be brought into the discussion. If we understand the first characterization in the statement as meaning 'pertaining to what the object is, *as opposed to what it is not*', we seem to have invoked matters of *individuation* and hence of properties that might confer distinctness or individuality – something that structuralists are routinely happy to discuss. But since definitions need not be individuating, this needn't be the same thing as *defining* the object.[25] Indeed, in particle physics we usually take defining an object to be a matter of listing off the determinate, supposedly fundamental, state-independent properties common to all members of the particle's *kind*, all of which are indistinguishable with respect to these properties. These properties physicists usually call 'intrinsic' properties, but – as Ladyman and Ross remind us – they correspond much more naturally to what philosophers would call 'essential' properties (partly because such properties are always, among other things, permanent and observer-independent).[26]

Given that preliminary reassurance, and in keeping with Correia's rough-and-ready criterion, we can say, to a first approximation, that the properties we should take to feature in a particle's essence are

- the fundamental, determinate, state-independent properties that serve to define its kind; and
- (some of) the properties involved in conferring distinctness from other members of its kind.

I will leave the latter very vague, as I will not go into much depth about how essence and individuation fit together; all I am going to do is invoke a couple of theorems of Fine's as and when we need them. But hopefully I have done enough to dispel at least some of the initial misgivings about what essence might involve in the fundamental physics context, and with that, let

[25]Whether a definition is individuating or not will depend on its relationship to a pre-agreed principle of individuation. Compare, say, the definition of a set in terms of its members, and the definition of an electron in terms of its having mass m, charge e and spin s.

[26]Ladyman and Ross [2007], p134.

me introduce Fine's system.

9.3 Fine's System

The first thing to be clear on is that Fine's analysis of ontological dependence is intended as completely general in scope, incorporating dependencies between (among other things) physical objects, properties, numbers, sets, persons, and states of things at a time. Thus it is unfortunate – and especially so in the current context – that Fine often uses the term 'object' to refer to the relata of dependence relations when the more generic 'entity' would be a better term. Therefore while, to preserve ease of reading, I will not replace 'object' with 'entity' in the ensuing quotes from Fine, the reader should be clear that Fine's theory is to apply to (possible) entities of *any* category and is not restricted to the category of objects alone. In the structuralist case studies, however, references to objects should be understood as references to entities *in the category of objects* and thus to entities distinct from those in category of structures.[27]

What we must acquaint ourselves with next is Fine's primitive operator '$\Box_x$', which generates the prefix 'it is true in virtue of the identity of x that':

$$\Box_x =_{def} \text{it is true in virtue of the identity of } x \text{ that}$$

The idea behind this operator is that when it operates on a predication ϕ of an object x, it generates the proposition that ϕ is an essential property of x:

$$\Box_x \phi(x) =_{def} \phi \text{ is an essential property of } x.$$

As I have already pointed out, Fine takes essential attributions to be more discriminating than necessary attributions, and as a consequence he

> accept[s] that if an object essentially has a certain property then it is necessary that it has that property (or has the property if it exists); but [he] reject[s] the converse,[28]

and it is clear that we can schematize the part he accepts as follows:

[27] I would like to thank Kit Fine for clarifying this point.
[28] Fine [1994], p4.

$$\Box_x \phi(x) \rightarrow \Box(Ex \rightarrow \phi(x)), \tag{9.1}$$

which says that if an entity is essentially ϕ, then it is necessary that it is ϕ if it exists. Here, following Fine and the standard literature on dependence, I use this 'E' to denote 'the existence predicate'.[29] As Correia notes again, this schema is basically uncontested in the debate; in what follows, I will refer to it as the 'basic schema'. Now, given that the necessary truths that we obtain in this way are 'not necessary simpliciter' but 'flow from the nature of the objects in question', we may say that the necessity attached to the consequent here is itself reflective of the nature of x.[30] It follows that ϕ's being an essential property of x plus the basic schema implies

$$\Box_x \phi(x) \rightarrow \Box_x(Ex \rightarrow \phi(x)), \tag{9.2}$$

which states that it is essential to an entity that if it exists, it is ϕ.[31] I have also said that, for Fine, 'ontological dependence should be tied to the nature of the dependent entity', which we can now express as

$$\Box_x(Ex \rightarrow Ey), \tag{9.3}$$

which is Fine's analysis of the statement that x *ontologically depends upon* y. So what I will try to do in what follows is derive statements of this form, with objects and structures in the appropriate positions, from what I have called the basic schema.

In order to do that, two more things will be required. First of all, when it is the natures of *two* entities that are involved in some dependence, we need a suitable generalization of the basic schema, and this is presumably

[29]See for example Correia [2008], p1017. To be clear, Fine does not regard '$\exists x x = x$' to adequately express 'x exists'; rather than endorsing a *quantificational* conception of existence, he argues for a *predicational* conception instead. See Fine [2009] for an exposition of his view.

[30]Fine [1995b] p7; p8.

[31]Though I will not show it here, this is a theorem of Fine's system and may be demonstrated through the principles laid down in Fine [1995b]. I would like to express my gratitude to Fabrice Correia for providing me with the proof.

$$\Box_{x,y}\psi(x,y) \rightarrow \Box_{x,y}((Ex\&Ey) \rightarrow \psi(x,y)), \tag{9.4}$$

which says that if ψ holds of x and y in virtue of their essences, then if they exist, ψ is true of them. For the same reasons as before, we may infer from this, plus the fact that ψ holds essentially of them, that

$$\Box_{x,y}((Ex\&Ey) \rightarrow \psi(x,y)) \tag{9.5}$$

is true, which says that it follows from the natures of x and y that if they exist, then ψ is true of them. There is also the corresponding statement of ontological dependence of a pair x and y on some z:

$$\Box_{x,y}((Ex\&Ey) \rightarrow Ez). \tag{9.6}$$

As well as these generalizations to two or more entities, we are going to need the notion of the *consequential essence*. According to Fine, the most intuitive way to grasp this notion is via the concept of the *constitutive* essence (though we should note that ultimately the constitutive essence may be dispensed with to leave a purely consequence-based account).[32] As Fine puts matters:

> A property belongs to the *constitutive* essence of an object if it is not had in virtue of being a logical consequence of some more basic essential properties; and a property might be said to belong to the *consequential* essence of an object if it is a logical consequence of properties that belong to the constitutive essence. [...] Thus a property of containing Socrates as a member will presumably be part of the constitutive essence of singleton Socrates, whereas the property of containing some member or other will presumably only be part of its consequential essence.[33]

Intuitive as that may be, there unfortunately still remains work to do in defining the consequential essence – for, as Fine concedes, as it stands the proposed definition of consequence will be useless (and this relates to the idea that the essential is more fine-grained than necessary). This is because the

[32]See Fine [1995a], Section II.
[33]*Ibid.*, p276.

property of being the same as, or distinct from, any object (or better, 'entity') y will be a logical consequence of *any* proposition about *any* given object, and so will form part of the consequential essence of anything. It will follow that everything depends on everything else – clearly an awkward result. What is needed is 'an independent way of distinguishing between those objects that enter into the consequential essence as a result of logical closure and those that enter in 'their own right', i.e. by way of the constitutive essence,' and to this end Fine proposes that we impose a test on the logical consequences of any essential attribution.[34] The test for x to be said to depend upon y is that y cannot be 'generalized out' of the consequentialist essence of x, or, in other words, that x will depend upon y 'just in case some proposition $P(y)$ belongs to the essence without its generalization belonging to the essence.'[35]

While the motivation for this test is clear enough, it is also apparent that if we are to gain any purchase on which truths are universalizable and which are not, we must first specify of what it is that we take the appropriate domain of quantification to consist. Fine states that 'the quantifier can and, indeed, should be taken to range over every possible object' in order to preserve the Barcan principle (which again we take as the statement that the quantifier ought to range over every possible *entity*), and it is clear that if Fine's thesis is to be of any use to structuralism, then the domain had better include any of the structures that may be deemed relevant in the metaphysics of present or future fundamental physics (such as laws, groups, metric structures and so on).[36] While *that* is clear, what is far less so is that we can specify all of these structures that must feature in the domain in a clear and well-defined way. There may therefore be a worry that we will not be in a position to state that a given proposition $P(y)$ is satisfiable for every y unless we can find a way to specify in advance every y that should feature in the domain. Nonetheless, it

[34] *Ibid.*, p278.

[35] *Ibid.*, p278. So, for example, 'although it is part of the consequentialist essence of Socrates that 2 = 2, it is also part of his consequentialist essence that every object whatever is self-identical,' and so the number 2 does not after all feature in the singleton's consequential essence (at least not on this basis). On the other hand, while it is *also* a logical consequence of the nature of, say, Socrates' singleton that there is *something* that it contains, it does not logically follow that, for any object whatsoever, *that* object is contained. In accordance with Fine's criterion, then, we recover the idea that the property of containing something does belong to the consequential essence of Socrates' singleton set – and hence to its essence *simpliciter*.

[36] *Ibid.*, p277.

is obvious that the task of securing *positive* attributions of dependence will not require us to trawl through every element of an infinite domain, for if we can find just one entity that we indisputably do want to be included, but that does not satisfy the proposition, then we will be home and hosed. So let us try to get along for the moment without worrying too much about how to specify the domain beyond the fact that it contains all the entities – including all the objects and structures – of which structuralism will want to make use.

That is basically all that will be needed of Fine's machinery here, so let me now show how we can get it to work in structuralism. In what remains, I will examine two prominent case studies in structuralism, beginning with the case of entangled quantum particles and after that returning to consider the case of the group-theoretic conception of elementary particles. In each case, I will use Fine's analysis to rigorously recover the core structuralist claim that objects ontologically depend on structures. But I will also address the issue of whether this dependence is reciprocated, and hence try to make some progress toward adjudicating on the issue of which, if any, of structuralism's radical and moderate variants is recommended to us in either case.

9.4 The Dependence of Objects on Structure 1: Entangled Quantum Particles

As already mentioned, where structuralists have gone into any kind of detail about the nature of the priority they have in mind it has tended to concern *identity*; structuralism is indeed often explicitly presented as the thesis that objects lack 'primitive identity'.[37] Perhaps the most discussed and clearly presented statements of this thesis revolve around the seminal work by Saunders on Leibniz's principles and their application to physics.[38] While this work was judged, by many at least, to undermine the underdetermination claims that had previous motivated the eliminativist thesis associated with radical structuralism, the same work was redeployed to sustain the latter's core priority thesis. While the claim that the structure of quantum relations

[37]A review of this approach to structuralism may be found in Section 4 of Ladyman [2007].

[38]Saunders [2003].

ruled out the individuality of fermions was dropped, in its place emerged the claim that those relations secured individuality only at the price of rendering them ontologically secondary (and as such, it is claimed, still metaphysically eliminable).[39] Ladyman and Ross, for example, note that in their opinion 'while Saunders' view vindicates an ontology of individuals in the context of QM, it is a thoroughly structuralist one in so far as individuals are nothing over and above the nexus of relations in which they stand'.[40] They take this 'structuralist' account of particle identity to form a core plank of the foundation for their ontic structuralism as a whole, a position that consists of 'a conjunction of eliminativism about self-subsistent individuals, the view that relational structure is ontologically fundamental, and structural realism.'[41] As already mentioned, however, Hawley has complained that what exactly these claims amount to is not clear as it stands. Esfeld and Lam have also noted that it is not at all obvious how the observations regarding identity that Saunders brings to the table 'could ground an ontological priority of relations over relata.'[42] Let me now attempt to repair this situation by explicitly setting the priority claims that are taken to follow from Saunders' discussion into a Finean framework and seeing whether they do indeed follow.

In order do so, I must first briefly recap the main thrust of Saunders' argument. The issue at hand is *identity*, and Saunders takes as his starting point the analysis of identity in a modern logical context. In that context, he argues that the *principle of identity of indiscernibles* or 'PII' – the statement that if two objects possess all the same properties and stand in all the same relations to all the same things, then it follows that they are identical – can be regarded as well motivated. Indeed, Saunders argues that the PII may be identifed with (what he calls) the 'Hilbert-Bernays principle', and since he takes the latter to provide an explicit *definition* of identity it follows that the PII may be equally well regarded, in this context, as *true by definition*.[43]

[39]See e.g. French [1998] and Ladyman [1998] for statements of the underdetermination argument. It is my understanding that, at present, Ladyman accepts that Saunders' argument vindicates an ontology of individuals, while French still holds that the dilemma persists.

[40]Ladyman and Ross [2007], p138.

[41]*Ibid.*, p145.

[42]Esfeld and Lam [2010], p148.

[43]See Saunders *op. cit.*, Section 1. I note that here I am merely presenting Saunders' argument as I find it: my principal purpose is to show how priority attributions may be rigorously built upon these foundations, not to criticize those foundations themselves.

The relevance of this to the specific issue of identity in quantum mechanics may be put as follows. It is a postulate of quantum mechanics that the states of interacting indistinguishable (i.e. 'same kind') quantum systems must be subject to *permutation invariance* – that is, they must be determinately either symmetric or antisymmetric with respect to the exchange of particle labels, producing either bosons and fermions, respectively.[44] This invariance under exchange has the consequence that there is nothing in terms of the essential properties that define their kind, nor in terms of the dynamical relationships they bear to one another, that can be used to distinguish between the particles in a given state in such a way that we may determinately refer to any one of them to the exclusion of any of the others. This predicament gave rise to the 'Received View' that quantum particles violated the PII and thus could not be regarded as individuals.[45] Saunders' insight, however, was that the idea that entangled quantum particles are thereby indiscernible rests on a view of discernibility that is unduly restrictive. So long as there exists an irreflexive relation between the objects at hand (a relation which, in the context of permutation invariance, will necessarily be symmetric), the particles can be regarded as distinct consistently with the PII: it will *not* be the case that the objects stand in all the same relations to all the same things due to the irreflexivity of the symmetric relation. And the relevant states in the case of fermions – which we regard as the fundamental constituents of matter – are of the form

$$\frac{1}{\sqrt{2}}(\psi_x(\uparrow)\psi_y(\downarrow) - \psi_x(\downarrow)\psi_y(\uparrow)), \tag{9.7}$$

which may be interpreted as meaning that two particles stand in the symmetric but irreflexive relation of having equal but opposite spin. Objects such as these, which may be secured as distinct only by appealing to the presence of a symmetric but irreflexive relation, are said to be *weakly discernible*.[46]

That Saunders' argument yields this much has not been without controversy but is nonetheless widely accepted. What is by contrast much less clear is how exactly his argument may be used to underwrite the claim that fermions

[44]Paraparticle states are also permitted, but do not seem to be instantiated.
[45]See, e.g. French and Krause [2006].
[46]Saunders' analysis was subsequently extended beyond fermions to bosons (see Muller and Saunders [2008]), but this has proved more controversial.

are thereby somehow ontologically secondary. By inserting this claim into Fine's framework, however, we may justify the claim as follows.

We have agreed that quantum mechanics supplies an irreflexive relation between entangled fermions, and Saunders has shown that objects satisfying such a relation are distinct. Thus we may write

$$E(R : R^{irref}(x, y)) \rightarrow x \neq y, \tag{9.8}$$

where I use '$E(R : R^{irref}(x, y))$' to express 'there exists some irreflexive relation holding between x and y'.[47] What we need now is one of Fine's theorems connecting essence and identity that were mentioned before, namely,

$$x \neq y \rightarrow \Box_{x,y}(x \neq y). \tag{9.9}$$

(To get a sense of what this means, it may be helpful to contrast it with

$$x = y \rightarrow \Box_{x} x = y \tag{9.10}$$

and note that, as Fine says, 'whereas a true identity $x = y$ depends upon the nature of the one object x, a true non-identity depends upon the nature of both objects' – which intuitively seems right.[48]) So while Saunders' analysis delivers that entangled fermions are distinct, Fine's analysis then tells us that it is *essential* to them to be distinct, and we have

$$\Box_{x,y} x \neq y. \tag{9.11}$$

What else can we deduce to be essential to these objects? Recall that, on the assumption that we are dealing with particles that are at best weakly discernible, it follows from the PII that if the objects are distinct, there must be an irreflexive relation between them. To put it schematically, we may write

[47] French and Krause ([2006], p9) suggest there is a 'worry' that by apppealing to irreflexive relations we beg the question; see Hawley [2009], Section 3.2 for discussion of related points. However, and to repeat, I am here simply presenting Saunders' argument as I find it.

[48] Fine [1995b], pp255-6.

$$PII(x,y) \rightarrow (x \neq y \rightarrow E(R : R^{irref}(x,y))), \tag{9.12}$$

where '$PII(x,y)$' symbolizes that x and y obey the PII (that is, that x and y are identical if they have all the same properties and stand in all the same relations to all the same things). Now, as emphasized above, according to Saunders the PII can be regarded as a *definition* of identity, so that '$PII(x,y)$' may be regarded as *true by definition* as well. We may thus move from (9.12) to

$$x \neq y \rightarrow E(R : R^{irref}(x,y)). \tag{9.13}$$

Now, we know from (9.9) that the property of being distinct that features in the antecedent of this expression is essential to x and y. Whatever follows logically from this property will therefore pertain to the consequential essence of both objects, *so long as* the implied proposition cannot be universalized – for that, to recap, is the test we must apply to see if the implied proposition belongs to the consequential essence. What is left is to test whether the corresponding universalized statement can be derived from the non-identity of x and y. Now, while we have remained quiet on the full content of the domain, we know that structuralists will hold that physical relations must feature in it. But from the fact that our two particles are distinct and obey the PII, we of course cannot deduce that *every* physical relation that the two particles enter into is irreflexive:

$$(PII(x,y) \& x \neq y) \nRightarrow All(R : R^{irref}(x,y)); \tag{9.14}$$

indeed, 'being of the same species' is presumably one physically significant but reflexive relation that holds between the (by assumption indistinguishable) x and y. We may therefore deduce that

$$\Box_{x,y} E(R : R^{irref}(x,y)), \tag{9.15}$$

and hence confirm that it is essential to x and y that there exists some irreflexive relation in which they stand. This, therefore, represents a further essential property of the pair.

Now let us go back to (9.4), which is what I called the 'basic schema' extended to two objects:

$$\Box_{x,y}\psi(x,y) \rightarrow \Box_{x,y}((Ex\&Ey) \rightarrow \psi(x,y)).$$

Since we have established that it follows from the natures of x and y that there exists some irreflexive relation for them to stand in, we may substitute in and write

$$\Box_{x,y}E(R : R^{irref}(x,y)) \rightarrow \Box_{x,y}((Ex\&Ey) \rightarrow E(R : R^{irref}(x,y))), \quad (9.16)$$

or more simply (cf. the move from (9.4) to (9.5) above),

$$\Box_{x,y}((Ex\&Ey) \rightarrow E(R : R^{irref}(x,y))). \quad (9.17)$$

But this is just the statement of the ontological dependence of the particles upon irreflexive relations, in accordance with Fine's definition (9.6).[49] Since structures are supposed to be 'nexuses of relations' (vague though that notion no doubt is), we seem to have arrived at just what the structuralists want us to buy into: namely, the *ontological dependence of quantum objects on structures*.

The steps that have just been gone through seem to come close to the sought-for *demonstration* of the claim that quantum objects depend on structures (and hence are not 'self-subsistent'), demonstrated using Fine's principles. It thus appears that this flagship statement of structuralism may indeed be sustained. In order to fulfil our objectives, however, we need to go further and ask whether the *radical* structuralist claim that 'relational structure is more ontologically fundamental than objects' thereby goes through (or at least does so in the case of fermions), or whether it is the *moderate* position that should be adopted in this context. Now, it is clear that this question is not settled by

[49]Since no particular irreflexive relation is being singled out here, the dependence is a *generic* dependence (to quote some standard terminology: see Correia [2008]).

what has so far been shown, for the issue of whether there is any *reciprocated dependence of relations on objects* must be investigated before any one position can be chosen. Whether this reciprocated dependence holds or not will be a function of how we choose to conceive of relations – in Fine's picture, on what we think their *nature* is. Traditionally, of course, there have been two ways to do this: we can either conceive of them *extensionally*, or we can conceive of them *intensionally*. But then it becomes clear that whichever is adopted, the radical structuralist in particular potentially has a problem.

The reason for this is that if we choose to conceive of relations extensionally, then given the identity criteria for relations in extension – namely, that two relations are distinct iff there is an ordered tuple in the extension of one that is not in the extension of the other – then, by deploying exactly the same sort of reasoning as that just engaged in, we will be able to deduce that the relations are likewise ontologically dependent on objects.[50] By adopting an extensional account of relations, then, the moderate position would be immediately vindicated.[51] Now, all that radical structuralists will take that to imply is that this route must be rejected – indeed, rejected as nothing other than a pillar in the whole 'object-orientated' approach to metaphysics that they explicitly denounce. And it is in fact sometimes gestured at that it is an intensional understanding that structuralists like Ladyman have in mind. In a couple of places, for example, Ladyman writes

> We eschew an extensional understanding of relations [...] According to Zahar, the continuity in science is in the intension, not the extension, of its concepts [...][52]

Exactly what this is intended to mean does not seem to be fully developed anywhere in the literature.[53] But of course, the big problem in the vicinity of any consideration of this sort is that the whole reason that Quine, for example, rejected intensional entities was that he deemed it very unclear what their

[50]Note that once again we would obtain a generic dependence in this case.

[51]I note too that Fine's analysis permits cyclical dependence, which would be the case here: see Fine [1995a], Section III.

[52]Ladyman and Ross, p128; also Ladyman [1998], p418.

[53]It has been suggested to me that it may be connected with the idea that structures possess 'primitive modality', but I do not want to put any words in Ladyman's mouth here.

identity criteria were supposed to be.[54] Now, this is of course not to say that such criteria cannot be provided in principle. The point is just that such criteria *need* to be provided, and defended, by structuralists of the radical stripe if their priority claim is to go through. For although many of those who defend intensional entities do so not because they think that intensional entities necessarily *have* perspicuous and reductive identity conditions, but because they *reject the idea that they must* if they are to be philosophically legitimate (and often on some sort of *tu quoque*-type grounds), this is not an option that is available to the radical structuralist (or at least not obviously).[55] Why, after all, should it be deemed obviously acceptable that *relations* can lack reductive identity conditions and thus possess primitive identity if the structuralist judges it to be so objectionable as applied to objects?[56]

The conclusion at this point can therefore only be stated in conditional form. If an extensional account of relations is adopted, then it seems that the moderate position wins out as the right metaphysics of fermions. If, on the other hand, an intensional account should be adopted, then in lieu of some identity criteria for relations-in-intension and, in particular, a policy on whether those identities are functions of objects or not, we simply *have no idea* whether or not the dependence is reciprocated. It follows that we do not know which structuralist stance is best recommended to us either. Without a positive statement on the identity criteria for relations, it therefore seems that there is nowhere for this debate to go.

That structuralists have had so little to say about the matter of the identity conditions of relations is on reflection a little surprising, given the centrality of both relations and identity considerations in structuralist metaphysics. And of course, since Fine's analysis *assumes* an understanding of the nature

[54] See, for example, Quine [1981].

[55] See for example Loux [2002], p57-7.

[56] I am not denying that it may be consistent for a radical structuralist to be a quidditist. But since identity considerations do so much work in structuralism, and given that proposals for analyses of property identities exist (as an example in the intensional case, see Hale [forthcoming]), I do think that structuralists have to say *something* by way of explaining why it is that primitive identities are acceptable in the case of properties and relations, if so objectionable in the case of objects. In any case, *some* explicit line must be taken regarding relation identity conditions if radical structuralists are to use Saunders' argument as support for their view, but as yet this seems to be lacking.

of the relata involved in dependence attributions, it can be of no help to structuralism in resolving this dispute. What has nonetheless been positively established by this point is that the flagship claim of structuralism – that objects are dependent on structures – may be shown to go through on Fine's conception of dependence. There is thus at least something for us to take home from this study of quantum mechanics and identity from a Finean point of view.

9.5 The Priority of Structure 2: The Group-Theoretic Conception of Elementary Particles

While the case just discussed is perhaps the most vaunted of all of the priority arguments in structuralist philosophy of physics, I want to return now to that other aspect of modern physics that is held up as a poster-child for structuralism and that was discussed in the last chapter, namely the *group-theoretic conception* of fundamental particles. The relevant structure here is of course the *symmetry* structure of physical laws, and the issue at hand is whether symmetries may be claimed to be more fundamental than even the so-called fundamental particles – where, to recap, that relative fundamentality claim is to be understood to relate *categories* and not levels.[57] This is something that we have already considered in the context of the global SU(3) flavour group and the strongly interacting particles (though of course with a supervenience-based, and not dependence-based, understanding of priority in mind). However, since the group-theoretic conception of particles has its roots in earlier work in relativistic kinematics, and it since it is this revisionary conception of the *nature* of objects that has largely motivated the privileging of dependence as the right priority relation for structuralism, it may be worthwhile to say something about how this conception of objects emerged from that earlier context.[58] What I therefore want to do in this section is discuss

[57]Just to remind us, to say that a law possesses a symmetry is to say that there is a set of transformations under which the form of that law remains invariant, and such a set of transformations may be shown to form a group in the mathematical sense.

[58]Of course, the particles discussed in the last chapter will also correspond to representations of the relativistic symmetry group, since their full symmetry group is taken to be the product of their 'internal' and 'external' groups (cf. Chapter 8, Section

how structuralism's priority claims regarding symmetries and particles look when viewed through the lens of Finean dependence, beginning with a potted history of the group-theoretic approach to particles itself.

The modern attitude to symmetries arguably emerged in the context of special relativity, in which Einstein famously deduced the laws governing free relativistic systems by assuming invariance under the Poincaré transformations. Einstein's seminal work was subsequently extended by Wigner, who used those same symmetries – in combination with the principles of quantum mechanics – to deduce not just the laws that relativistic quantum systems would satisfy, but also those systems themselves.[59] What made this derivation of particles possible is grounded in the fact that the states of physical systems obeying laws of a given symmetry will, as we by now know, fall into what are known as the *irreducible representations* of the group associated with that symmetry. For brevity, I will call these representations 'irreps'. As was described in the last chapter, we may think of these irreps as sets of states that are mapped into one another by the action of the transformations that together comprise the group, so that an irrep in this sense constitutes a vector space.[60] The crucial property of irreps for the purposes of Wigner's analysis is that states from different irreps cannot be mapped into one another by the transformations of the group, and the significance of this in the case of, in particular, the Poincaré group is that the differences between states drawn from different such representations may not be effaced by a mere change in perspective. It thus makes sense in this context to regard the states from different irreps of this group to be states of *objectively different physical systems*, since any differences between such states must be relativistically invariant. As such, it was proposed that *different irreducible representations* of the Poincaré group correspond to *different species of relativistic particle*, and it was in this way that the connection between symmetries and particle species was born.

2).

[59]The *locus classicus* of this is Wigner [1939]. Tung [1985], Chapter 10 also provides a full and rigorous, but approachable elucidation of Wigner's analysis.

[60]More than this is of course required of an *irreducible* representation – in particular that it contain no 'smaller' representations – but this will do for our purposes just now. Again, here I follow the standard (if slightly sloppy) physics practice in referring to the states that strictly speaking 'carry' the (matrix) representations as the representations themselves.

To assess the viability of this proposal concerning the intimate connection between particle types and symmetry groups, what was needed was a classification of the irreducible representations of the Poincaré group. This task was undertaken by Wigner and – glossing over some the subtleties that led to certain representations being discarded – his analysis demonstrated that the irreducible representations, and hence relativistic particles, should either have

- some determinate *mass* $\in \mathbb{R} > 0$ and *spin* $\in \mathbb{Z}/2$, or
- some determinate *mass* $= 0$ and *helicity* $\in \mathbb{Z}$.[61]

But it turns out that these are precisely the properties that the fundamental particles do in fact have. The first class describes the electrons, the quarks, the massive bosons – pretty much everything, in fact, except the photon and gluons, which are in turn described by the second. *All* of the elementary particles we know of so far conform perfectly to this scheme. This spectacular success of the classification of free relativistic particles in terms of the representations of the Poincaré group caused the general strategy to be emulated outwith the context of free inertial motion and in the study of the fundamental interactions, and it has been this study of the symmetry groups of dynamical laws that has facilitated the successful prediction of whole new families of fundamental particles. As we have already seen, the first three (up, down and strange) quarks, for example, were identified with the three states in the fundamental irrep of the SU(3) flavour group; eleven of the twelve gauge bosons were also predicted through an analysis of the representations of the local SU(3) and SU(2)$\otimes$U(1) gauge symmetry groups that are the lynchpin of the Standard Model (the photon was already known).[62]

This newfound ability to not just *describe* the particles that we regard as

[61] For example, in the case of massless representations, one simply throws away a continuous infinity of irreps on the grounds that the helicities of particles corresponding to such representations will take on the structure of translations: that is, they will not be restricted to integers or half integers, and will be unbounded from above and below (see e.g. Tung [1985], Section 10.14). This point clearly relates to that raised by Wolff [2012] and that was mentioned in the last chapter, in that the group structure alone cannot be said to *determine* what representations will be realized.

[62] As noted in the last chapter, in some situations we take the full representation to represent a particle and in others only a basis state. It is interesting to consider why this is, but I won't consider it in any detail here.

fundamental, but also to *predict* their existence and properties, clearly represents an extraordinary development in our understanding of matter. The strategy outlined above has in general been so fruitful, in fact, that one can find prominent physicists saying things like

> ever since the fundamental paper of Wigner on the irreducible representations of the Poincaré group, it has been a (perhaps implicit) definition in physics that an elementary particle 'is' an irreducible representation of the group, G, of symmetries of nature.[63]

Such an adage may in fact be found all over the particle physics literature (in one form or another). It therefore seems that through these manifest successes, physicists have grown to conceive of an elementary particle as something that is inherently group-theoretic, and it is of course this conception of fundamental particles that contemporary physicists seem to have adopted that structuralists believe should be imported into fundamental metaphysics. And we should be clear that the implications of this idea for fundamental metaphysics are potentially enormous. In addition to the fact, noted at the beginning of this chapter, that the group-theoretic conception of objects appears to be starkly different to the conceptions of objects typically found in metaphysics, given that fundamental entities are often taken to be those 'whose existence and features have no further explanation' it is no longer clear that there even *are* any fundamental particles by this definition, since the properties that particles have, and the way in which they are knitted together, both appear to be explicable via considerations of group structure (at least to some significant extent).[64] It is this apparent consequence that fuels the structuralist claim that even 'fundamental particles' are not *truly* fundamental, and that what *should* be regarded as properly fundamental is the symmetry structure, or group structure, that explains their basic features.[65] Lyre for example takes the above considerations to 'support structural realism' on the grounds that

> a group theoretic definition of an object takes the group structure as primarily given, group representations are then constructed

[63]Ne'eman and Sternberg [1991], p327; quoted in Roberts [2011].
[64]This definition of the fundamental is given in deRosset [2010], p2-3.
[65]Again, I would urge that this claim ought to be understood as a claim about categories and not levels.

from this structure and have a mere derivative status.[66]

Likewise, Kantorovich has claimed that these sorts of considerations demonstrate that the representations of groups, and hence the elementary particles, have 'a lower ontological status' than the symmetries themselves.[67] These claims regarding the 'lower' or 'derivative' status of irreducible representations of course seem to be gesturing toward attributions of priority, but so far these claims remain largely unanalyzed.[68] In order to better articulate them, the focus on *definition* in the quote above naturally invites us to seek an approach

[66] Lyre [2004], Section 3.2. This quote may be found repeated all over the survey literature: see, e.g., Ladyman and Ross [2007], p147; Ladyman [2007].

[67] Kantorovich [2009], pp79-80.

[68] I say 'largely' because, as was noted in the last chapter, (i) in places Kantorovich seems to analyze priority in terms of supervenience, which is a defined notion; and (ii) as also mentioned, in other places Kantorovich seems to analyze priority in terms of dependence, which he explicitly defines. However, regarding (i), as we saw in the last chapter understanding priority in terms of supervenience does not establish that objects are less fundamental than structures (and hence of 'lower status') since one can show, at least in the case of simple Lie groups (of which the Poincaré group is not one!), that the determination goes both ways. Regarding (ii), his argument that structure is *dependent* on objects analysis simply fails to establish its conclusion. In more detail, the 'master argument' Kantorovich offers for his dependence claim consists of a 'thought experiment' in which we are asked to imagine a physically possible world with hadronic matter and consider the global SU(3) flavour symmetry. We are then asked to agree there can be a moment in time when that world is free of hadron matter and only photons and leptons are left, but it is nonetheless the case that hadrons may be produced in high-energy lepton or photon-photon processes. Kantorovich holds that this shows that '*the internal symmetry exists as an underlying structure whereas hadrons are uninstantiated*' (Kantorovich [2003], p664). Now, since Kantorovich adopts something like a purely modal definition of ontological priority – holding that 'If for any two physical entities A and B there is a possible world in which there is a physical situation for which A exists but B does not, but there can be no world in which B exists but A does not, we may say that A is 'ontologically prior' to B' (see e.g. [2003], p664) – this is taken to produce the result that the symmetry is prior to the hadrons. But even ignoring the deficiencies of such an approach to dependence (and here I simply allude again to Fine [1995a]), Kantorovich's argument patently begs the question against someone who holds that the existence of the SU(3) symmetry is in fact just a summary of the regularities holding amongst hadrons, and who thus holds that they would not exist in a world in which hadrons were not instantiated (or at least would not for as long as those hadrons were not instantiated, assuming that that makes sense). Indeed, even someone who took a 'global regularity' view of laws and thus countenanced that the SU(3) symmetry can be a global property of worlds and thus any hadron-free time-slices of them, they would *still* deny that his argument shows that symmetries should be regarded as prior to hadrons.

to ontological priority based on it; but that is of course exactly what we have been considering in this chapter. Let me therefore now show how Fine's conception of dependence may be used to sharpen up this move from the reconceptualization of objects in group-theoretic terms to novel claims about priority, focussing for concreteness on the case of the Poincaré group and its representations.

If what it is to be an elementary particle is *defined* in terms of its being an irreducible representation of the Poincaré group, then – in accordance with our discussion in Section 3 above – that forms part of its essence. Thus, where x is a relativistic particle, we have

$$\Box_x IR_{PG}(x), \tag{9.18}$$

where '$IR_{PG}(x)$' means 'x is an irreducible representation of the Poincaré group'. And it is clear that from that essential property one may deduce the existence of the Poincaré group, for it is the transformations *of* this group that define the representation. (To recap, an irreducible representation of a group is defined in terms of a set of states that is closed under the action of the group transformations.) We may represent this as

$$IR_{PG}(x) \Rightarrow E(G : G = PG), \tag{9.19}$$

where '$E(G : G = PG)$' stands for 'there exists a group which is the Poincaré group' (or more simply, that the Poincaré group exists).[69] What we must do now is check that the deduced statement cannot be universalized and thus that it passes the test alluded to above. But it is immediately clear that it *cannot*. It is plainly not the case that anything other than the Poincaré group is the Poincaré group; that accolade, obviously, belongs only to that particular group itself. We may therefore confirm that it is indeed part of the consequential essence of a relativistic particle that the Poincaré group exists:

[69]Just to repeat, the ontic structuralist will insist that the domain of quantification in dependence attributions must contain structures, including group structures.

$$\Box_x E(G : G = PG). \tag{9.20}$$

From this, we may then derive the ontological dependence of elementary particles on this group, just as we did before; substituting into the basic schema we obtain

$$\Box_x E(G : G = PG) \rightarrow \Box_x (Ex \rightarrow E(G : G = PG)), \tag{9.21}$$

which together with (9.20) gives us

$$\Box_x (Ex \rightarrow E(G : G = PG)). \tag{9.22}$$

We thus obtain exactly what the structuralists want, namely, the dependence of relativistic particles on the Poincaré group, and hence on the group structure of relativistic laws.[70]

Once again, therefore, we see that the central structuralist claim – that objects depend upon and hence are not prior to structures – may be straightforwardly established through Fine's analysis. But, as before, we are not yet done. Insofar as we want to ascertain whether a *superior* status may be accorded to group structure, and hence decide whether it is the radical or the moderate position that represents the right philosophy for this revolution in physics, we need to address the converse relationship between the groups used in physics and their representations. This, however, is a more difficult question to answer, because although structuralists (and physicists) have had plenty to say about particles *qua* irreducible representations, it seems that less attention has been paid to the *ontological interpretation* of group structure. Of course, it is perfectly straightforward to say how a given group is defined *mathematically* – we can go and look that up in a book – but structuralists take the statement that symmetry structure is ontologically fundamental to be a statement about what should be regarded as fundamental *to physical ontology*.[71] The question of what qualifies some, but only some, mathematical structures to enjoy the

[70]Note that this time it is a 'rigid' dependence: the nature of these particles *qua* representations of the Poincaré group demands not just that some group exists, but that a *specific* group does.

[71]See e.g. French and Ladyman [2003b].

status of aspects of physical reality is therefore one that structuralists about physics cannot avoid facing up to. Given that Fine's analysis ties dependence to the nature of the dependent entitity, until we are clearer on this issue there is little progress to be made on the question of whether the group structures that structuralists promote themselves depend on objects. But there are a few things that I think we *can* say, however.

The first thing to mention is that not all groups need be on a par with one another when it comes to matters of physical interpretation. The Poincaré group, for example, consists of a set of operations that each have a clear physical meaning, since we know very well what it means to boost, or rotate, or translate a physical system such as an observer through space and time (and can indeed in principle observe that such a transformation has taken place). This feature does not, however, seem to be a general feature of the groups that we use in physics: the groups mentioned above that encode facts about interactions, for example, do not in general enjoy this luxury.[72] Indeed, there appears to be *no* physical interpretation to be had in the case of the local gauge transformations that underpin the Standard Model, since such transformations may be shown to correspond to mere changes in representation only.[73]

How, then, are we to do it? Something that structuralists such as French and Ladyman have cited as a means of distinguishing the structures they wish to reify from merely *mathematical* structures, and thus rebut the accusation of 'Platonism', is to characterize them as *causal*.[74] Now, while at that time French and Ladyman 'acknowledged that causal relations constitute a fundamental feature of the structure of the world,' this is something that Ladyman at least now seems to have retracted.[75] In any case, causality is notoriously problematic – especially when dealing with quantum systems – and it would be nice not to have to appeal to it. Furthermore, deciding how exactly to cash out group structures as 'causal' in anything like the sense in which we regard objects as such is likely to prove difficult – not least in the absence of a clear physical interpretation of the group in the first place.[76]

[72]See e.g. Wigner [1968], p810.

[73]See e.g. Lyre [2004], p650. To be clear, I here mean 'representation' in the generic sense of mathematical representation, not in the sense of (reducible or irreducible) group-theoretic representations.

[74]*Op. cit.*, p75; see also French [2010], Section 4.

[75]*Op. cit.*; Ladyman and Ross [2007].

[76]In any case, since those who do take causation seriously often offer the funda-

But there is a related and less metaphysically loaded consideration in the neighbourhood. Surely a very minimal, necessary condition on the physical significance of some structures over others is that they have *empirical consequences*. Indeed, this is something that French and Ladyman themselves acknowledge; they ask, for example,

> What makes a structure 'physical'? Well, crudely, that it can be related – via partial isomorphisms in our framework – to physical 'phenomena'. This is how physical content enters.[77]

Now, if symmetry structures are to be 'related to phyical phenomena', they must of course be relatable to *measurement*. And it is obvious that modern, quantitative empirical testing is all about the detection of determinate properties – something that is made especially explicit in the basic formalism of quantum mechanics, in which a measurement is represented as the obtaining of a real eigenvalue. But this makes it clear that some reference to a group's representations must enter into any characterization of the group if it is to be considered as a part of empirical reality, for it is the *irreducible representations* of the symmetry groups that carry determinate values, not the symmetry groups themselves.[78] The irreps of the Poincaré group, for example, possess determinate mass and spin; the Poincaré group itself clearly does not. (It clearly doesn't make sense to ascribe mass and spin to a set of transformations between observers.) Likewise, it is the *states in* the irreps of the SU(3) flavour group that possess the determinate properties of isospin and hypercharge that define the first three quarks; the SU(3) group does not. Putting everything together, then, it seems that reference to representations must be included in the definition of group structure *qua* denizen of physical reality, since it is only these that can furnish the required connection with phenomena. And that, as will by now be clear, will generate a *reciprocal dependence of group structures on objects* once we turn the handle on Fine's machinery, so that it appears to

mental determinate properties as the ultimate locus of causal agency, the conclusion of the argument regarding the empirically testable nature of structures that I am about to put forward will apply even if we choose to cash out the physical nature of structures in causal terms.

[77] *op. cit.*

[78] I stress that here I am not saying that something must itself *possess* determinate physical properties to count as part of empirical reality, only that it must be *suitably related* to them.

be decisively the *moderate* stance that is vindicated in this case.[79]

All in all, while one could certainly claim that, *qua* mathematical abstractions, there is no essential dependence of groups on vector spaces or their irreducible representations, as denizens of physical reality the matter looks very different. And unless the radical structuralist can find another way of characterizing the physical significance, including the testability, of the groups used in particle physics that does not involve any reference to the representations, we cannot assert that the representations have *merely* 'derivative status'. Rather, the irreducible representations and the symmetries of nature are each ontologically dependent on the other; given the 'reconceptualization' of fundamental particles in terms of group-theoretic representations, that in turn means that fundamental particles and group structures are likewise on an ontological par.

9.6 Concluding Remarks

In the course of this chapter, I firstly argued that ontic structuralism can and should make use of Fine's notion of dependence to articulate its characteristic priority claims, on the grounds that is was this relation that was best suited to articulating any priority facts that may be embodied in its attendent 'reconceptualization' theses. I then put Fine's system to work to show that, in both the entanglement and group-theoretic cases, the ontological dependence of objects on structure can be rigorously sustained, and thus that (what I have called) the 'core claim' of structuralism can in each case be decisively established. From that point on, however, the two cases behaved rather differently. In the case of entanglement structures, while the dependence of objects on structure could be secured without any trouble, bereft of any positive statement of the identity conditions of relations we found ourselves hamstrung in trying to either establish or deny the existence of reciprocated dependence. By contrast, in the context of group-theoretic structures and

[79]We should note for completeness that unless there are special reasons for any *one* representation to be realized, we should expect this dependence to be *generic*. (There may be such special reasons in the case of the adjoint representation of the gauge symmetry groups featuring in the Standard Model, for example, since these correspond to the gauge bosons.)

their associated objects we were able, through a plausible interpretation of the physical nature of group structures in empirical terms, to mount good arguments that the dependence should postively be taken to be reciprocated, and thus that the moderate position wins out here.

As a first conclusion, we may note that in both cases we encountered much more difficulty in assessing whether there is any dependence of structures on objects than we did in assessing the converse. Given that Fine's analysis ties dependence claims to the nature of the dependent entity, this suggests that there is a lack of clarity not in our understanding of priority, but rather in our understanding of *the nature of structures*. Since it is precisely these entities that structuralists entreat us to regard as constituting the very foundation of physical ontology, the fact that we struggled to ontologically articulate these entities potentially carries a serious message for ontic structuralists. As a second conclusion, however, it seems that we are in a position to determinately declare that an unqualified acceptance of the radical position is untenable, since our second case study showed that particles do indeed have to be regarded as on a par with at least one extremely important class of structures. That of course entails in turn that any 'eliminative' structuralism in which objects are purged from the fundamental basis is likewise untenable as a general thesis about physics, since one cannot eliminate the objects without thereby eliminating the structures – something which would clearly be disastrous from the structuralist point of view.[80] Radical structuralists thus cannot maintain the two theses most closely associated with them when it comes to particle physics: they cannot both maintain that objects must be reconceptualized in terms of structures *and* that they be eliminated, *qua* metaphysically secondary entities, in favour of the associated structures.[81]

The net result of this discussion, then, is that the more radical claims made

[80]Of course, it could be that the moderate stance is vindicated with respect to *some* objects and structures, and the radical stances with respect to others. But radical structuralism in particular tends to present itself as an entirely *general* thesis about the relation between the structures and the objects of physics. (Consider – to take just one example – Ladyman and Ross' completely unqualified and uncontextualized claim that 'relational structure is more fundamental than objects' ([2007], p145.) But I will not reflect further on that thought here.

[81]Recall from the end of the last chapter that I hold that, of French's 'failed natural kind term' elimination and Ladyman and Ross' 'reductive' elimination, it is only the latter that has any initial plausibility.

by ontic structuralists must be regarded as *unjustified – certainly so in the* case of symmetry structure. And since I argued that these structures are the ontological correlates of the principles discussed in Chapter 7, a corollary of this is that the claim of Weinberg that principles are more 'elementary to all of nature' than even fundamental particles cannot be regarded as correct. Rather, these principles and such particles must be regarded as ontologically on a par with one another. But what we are left with nonetheless is a picture of the fundamental that is very different from that which is presented to us by purely 'object-oriented' metaphysics. It is a picture in which we regard even elementary particles as no more fundamental than (at least some of) the dynamical structures of contemporary physics, and in which a rich nexus of metaphysical dependencies weaves the two categories together. Thus while I think we all must agree that the more radical versions must be left behind, it remains the case that ontic structuralism has a highly revisionist, and hopefully now more rigorously supported, proposal to make to contemporary fundamental metaphysics.

Chapter 10

Conclusion

We have reached the end of the road. To draw the discussion to a close, let me retrace my steps and tentatively suggest some further avenues to which the thoughts adduced above might lead.

I began this thesis by noting that the assumption that there exists a fundamental level to reality is one that 'pervades contemporary metaphysics'. I noted further that it is standardly assumed that physics will describe such a level. I then asked whether physics might have more of a role in fundamentality metaphysics than it has been allocated thus far, and in particular, whether we might be able to use physics to deny that there even exists a fundamental level. It was this question that was the principal focus of Part 1 of this thesis. After arguing that contemporary metaphysics does not provide us with any compelling grounds to believe in a fundamental level, I considered whether Schaffer's meta-inductive argument, based on the history of physics, could support a positively anti-fundamentalist conclusion. While I argued that Schaffer's argument fell short in its ambitions, its shortcomings recommended that we pursue (what I called) the 'internal' approach to denying fundamentality. By means of two case studies, the Analytic S-matrix theory and the effective approach to quantum field theory, I then went on to argue that the internal approach can indeed be used to argue against the existence of a fundamental level – though also that those arguments may have to be regarded as qualified in significant ways.

It therefore seems that it is indeed possible to use physics, through the internal approach, to deny the existence of a fundamental level. In spite of this non-trivial achievement of the internal approach, however, we saw that using this approach imposes a profound limitation on the sort of fundamentality that we can hope to deny through its means. In particular, while we can use it to deny the existence of fundamental particles, or fundamental laws, we cannot use it to deny the existence of fundamental physical *principles*. This is because some such principles must at least be *treated* as fundamental in the context of the internal approach — namely, the set of physical principles from which our anti-fundamentalist conclusions are derived. This dialectical or methodological centrality of physical principles to fundamentality questions, however, naturally invited the question of how to understand such principles from an ontological point of view, and, in particular, whether such principles could be regarded as more *ontologically* fundamental than any particle or law. I furthermore pointed out that some contemporary particle physicists, such as Weinberg, have mooted precisely this latter view. After reviewing certain challenges facing any attempt to construe principles in ontological terms, I argued that we ought to understand the proposed greater fundamentality of principles relative to particles in terms of the priority of the *category of dynamical structure* over the *category of physical objects*. I noted further that this priority claim regarding the category of structure and the category of objects constitutes the core thesis of *ontic structural realism*, and that the claim represents a related, though different, anti-fundamentality claim on behalf of particles than that which was investigated in Part 1. Therefore, in Part 2 of this thesis, I examined whether this claim of ontic structuralism can in fact be defended. As part of this project, I attempted to adjudicate between structuralism's so-called 'radical' version, in which structure enjoys a one-way priority over objects, and its 'moderate' version, in which each category is regarded as on an ontological par with the other.

The tenability of ontic structuralism was investigated by examining some of structuralism's classic case studies. The first such study concerned the Eightfold Way classification of hadrons and the ensuing constituent quark model, and was investigated under the assumption that priority was to be understood in supervenience terms. Relative to that assumption, the analysis delivered that it is the moderate variant of ontic structuralism that is the better justified in this case. However, I then went on to argue that it is Fine's notion of ontological dependence, not the relation of supervenience, that represents the most appropriate priority relation for structuralism. On the understanding that priority is understood in those terms, I then enlisted further case studies featuring aspects of particle physics and assessed structuralism's proposals in those contexts. However, close analysis of these further cases only seemed to underline that it is the moderate position that is, on balance, the better vindicated form of structuralism. I therefore concluded that it is moderate ontic structuralism that represents the best metaphysics for particle physics. It therefore appears that, *contra* Weinberg, fundamental physical principles, should they exist, are no more and no less fundamental than any putative fundamental particles.

The principal results of this investigation are therefore as follows. Firstly, it is indeed possible to use physics to deny the existence of a fundamental level. The 'pervasive' assumption that there exists a fundamental level to reality is therefore not one that the naturalized metaphysician need share. Secondly, however, it seems that even the anti-fundamentalist about levels must admit that we will never be in a position to deny the existence of fundamental physical principles. Thirdly, the dynamical structures that may be argued to be the ontological correlates of these principles ought – again, in the cases we looked at – to be regarded as comprising a category *no less and no more* fundamental than that comprised by the objects of particle physics. Since the category of physical objects and the category of dynamical structure seem to be equally fundamental categories, it is the moderate version of structural realism that is vindicated in particle physics.

These conclusions will, I hope, be of interest to anyone who holds that the burgeoning literature on the metaphysics of fundamentality would benefit from naturalistic interventions. But given that physicalism, as a thesis about the fundamental, is almost universally accepted in metaphysics, it seems that the burden of proof is on anyone who holds that physics should *not* be brought to bear on fundamentality questions. Furthermore, my conclusions should be of interest to those concerned with structuralist philosophy of physics. In particular, I hope that Part 2 of this work constitutes a more developed, and perhaps more interesting, defense of the moderate variant than that already offered by Esfeld and Lam.[1] I furthermore hope that I have done service to

[1]As pointed out in Chapter 8, Section 1, for them the (reciprocated) priority of

structuralism in sharpening up the fundamentality claims that are definitive of it, by (i) clarifying that they are to be understood as claims about the fundamental *category* and not the fundamental *level*, and by (ii) precifying the notion of priority that is most appropriate to them.

While I hope that the conclusions I have reached in the course of this thesis will be regarded as a serviceable contribution to metaphysics, including the metaphysics of physics, it is nonetheless clear that much more could be done to improve and expand upon them. I will discuss just three such potential developments here.

Perhaps the most obvious way in which I might enrich the thesis would be by expanding the number of case studies used to establish the anti-fundamentality claim of Part 1. While one could argue that, strictly speaking, only *one* successful case study is required to establish the claim, the more anti-fundamentalist case studies that we can marshal, the more we can discover about the structure and features that anti-fundamentalist arguments can or must have. Furthermore, one could, with *some* justification, have reservations about the case studies that I did look at. Though it was very much the mainstream at one point in time, the Analytic S-matrix theory is regarded, by some, as of dubious scientific standing even then.[2] Furthermore, and as already pointed out, the anti-fundamentality claim that was obtained in the QFT case was not only undeterdetermined, but obtained only relative to a backdrop of perturbative assumptions. This is clearly a major qualification. However, it is equally clear that the more theories we can muster that have anti-fundamentalist implications, the less compelling will become any objection that the examples cited in support of anti-fundamentalism are somehow especially suspect, and the anti-fundamentalist world-view equally so.

objects over structures is established only through an insistence that 'for relations to be instantiated, there has to be something that stands in the relations' – a claim which does not establish the supposed priority, even if one can argue that it is true. In my approach, by contrast, the moderate view is defended by attending directly to the mathematics and the physics of the cases at hand, assessed against the background of an assumed priority relation.

[2]See, for example, the polemics of Woit [2006]. (While most of this book is aimed at criticizing string theory, since the S-matrix theory was instrumental in string theory's early development, Woit attacks this theory as well.)

As mentioned in Chapter 4, it has been suggested to me that the topic of dualities in string theory may offer further anti-fundamentalist support, and it is thus to this topic that I will first turn in pursuit of this end.[3] While I do not yet have the wherewithal to assess the content of the anti-fundamentalist implications of this concept, it is my understanding that one of its seeming consequences is that entities that are regarded as mereologically fundamental according to one theory may be regarded as composite in the dual theory.[4] While what this implied 'relativity of fundamentality' may have in store for anti-fundamentalism, in particular, has yet to be seen, the idea that fundamentality assumptions must always be assessed relative to a theory is clearly in the spirit of the internal approach. (Of course, whether string theory has a claim to being *a theory*, let alone an *extant* theory, is something that will require defence.)

Another way in which the thesis could be enriched and extended would be by pursuing in more detail the relationship between principles and structures that was alluded to in Chapter 7. As noted, some eminent physicists have stated that principles ought to be regarded as deserving the mantle of fundamentality in modern physics – something that gives the task of articulating the role and nature of principles in particle physics some independent motivation. But there already exists work on precisely this topic that may be built upon in order to do so, as several works have been written recently defending the notion of 'principle explanations'.[5] Since relative fundamentality is often, as noted in Chapter 6, cashed out in terms of explanation, this is a natural place to look in developing how principles might be oriented within fundamentality metaphysics.[6] Furthermore, preliminary work has been done relating such explanations to structuralist philosophy of physics, and thus this constitutes a natural place to start in investigating the ontological import of principles and their relationship to structuralism – something I myself gestured to in

[3]It was Dean Rickles who suggested this to me as a potentially worthwhile line of pursuit.

[4]See e.g. Castellani [2009], p11.

[5]See, e.g., Felline [2011]. Put simply, principle explanations explain phenomena in terms of the dynamical principles governing it; they are typically constrasted wtih 'constructive explanations', which explain phenomena in terms of the properties of its constituents.

[6]I have not explicitly discussed how fundamentality relates to explanation in this thesis.

Chapter 7.[7]

Finally, while discussing the Eightfold Way hypothesis and the ensuing constituent quark model in Chapter 8, I claimed that the core structuralist priority thesis was not significantly undermined by the fact that simplicity assumptions must be postulated *in addition* to the relevant structure if the corresponding objects are to be derived. However, I did little more to justify this claim than appeal to the reader's sense of charity and that is, of course, not satisfactory. But as also noted there, it appears that little work has been done on the question of whether, and how, non-structuralist elements, including putatively methodological elements, may be incorporated into structuralism without damage to the basic contours of the structuralist thesis. Perhaps this example could be regarded as a useful springboard from which to launch a more extended discussion of this matter, and thus represents a potential further line of development.

All in all, there is much more work to do in pursuing this investigation into the contributions of physics to the metaphysics of fundamentality. But I hope that something of value has been shown nonetheless, and that still more insights may yet be extracted.

[7]See e.g. Dorato and Felline [2011].

Chapter 11

Appendix to Chapter 5

In this Appendix I present some more of the mathematical details of Analytic S-matrix theory than were provided in Chapter 5. Part 1 describes the first five postulates of the theory and shows how they can be used to construct the S-matrix. Part 2 develops in detail the rationale for and consequences of the theory's sixth postulate. In what follows, I will consider only $2 \rightarrow 2$ processes, treat the external particles as 'non-identical' but of equal mass, and neglect the spin of the external particles.

Part A1 The Early Analytic S-Matrix: Derivation of the Amplitude

The S-matrix relates free-particle states to free-particle states. It operates in momentum space, and in what follows single-particle states for the ith particle are denoted by $|P_i\rangle \equiv |p_i, QN_i\rangle$, where QN_i is specific to each particle type and encodes the quantum numbers of that type, and p_i stands for the particle's 4-momentum $|p^0, \vec{p}\rangle$. Since the S-matrix always relates free particles, this relation is easily generalized to many-particle states $|P_i, ...P_n\rangle$ through the relation $|P_i, ..., P_n\rangle = |P_i\rangle \otimes ... \otimes |P_n\rangle$. The basic idea behind Analytic S-matrix theory is that certain constraints upon the matrix suffice to determine it uniquely, and these constraints are expressed in the following principles.

A1.1 Strong interaction forces are short-range.

It is known from nuclear physics that the strong interaction operates at distances no greater than the order of 10^{-15}m (a few pion Compton wavelengths). This means that we can regard particles as overwhelmingly likely to be free except when they are very close together in space. That in turn means that we can regard the asymptotic states – the states before and after the experiment is performed – as likewise essentially free. These free states are the only states that can be observed in the theory.[1]

A1.2 The Principle of Superposition.

This is the only distinctively 'quantum' aspect of quantum mechanics that is explicitly retained by this theory. The superposition principle tells us that, given an initial state $|P_1, P_2\rangle$ of two particles that subsequently come together, interact, and separate, the final state can be written as $S|P_1P_2\rangle$, where S is a linear operator.[2] As usual, the set of states is assumed to be orthogonal and complete.

A1.3 Lorentz invariance.

The scattering process, and hence the S-matrix, is assumed to be invariant under Lorentz transformations. That is, where L is a proper Lorentz transformation, $L|P\rangle = |P'\rangle$, we require that $|\langle P'_m|S|P'_n\rangle|^2 = |\langle P_m|S|P_n\rangle|^2$. The definition of the S matrix does not specify the phase uniquely so we can strengthen this to $|\langle P'_m|S|P'_n\rangle| = |\langle P_m|S|P_n\rangle|$. This has the consequence that the matrix elements for $2 \rightarrow 2$ scattering $|\langle P_3P_4|S|P_1P_2\rangle$ depend on the four-momenta $p_1, .., p_4$ only through their invariant scalar products. Since the particles concerned are free particles, the four-momentum of any state satisfies the mass-shell condition $p_0^2 - \vec{p}^2 = m^2$. Lorentz invariance demands that we must adopt a covariant normalization, and also implies energy-momentum conservation. The latter means that the matrix element for the $2 \rightarrow 2$ transition will contain a factor $\delta^4(p_1 + p_2 - p_3 - p_4)$.

[1]Chew [1966], p5.
[2]Collins [1977], p6.

Since I have just introduced the conservation of momentum, I will also point out here that the various strong interaction conservation laws associated with the global $SU(3)$ flavour symmetry are simply *assumed* in this theory. While research was done into 'bootstrapping symmetries' and deducing the SU(3) group structure from S-matrix principles, it was never shown that these internal symmetries were required on this basis, so that the various quantum number conservation laws remained as 'natural laws not even linked qualitatively' to the basic principles of S-matrix theory.[3] As such, the SU(3) flavour symmetry entered and remained as a postulate.[4]

A1.4 Unitarity.

The scattering matrix must be unitary: that is, we require $S^\dagger S = 1$, where '$\dagger$' denotes Hermitian conjugation. As always, this is to ensure that probability is conserved.

It is useful at this point to decompose the S-matrix S into $S = 1 + iA$. Such a decomposition has a clear meaning. The first term describes the S-matrix when the particles never get close enough to interact. For the reasons already given, it is in fact overwhelmingly likely that two particles will not in fact do so in a given scattering experiment, and hence this is the dominant term. The second term is the non-trivial part that describes interactions, and it is the properties of this amplitude for interaction that we shall study from now on.

The statement that S is unitary gives for A the equation $A - A^\dagger = iA^\dagger A$, or

$$2\mathfrak{Im}\langle P_3, P_4|A|P_1, P_2\rangle = \langle P_3, P_4|A^\dagger|n\rangle\langle n|A|P_1, P_2\rangle,$$

where the sum is over all intermediate states allowed by all conservation laws. We can write this more compactly as

$$2\mathfrak{Im}A_{ij} = \sum_n A^\dagger_{in}A_{nj}. \tag{A1.4a}$$

Consider first of all the contribution of a single one-particle state $|Q\rangle =$

[3]Chew, [1968a], p67; see also Chew [1968b], p765, and Chew [1966], p93.
[4]Cushing [1990], p159-160.

$|k, QN\rangle$ to the sum. I will take it as implicit throughout that the quantum numbers are conserved, so that only states $|Q\rangle$ with $QN = QN_1 + QN_2 = QN_3 + QN_4$ are allowed.[5] Neglecting constant factors, the RHS of (A1.4a) can be written as

$$\int d^4k\delta^4(k^2 - m^2)\langle P_3, P_4|A^\dagger|Q\rangle\langle Q|A|P_1, P_2\rangle.$$

Imposing the 4-momentum conservation explicitly on both sides, we have for the RHS

$$\int d^4k\delta^4(k^2 - m^2)\langle P_3, P_4|A^\dagger|Q\rangle\langle Q|A|P_1, P_2\rangle\delta^4(p_1 + p_2 - k)\delta^4(p_3 + p_4 - k).$$

This integration can be performed and gives

$$\delta^4((p_1 + p_2)^2 - m^2)\delta^4((p_1 + p_2) - (p_3 + p_4))\langle P_3, P_4|A^\dagger|Q\rangle\langle|Q|A|P_1, P_2\rangle;$$

since 4-momentum must be conserved on the LHS as well, this is equal to

$$2\mathfrak{Im}\langle P_3, P_4|A^\dagger|P_1, P_2\rangle\delta^4((p_1 + p_2) - (p_3 + p_4)).$$

Thus we get, for the one-particle intermediate state contribution to the imaginary part of the amplitude,

$$\mathfrak{Im}\langle P_3, P_4|A|P_1, P_2\rangle = \frac{1}{2}\delta^4(k^2 - m^2)\langle P_3, P_4|A^\dagger|Q\rangle\langle Q|A|P_1, P_2\rangle, \quad \text{(A1.4b)}$$

where $k^2 = (p_1 + p_2)^2$. Contributions from 2-particle, ..., n-particle intermediate states can be computed similarly. For example, for a two-particle intermediate state $|Q_1Q_2\rangle = |k_1, QN_1; k_2, QN_2\rangle$, where the particle masses are m_1 and m_2, we obtain

$$\mathfrak{Im}\langle P_3, P_4|A|P_1, P_2\rangle = \frac{1}{2}\delta^4(k_1^2 - m_1^2)\delta^4(k_2^2 - m_2^2)\langle P_3, P_4|A^\dagger|Q_1Q_2\rangle\langle Q|A|P_1, P_2\rangle, \quad \text{(A1.4c)}$$

where $k_1 + k_2 = p_1 + p_2$.

[5]While I neglect to mention explicitly the quantum number delta functions, I will include those of 4-momentum. This is because the amplitude is taken to be an analytic function of the latter and hence the delta functions in momentum must be handled very carefully – as will be seen below.

The constraints expressed in (A1.4b) and its many-particle analogues will be very useful in obtaining an explicit expression for the amplitude.

A1.5 The Principle of Maximum Analyticity of the First Kind.

The scattering amplitudes A are assumed to be maximally analytic functions of complex momentum variables, where the modifier 'of the first kind' refers to analyticity in the *linear* momentum variables only.[6] Now, the word 'analytic' is here used in a looser sense than that meant by mathematicians. When a mathematician says that a function is 'analytic' in a given region, what is meant is that it contains no singularities at all (it is *holomorphic* in that region.) What is meant here is that it contains only *isolated* singularities (it is *meromorphic* in that region), such as poles and branch points. The point is simply that it is free of the pathological behaviour associated with 'essential' singularities such as Kronecker delta functions, Dirac delta functions, step functions and so on. *Maximal* analyticity means that there are only such (isolated) singularities as are demanded by unitarity. Though they are assumed to be functions of complex variables, it is the real boundary values of the S-matrix that are taken to represent the physically meaningful, measurable quantities.

Maximal analyticity can be put to good use in describing physical processes, and it is at this point that we begin the process of actually writing down a workable expression for the amplitude. This is because if the physical parameters are expressed as the real limits of complex functions, and if the singularities in the complex plane are isolated and identifiable, then the amplitude $A(z)$, where z are a set of Lorentz-invariant complex variables, can be expressed via Cauchy's theorem as

$$A(z) = \frac{1}{2\pi i}\oint_C \frac{dz'}{z'-z}A(z'), \qquad \text{(A1.5)}$$

where the contour loops around all the singularities (poles and branch cuts). To see what work Cauchy's theorem can do in producing an explicit expression for the amplitude, suppose for concreteness specify that we are interested in

[6]Analyticity of the *second* kind refers analyticity in angular momentum variables, and we shall discuss it at length shortly.

computing the S-matrix for a $2 \to 2$ reaction

$$\alpha + \beta \to \gamma + \delta \tag{σ}$$

and refer to this as 'Reaction σ'. We must first define the variables appropriate to this reaction. Lorentz invariance compels us to use relativistic invariants as the variables of the S-matrix, so that the amplitude is a function of the p_i^2. Three obvious candidates present themselves, namely, the square of the CM energy, and the two squared functions of momentum transfer in that frame. We thus define the 'Mandelstam variables' s, t and u as follows:

$$s = (p_\alpha + p_\beta)^2 = (p_\gamma + p_\delta)^2$$

$$t = (p_\alpha - p_\gamma)^2 = (p_\beta - p_\delta)^2$$
$$u = (p_\alpha - p_\delta)^2 = (p_\beta - p_\gamma)^2.$$

It is easy to see that only two of these are independent, since $s + t + u = \Sigma m_i^2$, where the m_i are the masses of the external particles. We may thus consider A as $A(s,t)$, or as a function of any other pair of these variables. Since s represents the total energy of the reaction, we say that s is in its 'physical region' whenever $s \in \mathbb{R}$ and $s \geq max\{(m_\alpha + m_\beta)^2, (m_\gamma + m_\delta)^2\}$. The other two variables represent the 4-momentum transfers and are always real and negative in the s-channel physical region.[7]

Looking again at the one-particle contribution to the amplitude (A1.4b), we can now replace $\delta((p_1+p_2)^2 - m^2)$ with $\delta(s - m^2)$. However, now that we have introduced the postulate of maximal analyticity, so defined, it is *prima facie* very worrying that the unitarity equations should feature any such δ-functions at all. We know that there is a δ-function wherever there is an on-mass shell particle or particles which conserve the energy-momentum and quantum numbers of the reaction in question, but such singularities are apparently forbidden by the postulate of maximal analyticity. However, this worry is dispelled by a trick from complex analysis. There is a well-known relation for

[7]'In general, those two [Mandelstam] variables that for a particular channel are not the squares of the total energy may be interpreted as the squares of the momentum transfer and have physical regions that extend to -∞'(Chew [1962], p12).

a point lying close to the real axis in the complex plane, $z = z_r \pm i\epsilon$: it is

$$\frac{1}{z \pm c} = P\frac{1}{z_r - c} \mp i\pi\delta(z_r - c),$$

for an arbitrary point c, where P denotes the 'principal part'.[8] Now re-cast this expression into terms that are relevant here:

$$\frac{1}{s - m^2} = P\frac{1}{s_r - m^2} + i\pi\delta(s_r - m^2), \tag{A1.5b}$$

Comparison with (A1.4b) shows that a delta function of this sort in the imaginary part of the amplitude – that given in the LHS of the unitarity equations – corresponds to a simple pole $\frac{\rho}{s-m^2}$ in the total (i.e. real plus imaginary) amplitude, with residue given by

$$\rho = \langle P_\gamma, P_\delta | A^\dagger | Q \rangle \langle Q | A | P_\alpha, P_\beta \rangle. \tag{A1.5c}$$

This residue can be taken to be the product of the two coupling constants for the interaction so that $\rho = g_{\alpha\beta\Sigma} g_{\Sigma\gamma\delta}$ (where Σ is the intermediate particle).[9] To make this dependence explicit, we now abbreviate ρ by g^2. And although in this part we are generally neglecting spin, it will be useful for future reference to note that, where the spin of the particle corresponding to the pole is included, we have[10]

$$\rho = |g^2| P_\ell(\cos(\theta)). \tag{A1.5d}$$

Thus we have reached our first substantial result: the one-particle intermediate states which appear in the unitarity equations correspond to simple poles in the amplitude. This is the origin of the S-matrix 'pole-particle correspondence'.[11] When more than one particle is produced, we find not simple poles at specific mass points but rather *branch-cut* singularities. These are 'cuts' on the complex plane corresponding to the occurrence of a many-particle state or 'channel' with the appropriate quantum numbers. These cuts run from the

[8] See Jacob and Chew [1964] p21; Joachain [1975], p295.
[9] See Martin and Spearman [1970], p287.
[10] See Martin and Spearman [1970], p287. That the residue is factorizable like this is due to the short range of the strong interactions.
[11] See Cushing [1990], p109.

energy threshold of the channel to infinity, corresponding to the unlimited centre-of-mass energies which can be associated with the state consistent with all internal particles being on-mass-shell.

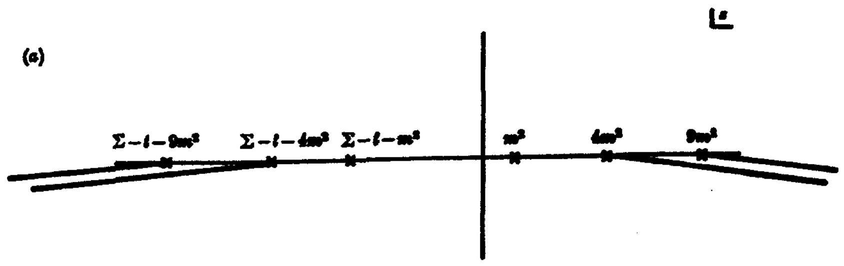

Figure 11.1: Pole and branch cuts on the s-axis (from Collins [1977], p30.)

We know, then, that Reaction σ will have singularities (poles and branch cuts) on the s axis at the values of energy wherever there exists a particle or particles with (combined) mass equal to the CM energy of the reaction and with the combined quantum numbers of particles α and β. But as we have seen, s is not the only variable required to completely specify in the amplitude, for there are *two* independent variables in play. We can therefore not be sure that by considering s alone we are uncovering all the singularities for Reaction σ. Nonetheless, how exactly it is that the other Mandelstam variables could contribute to the singularity structure is not at all clear, since the δ-function in the unitarity equation above, (A1.4b), ostensibly involves the s variable alone. It is at this point that we can again put analyticity to good use, by using it to derive the *crossing principle* from which a great deal of physical information can be extracted.

A2. The Crossing Principle

The crossing principle states that the same amplitude governs reactions that are related by repeated 'crossing': that is, reactions that may be obtained from one another by the interchange of incoming particles with outgoing antiparticles. By borrowing some concepts gleaned from Dirac theory, I will try to bring out the motivation for this principle, and will do so by means of the following (informal) argument.

Consider again Reaction σ above. It is clear that the scattering amplitude for this reaction be expressed (provisionally) as $A^\sigma(p_\alpha, p_\beta, p_\gamma, p_\delta)$ (where I say 'provisionally' because the momenta must be recast into Lorentz invariants). Let us now add to the incoming particles a γ particle–anti-particle pair, with 4-momenta as follows:

$$\alpha(p_\alpha) + \beta(p_\beta) + \gamma(p_\gamma) + \overline{\gamma}(-p_\gamma) \to \gamma(p_\gamma) + \delta(p_\delta). \qquad (\sigma^*)$$

We can do the same to the outgoing state with a β particle–anti-particle pair: we obtain

$$\alpha(p_\alpha) + \beta(p_\beta) + \gamma(p_\gamma) + \overline{\gamma}(-p_\gamma) \to \gamma(p_\gamma) + \delta(p_\delta)+) + \beta(p_\beta) + \overline{\beta}(-p_\beta). \quad (\sigma^{**})$$

If we adhere to the Dirac idea that a particle–anti-particle pair with equal and opposite 4-momentum in some sense amounts to 'nothing' (i.e. the vacuum), then in both cases these additions add nothing to the incoming and outgoing states. So Reactions σ, σ^* and σ^{**} can be regarded as 'equivalent'. Furthermore, since the same idea implies that a general reaction $\alpha+\beta \to \gamma+\delta$ is equivalent to the reaction $\alpha + \beta + E \to \gamma + \delta + E$, where E is any set of particles whatsoever, then Reaction σ^{**} is equivalent to

$$\alpha(p_\alpha) + \overline{\gamma}(-p_\gamma) \to \delta(p_\delta) + \overline{\beta}(-p_\beta). \qquad (\tau)$$

By transitivity, σ and and τ are 'equivalent' too. Generalizing in the obvious way we thus obtain a *simple rule* that, given any reaction, it is possible to obtain another reaction equivalent to the first simply by interchanging ingoing particles with 4$-$momentum p with outgoing anti-particles with 4-momentum $-p$ (and vice versa). This process is called 'crossing'. Therefore, if we introduce an amplitude $A^\tau(p_\alpha, p_{\overline{\gamma}}, p_{\overline{\beta}}, p_\delta)$, then we must have that

$$A^\sigma(p_\alpha, p_\beta, p_\gamma, p_\delta) = A^\tau(p_\alpha, -p_\gamma, -p_\beta, p_\delta). \qquad \text{(A2a)}$$

However, we must be *very* careful in interpreting both Reaction τ and the stated equivalence (A2a) between the amplitudes for crossed reactions. The difficulty stems from the all the asymptotic states involved in reactions in this theory are supposed to be *observables*, but if the particle energies in Reaction σ are all positive then the energies of the particles $\overline{\beta}$ and $\overline{\gamma}$ in Reaction τ

cannot be. Therefore (A2a), despite its formal simplicity, has no *physical* meaning as it stands, for 'it is impossible that the two members correspond together to a physical situation'.[12]

This impasse is broached by invoking analyticity. The basic principle that (A2a) establishes is that the same *function* governs the amplitude for the two processes. If we assume that that function is maximally analytic, we should be able to find a way to *analytically continue* the $-p_\beta, -p_\gamma$ variables in the momentum (i.e. s, t, u) plane until the energy components acquire positive, and hence physically meaningful, values.[13] So let us suppose we can do that for the above reaction. That is, let us analytically continue and put $-p_\beta \mapsto p_\beta$ and $-p_\gamma \mapsto p_\gamma$, where the '$\mapsto$' indicates not just replacement but replacement *by analytic continuation*. Then we shall write $A^X|_{R(Y)}$ to indicate that we are evaluating the amplitude for the reaction X in the physical region for reaction Y: that is, in the region in which all the particles which feature in Y have positive energies. Then

$$\begin{aligned}
A^\tau(p_\alpha, p_{\overline{\gamma}}, p_{\overline{\beta}}, p_\delta)|_{R(\sigma)} &= A^\tau(p(\alpha), -p(\gamma), -p(\beta), p(\delta))|_{R(\sigma)} \\
\mapsto A^\tau(p(\alpha), p(\gamma), p(\beta), p(\delta))|_{R(\tau)} &= A^\tau(p_\alpha, p_{\overline{\gamma}}, p_{\overline{\beta}}, p_\delta)|_{R(\tau)}
\end{aligned}$$

so that under this continuationthe input $\overline{\gamma}$ in Reaction τ takes on the momentum of the output γ in Reaction σ; and so for the $\overline{\beta}$. In terms of the Mandelstam variables above, this transformation gives

$$\begin{aligned}
s &= (p_\alpha + p_\beta)^2 \mapsto (p_\alpha - p_\beta)^2 \\
t &= (p_\alpha - p_\gamma)^2 \mapsto (p_\alpha + p_\gamma)^2 \\
u &= (p_\alpha - p_\delta)^2 \mapsto (p_\alpha - p_\delta)^2.
\end{aligned}$$

We can therefore see that the analytic continuation from negative to positive values of the momenta in the 'crossed' reaction has resulted in a permutation of the roles of the Mandelstam variables in the continued-into region of the plane: now it is t that represents the CM energy of Reaction τ, and s and u the momentum transfers. Perfectly analogously to the previous case, the region of the s, t, u plane in which $t \geq (m_\alpha + m_{\overline{\gamma}})^2$ and $s, u \leq 0$ constitutes the physical region for Reaction τ.

[12] Omnès and Froissart [1963], p62.
[13] For the definition of analytic continuation, see Cushing [1975], Section 7.11.

We can, of course, play the same game with another crossed reaction that can be obtained from Reaction σ. Expressed in terms of the momenta of Reaction σ, this is Reaction υ:

$$\alpha(p_\alpha) + \overline{\delta}(-p_\delta) \to \overline{\beta}(-p_\beta) + \gamma(p_\gamma) \qquad (\upsilon)$$

We can then continue the momenta of the crossed particles to take equal but opposite values in the physical region for Reaction υ (which is the just region of the s, t, u plane in which $u \geq (m_\alpha + m_{\overline{\delta}})^2$ and $s, t \leq 0$). The physical regions for the three reactions are always disjoint.

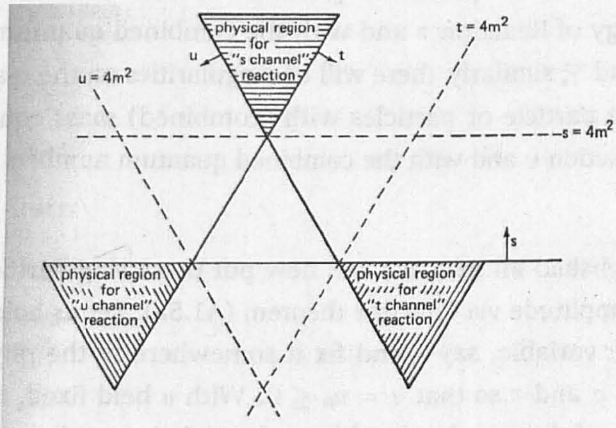

Figure 11.2: Mandelstam diagram for equal mass elastic scattering, showing physical regions for each channel. (From Joachain [1975], p33.)

Analytic continuation between positive and negative momenta, then, permits us to link the amplitudes for crossed reactions in a physically meaningful way. The amplitude for each reaction is given by *one and the same function*, evaluated in the physical region for that reaction. In Chew's words,

> These different reactions are distinguished by the sign of the energy variables, which are positive or negative according to whether ingoing or outgoing particles are involved, but if the controlling amplitude is known for one reaction it can be obtained from the two others by smooth extrapolation in energy.[14]

[14] Chew [1964a], p32.

A3. The Mandelstam Representation of the Amplitude

Having established that one and the same amplitude governs reactions related by repeated crossing, we now resume contact with the problem of actually *finding* the amplitude. We saw in Section A2 that the amplitude in a given channel, say that for Reaction σ, is a function of the singularities on the s-axis corresponding to the creation of new bound states in that channel. Combining Sections 2 and 3, we know that there will also be singularities on the t axis wherever there exists a particle or particles with (combined) mass equal to the CM energy of Reaction τ and with the combined quantum numbers of particles α and $\overline{\gamma}$; similarly, there will be singularities on the u-axis wherever there exists a particle or particles with (combined) mass equal to the CM energy of Reaction υ and with the combined quantum numbers of particles α and $\overline{\delta}$.

Having established all that, we can now put these singularities to work in finding the amplitude via Cauchy's theorem (A1.5a). Let us hold one variable fixed – the u variable, say – and fix it somewhere in the physical regions of Reactions σ and τ so that $u = u_0 \leq 0$. With u held fixed, the amplitude is a function of the s and t variables only, and since only two of s, t, u are independent, we have only one independent variable here. (This is why I chose one variable fixed to start with.) We know that s will have a pole at $s = s_p \equiv m_s^2$, the point at which there is one particle with the right mass and quantum numbers for Reaction σ, and that there will also be a branch point at s_b, the first value at which a plurality of particles collectively meet these criteria. As new such sets of particles are produced as t increases, these branches will then be superimposed on top of segments of the branch starting at s_b . Likewise, there will be poles in t at $t_p = m_t^2$, and branch points at the corresponding values t_b for Reaction τ. Such singularities may be shown graphically as a function of s alone, since with u fixed the t singularities can be expressed in terms of s.

The Cauchy integral equation for the amplitude as a function of s is now evaluated over a closed contour in the complex plane of the independent variable such that A is holomorphic (singularity free) inside and on that

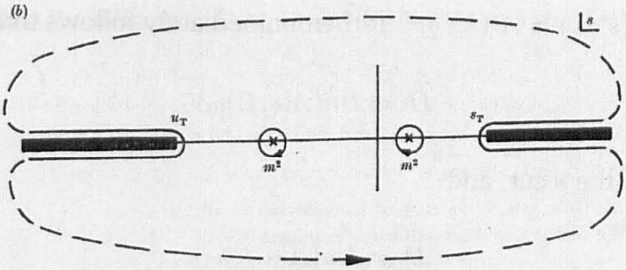

Figure 11.3: The intergration contour in the complex s plane, expanded to infinity but enclosing the poles and branch cuts. (From Collins [1977], p30.)

contour. We then expand the contour so that it encircles the poles and encloses the branch cuts. Doing so means that we can then write

$$A(s,t,u_0)=\frac{g_s^2}{m_s^2-s}+\frac{g_t^2}{m_t^2-s}+\frac{1}{\pi}\oint_C A(s,t,u_0),$$

where C is the large contour. Here $A(s,t,u)$ is a new notation for $A_{ij}=\langle j|A|i\rangle$ with $|i\rangle=|p_\alpha,QN_\alpha\rangle|p_\beta,QN_\beta\rangle$ in the region where s represents the square of the total momentum (and the other variables the momentum transfer), and with $|i\rangle=|p_\alpha,QN(\alpha)\rangle|p_{\overline{\gamma}},QN(\overline{\gamma})\rangle$ in the region where t represents the square of the total momentum (and so on). The residues g^2 of the poles are given by the same means as at (A1.5c). The rather drastic assumption is then typically made at this point (though it shall later be renounced): it is assumed that $A(s,t,u)\to 0$ as $s\to\infty$, so that the contribution from the circle disappears. We are then left with the *discontinuities* of the amplitude across the right and left-hand cuts respectively, defined by

$$2iD_s\equiv A(s_+,t,u_0)-A(s_-,t,u_0),$$

where $s_\pm=s\pm i\epsilon$, and

$$2iD_t\equiv A(s,t_+,u_0)-A(s,t_-,u_0).$$

The amplitude will be given in the limit that ϵ tends to zero. Now, given that the *Schwartz reflection principle* holds as a consequence of analyticity, we know that $A(s^*,..) = A^*(s,..)$.[15] It then immediately follows that

$$D_s = Im(A(s,t,u_0)$$

evaluated on the s cut, and

$$D_t = Im(A(s,t,u_0)$$

evaluated on the t cut. We can now write out the amplitude fully as

$$A(s,t,u_0) = \frac{g_s^2}{m_s^2 - s} + \frac{g_t^2}{m_t^2 - s} + \frac{1}{\pi}\int_{s_b}^{\infty} \frac{\mathfrak{Im}A(s',t,u_0)}{s'-s}ds' + \frac{1}{\pi}\int_{t_b}^{\infty} \frac{\mathfrak{Im}A(s,t',u_0)}{t'-t}dt'. \tag{A3a}$$

This is the 'Mandelstam representation' of the amplitude. Here, the first two terms on the RHS correspond to the particle poles, and the last two to the branch cuts on the s and t axes. Note that since the imaginary parts of the amplitude obtained from the discontinuities are evaluated in the physical region of the appropriate variable, we can use the unitarity equations for the corresponding reactions in order to find them. These equations are therefore 'in principle' sufficient to determine the amplitude, given the particle poles. Now, as can be appreciated by looking again at (A1.4d), 'the unitarity equation' for any given reaction is actually best regarded as an infinite set of coupled non-linear equations, and is therefore impossible to solve exactly. But it nonetheless turns out that the analyticity postulate may tested in special cases, even in the absence of an exact solution of these equations in the general case. In particular, if we consider reactions, such as $\alpha + \beta \rightarrow \alpha + \beta$, that are elastic in two channels (let them be s and t), and set the Mandelstam variable in the remaining channel (hence u) equal to zero so that we are looking at zero momentum transfer or 'forward' scattering, we can use the *optical theorem*, namely that

$$\sigma_s^{tot} = \frac{1}{2\vec{p}_s\sqrt{s}} ImA(s,0)$$

$$\sigma_t^{tot} = \frac{1}{2\vec{p}_t\sqrt{t}} ImA(t,0)$$

[15] For proof of this Schwartz reflection principle, see e.g. Joachim [1975], p293.

to obtain empirically a value of the imaginary part of the amplitude.[16] (Here, $\vec{p_s}$ is the 3-momentum of the s-channel particles as measured in the CM frame for Reaction σ, and analogously for $\vec{p_t}$.) By plugging in the measured values of the cross sections, it is possible to subject the amplitude for these sorts of elastic reactions to 'exhaustive test'.[17] Regarding the case of pion-nucleon scattering at zero momentum transfer,

> experiment confirms to very high precision [the amplitude] as corrected to take into account the nucleon spin. Hence there is every reason to believe that at least in this well-defined instance the scattering amplitude has no singularities in energy than those we have found.[18]

Though maximal analyticity enters as a postulate, and indeed would appear purely *mathematical* in nature, it nevertheless seems to be susceptible to a degree of empirical support. It is also this postulate that ushers the concept of *force* into the analysis, as I will now try to explain.

A4 The Interpretation of the Amplitude

A4.1 The Physical Interpretation (1): Forces

The Mandelstam representation of the amplitude, (A3a), gives the amplitude for the s and t channel reactions at fixed u. To proceed, let me introduce a final bit of jargon: when we are in the physical region of the Mandelstam plane for a given reaction, say Reaction σ, we say that s is the 'direct channel', and that both t and u are *crossed* channels. When we shift to the t-channel physical region, it is now t that is the direct channel and s, along with u, is a crossed channel. The essential point behind the Mandelstam representation is that *the amplitude for the direct channel reaction is always a function of the singularities in the crossed channels*. We have so far seen how this symmetry arises formally, but now we would like to interpret it. After all, that the

[16] See, e.g. Chew [1966], Section 5.3; see Collins [1977], Section 1.9 for the derivation of the optical theorem.
[17] Chew [1966], p37.
[18] Omnès [1971], p297.

amplitude for the direct channel process should be a function of the particle production processes in that channel is hardly surprising; why *physically* the amplitude for this reaction receives an equally important contribution from the crossed channels is much more surprising.

The physical story told here rests upon a comparison of (A3a) with the *Born approximation*, as it appears in quantum field theory, to scattering under the influence of a Yukawa potential. The Yukawa potential generated by the exchange of a single particle of mass μ is

$$U(r) = U_0 \frac{e^{-\mu|r|}}{|r|},$$

where μ^{-1} is the 'range' of the interaction.[19] This gives as the first contribution in the Born approximation to the scattering amplitude a term

$$A^B = \frac{U_0}{t - \mu^2}, \tag{A4.1a}$$

where t is the momentum transfer.[20] When we are dealing with many-particle exchange in field theory, the potential is given by a *superposition* of Yukawa potentials, which is expressed by

$$U(r) = \frac{1}{|r|} \int_{\mu_0 > 0}^{\infty} \rho(\mu) e^{-\mu|r|} d\mu^2,$$

where $\rho(\mu)$ is a weight function and μ_0^{-1} again represents the range.[21] This potential gives a contribution

$$A^B(s,t) = \int_{\mu_0}^{\infty} \frac{\rho(\mu)}{t - \mu^2} d\mu \tag{A4.1b}$$

to the Born approximation to the amplitude.

Comparing (A4.1b) with the cut terms in (A3a), we can agree with Chew that the latter 'looks like the Born scattering due to a superposition of Yukawa potentials, where the range is $1/\sqrt{t}$'.[22] We can say the same sort of thing

[19]Joachain [1975], 172.
[20]Chew [1962], p31 (see also p12; Joachain [1972], Section 8.6.1).
[21]See Joachain [1972], Section 8.6.2 for full details.
[22]Chew [1962], p31.

when we compare (A4.1a) with the pole terms in the same equation. Now, the 'customary interpretation' of the Yukawa interaction is in terms of *particle exchange*. And it is trivial to show that the particles produced in the crossed channels have precisely the right quantum numbers to be exchanged between the input (or output) particles in the direct channel consistently with the quantum number conservation laws.[23] *Via this explicit analogy* with the (rival) QFT programme, then, we can therefore interpret the contributions of the crossed channels to the amplitude for a reaction producing a bound state in the direct channel as follows: the input particles in the direct channel *exchange* the intermediate states of the crossed channel. As Chew put it,

> The unphysical singularities [i.e., the singularities that occur outside of the physical region of the direct channel process under consideration] of an elastic-scattering amplitude correspond to the systems that can be 'transferred' between the particles undergoing scattering. Only by such transfers can a force be transmitted, and it is well-known that, according to the uncertainty principle, the range of the force is $\approx 1/E$, if E is the total energy necessary to create the transferred system.[24]

The cross channel terms thus provide the interaction *potentials*. The strength and range of the potentials will be controlled by the discontinuity functions D_t and D_u and hence by the imaginary parts on the RHS of (A3a). The longest range potentials will come from the exchange of the lightest systems permitted by cross channel unitarity.[25]

It seems that we can agree, then, that there is a real sense in which 'cross-

[23] 'The s-channel ($ab \rightarrow cd$) and the t-channel ($a\bar{c} \rightarrow \bar{b}d$) [are] processes which are related by crossing... The particles exchanged in the process $ab \rightarrow cd$ must have the quantum numbers of $a\bar{c}$ and $\bar{b}d$' (Collins and Martin [1984], p67). For example, in our channel t above the quantum numbers of the intermediate state are $QN_a + QN_{\bar{c}} \equiv QN_f$. If particle a in the s channel emits particle f and in doing so turns into (say) output state c, then we must have $QN_a = QN_f + QN_c$. But we know that $QN_a + QN_{\bar{c}} = QN_f$; so $QN_a = QN_f - QN_{\bar{c}} = QN_f + QN_c$, as required. Analogous stories can be told about particle b emitting the u-channel particle and turning into d; particle a emitting and turning into d instead of c, etc. Thus it is always the case that the cross channel particles have the right quantum numbers for exchange in the direct channel.

[24] Chew [1962], p7.

[25] See Chew [1962], p31.

ing symmetry determines the forces' in S-matrix theory.[26] Note that such a connection can be forged is highly non-trivial: given that the S-matrix deals explicitly with asymptotic states alone – that is, those in which no interactions are present – it is not at all obvious that such a correspondence, and thus a desription of short-range interactive forces, could ever have been achieved.[27]

A4.2 The Physical Interpretation (2): Chew and the Reciprocal Bootstrap

As I have noted, there is a formal symmetry operating between the three channels in the Mandelstam representation of the amplitude (equation (A3a)).[28] Any Mandelstam variable can be held fixed giving the amplitude for a given reaction in terms of the other two variables; we can then rotate around the Mandelstam plane to take either variable out of its physical region and into that of the other channel so that the amplitude for *that* reaction may in turn be computed. It then follows from the definition of the 'direct' and 'crossed' channels that the two roles may be interchanged in this way, and – given the discussion above – this in turn seems to mean that the role of the *intermediate bound state* and that of the *force-carrying particle* are likewise interchanged.

Let us therefore rotate around the plane and permute the roles of the s and t channels. Placing the variables in the physical region for Reaction τ, it is now the s-channel terms in the amplitude which correspond to Yukawa forces, and (A3a) may now be interpreted as a formula expressing that the s-channel particle fuels the binding of the hadron T in the t channel. This combination of force generation by particle exchange and Mandelstam symmetry lies at

[26] Omnès [1971], p299.

[27] Of course, the argument offered above – and that Chew offered – borrows explicitly from QFT, and thus it is perhaps not so surprising that such a correspondence could be forged.

[28] As Cushing puts it, 'The representation of [the general equation for the amplitude above] places s, t and u each on the same footing and allows one easily to 'cross' from one channel (or reaction) to another just by allowing s (or t or u) to play the role of, say, the 'energy' variable [i.e. serve as the 'direct' channel]...' (Cushing [1990], p121.)

the heart of the S-matrix dynamical scheme. Chew's own (and oft-repeated) summary of the dynamics goes as follows.

> The forces producing a certain reaction are due to the intermediate states that occur in the two 'crossed' reactions belonging to the same diagram. The range of a given part of the force is determined by the mass of the intermediate state producing it, and the strength of the force by the matrix elements connecting that state to the initial and final states of the crossed reaction. By considering all three channels on this basis we have a self-determining situation. One channel provides forces for the other two – which in turn generate the first.[29]

Thus Chew arrives at his 'reciprocal bootstrap': bound-state particles of type Σ depend on bound-state particles of type T to bind them together *and vice versa*. In this way, the formal symmetry that exists between the Mandelstam variables finds an ontological expression, namely, in a *symmetry of dynamical dependence* between the particles produced in each channel.

Part A2. Extension of the Analytic S-matrix

The results so far achieved – both in terms of arriving at an explicit expression of the amplitude and in terms of physically interpretating it – are highly non-trivial. Nonetheless, the story that I have told so far cannot be the whole story. Two reasons present themselves as to why the preceding analysis should and must be supplemented. First of all, the failure to incorporate *partial wave analysis*, familiar from non-relativistic scattering theory, represents a conspicuous absence. S-matrix theory is, after all, a theory based entirely on the properties of the scattering matrix; it would thus be almost unthinkable not to take advantage of the powerful methods that partial wave analysis facilitates for deducing them. However, it turns out that when we analyze the scattering amplitudes in terms of partial waves, crossing symmetry will not be respected if the principles already governing the S-matrix are not augmented. Secondly, the derivation of the Mandelstam representation above relied on unrealistic assumptions about the *convergence* of the amplitude that will have

[29]Chew [1962], p32.

to be relinquished. This assumption regarding convergence is critical in this context since, where that assumption is not fulfilled, the method of *imposing* convergence brings with it arbitrary parameters which are antithetical to the S-matrix's 'bootstrap' philosophy, in which every particle's parameters are fixed by those of the particles in the relevant crossed channels (see Chapter 5, Section 3). This clearly another problem that must be addressed.

There are thus two reasons why we need to continue the story: first, because we must utilize partial waves, though this will bring in its wake a serious problem regarding crossing; second, because we must make a more realistic convergence assumption even though that will cause problems for the S-matrix theory's 'bootstrap' philosophy as well. Surprisingly, however, it turns out that the same postulate of *maximal analyticity of the second kind* will solve both of these problems in one swoop, since it allows for the construction of crossing-symmetric partial wave amplitudes *and* prohibits the undetermined parameters associated with the method of imposing convergence. Together with the fact that it represents a natural extension of the analyticity postulate already present, these features suggest that this postulate should become an essential part of the S-matrix toolkit. I begin, then, with articulating the first problem that occasions its inclusion, and that is the incorporation of partial wave analysis. To keep things simple, I will focus on bosons – and hence states of integer ℓ – throughout.

A5.1 Problems of Convergence (1): The Partial Wave Series

Following Wigner's work on the rotation group, there is no ambiguity about the definition of the partial wave reaction amplitudes for states of well-defined angular momentum ℓ: the partial wave projection rule, in a notation most familiar to physicists, is

$$A_\ell(E) = \frac{1}{2}\int_{-1}^{1} dz P_\ell(\cos(\theta)A(E, \cos(\theta)).$$

Replacing E with the s – the square of the CM energy in the direct channel – and using the relation appropriate to equal-mass scattering (which I will assume for simplicity)[30]

$$\cos(\theta_s) = 1 + \frac{2t}{s - 4m^2} \equiv z_s(t)$$

where θ_s is the angle of scattering in the s-channel CM frame. We thus obtain

$$A_\ell(E) = \frac{1}{2}\int_{-1}^{1} dz P_\ell(\cos(\theta)A(E, z_s(t)). \qquad \text{(A5.1a)}$$

The partial wave expansion of the scattering amplitude may then be written as

$$A(s, z_s) = \sum_{\ell=0}^{\infty}(2\ell + 1)A_\ell(s)P_\ell(z_s). \qquad \text{(A5.1b)}$$

This expression can be shown to be convergent and hence well-defined in the s-channel's physical region (defined by $s \geq 4m^2$ and $-1 \leq z_s \leq 1$). The question arises as to whether it is also well-defined in a domain large enough to contain the physical regions of all the Mandelstam variables – for only if this is answered in the affirmative will it be able to incorporate crossing symmetry. But it is clear however that it is *not*. Consider for example the t-channel singularities. Whilst the s-channel singularities may be contained in the partial-wave amplitudes $A_\ell(s)$, the t-dependence can be contained only in the Legendre polynomials $P_\ell(z_s)$; but these are *entire functions* – that is, holomorphic (singularity free) over the entire complex plane. Thus the singularities in t can manifest themselves only in the divergence of the series, which would render it mathematically senseless.[31] Since crossing symmetry demands that if there are singularities in the s-channel there must be singularities in the t-channel, and these singularities are bought only at the price of rendering it mathematically ill-defined, this clearly represents a significant problem for the theory.

But all is not lost.[32] First of all, it can be shown that, assuming ℓ to be real, the 'domain of convergence' of $A(s, z_s)$ is actually a bit larger than the s-channel physical region: it is in fact an elliptical region with foci at $+1$ and -1 (the 'Lehmann ellipse'). Nevertheless, it remains that the amplitude cannot be continued to regions in which the crossed variables take on *arbitrarily* large

[30] See Collins [1977], p21 for the (straightforward) derivation.
[31] This argument is adapted from Barone and Predazzi [2002], p87.
[32] See Barone and Predazzi [2002], Section 5.3.

values. However, if we assume that we can let the amplitude take on *complex* ℓ values, and consider the extreme case in which the angular momentum is purely imaginary, the domain of convergence as $\ell \to \infty$ is an open domain: in particular, a hyperbola with foci at $z = \pm 1$. This hyperbola overlaps with the Lehmann ellipse, and this guarantees that if we can continue the partial wave expansion of the scattering amplitude to imaginary ℓ values, the new expansion will represent the same analytic function (i.e. the same scattering amplitude) in a domain in which either crossed channel *can* become arbitrarily large.[33]

It is thus the extension of the amplitude (A5.1b) to become a function of partial waves with complex angular momentum which is the key to creating a crossing-compatible amplitude. The proposed new analytic partial wave functions must of course coincide with $A_\ell(s)$ at physical (non-negative integral) ℓ values. By Liouville's theorem, unless these functions are constant they must have singularities in the complex plane, and these singularities of the partial wave amplitudes are called 'Regge poles'.[34] All of these poles may be shown to trace out continuous paths parameterized by the energy, so that we may write $\ell = \alpha(s)$, where $\alpha(s)$ represents the 'moving' Regge pole.[35] The path traced out in angular momentum space by the poles as s varies is called the 'Regge trajectory'. 'Regge theory' as a whole denotes the incorporation of the analytic continuation of angular momentum into the complex plane into scattering theory. We will see how introducing Regge theory into the picture connects up with anti-fundamentalism later on, and momentarily how these analytic amplitudes were constructed. But before that, let us recall the second problem mooted above.

[33] Barone and Predazzi [2002], p86.

[34] They may also be cuts, but to stop this discussion getting more complicated still I shall focus exclusively on the poles.

[35] See Gribov [2003], Section 3.3 for discussion, and also Barone and Predazzi [2002], p84.

A5.2 Problems of Convergence (2): The Mandelstam Representation

In the course of deriving the Mandelstam representation of the amplitude in Section A3, I assumed that the amplitude tended to zero in the asymptotic region of the direct channel energy. This assumption, however, is not in general in agreement with experience, for what *is* experienced is instead a power-bound asymptotic behaviour. More precisely, what is found is that, as one variable (say s) is held fixed, while a crossed channel (say t) is taken off to infinity, t is bounded by a finite power, a power which is moreover a function of s. Thus as $t \to \infty$ with s fixed, we have $A(s,t) \to Ct^{\lambda(s)}$ for some constant C, and $\lambda(s) < \infty$.[36] Given this much slower convergence than was originally assumed, the integrals obtained above in the Mandelstam expression for the amplitude (A3a) will diverge, and the method for deriving the Mandelstam representation must be revisited as a result.

The way that convergence is restored is by *the method of subtractions*, and I will briefly go through what is involved here. Given that $A(s,t) \to t^{\lambda}$ as $t \to \infty$ and that the problem resides in the power of this divergence, we know that we *can* write a dispersion relation for

$$\frac{A(s,t)}{(t-t_1)...(t-t_N)}$$

where N is the smallest integer greater than λ, since clearly *this* disappears asymptotically. Ignore for a moment the other terms in the Mandelstam amplitude, so that we can write simply

$$A(s_0,t) = \frac{1}{\pi}\int_{t_b}^{\infty} \frac{D_t}{(t-t')}dt'.$$

(The other cut can be treated analogously, and the poles are unaffected.)

[36]See Chew [1966], p50 for a discussion of the experimental evidence.

Then for the subtracted version of this amplitude, we have

$$A(s_0,t)\prod_{i=1}^{N}(t-t_i)^{-1}=\sum_{j=1}^{N}\frac{A(s_0,t_j)}{t-t_j}\prod_{i=1,i\neq j}^{N}(t_j-t_i)^{-1}+\frac{1}{\pi}\int_{t_b}^{\infty}\frac{D_t(s_0,t')}{(t'-t_1)...(t'-t_N)(t'-t)}dt'$$

where the first term on the RHS comes from the contributions of each of the poles at $t=t_1,...,t_N$. Hence we obtain

$$A(s_0,t)=\sum_{n=0}^{N-1}c_n(s)t_s^n+\prod_{i=1}^{N}(t-t_i)^{-1}\frac{1}{\pi}\int_{t_b}^{\infty}\frac{D_t(s_0,t')}{(t'-t_1)...(t'-t_N)(t'-t)}dt' \tag{A5.2a}$$

where $\sum_{n=0}^{N-1}c_n(s)t_s^n$ is an arbitrary polynomial in t of degree $N-1$.[37] But note that the presence of this polynomial means that the amplitude is not fixed by its discontinuities, hence nor its imaginary part (see Section A3) – and hence not by unitarity. The the divergence problem is apparently thus solved at the expense of introducing an arbitrary polynomial, and thus one which is not determined by the unitarity equations. But this is of course in contradiction with the S-matrix idea that the amplitude can be reconstructed using only the axioms already assumed.

Just as convergence difficulties plagued the familiar partial wave expansion and rendered it unsuitable for crossing, here again it seems that convergence difficulties in the Mandelstam representation appear to undermine the very foundation of S-matrix principles. But just as an analytic extension in angular momentum helps ameliorate the first problem, it also effaces the second. To see how, I will start from the subtracted equation for the amplitude, (A5.2a), and indicate how an analytic expression for the partial wave amplitudes may be derived from it.

A5.3 The Froissart-Gribov Representation

Suppose again that the s-channel is the direct channel and continue to assume that for fixed s the asymptotic behaviour of $A(s,t)$ is $A(s,t)\rightarrow t^{\lambda}$, for some finite $\lambda(s)$. In the new notation, we can write $A(s,z_s)\rightarrow z_s^{\lambda}$ (recall that $z_s =$

[37]See Collins [1977], p31; Barone and Predazzi [2002], p72.

$\cos\theta_s$, a function of t). Consider now the N-times subtracted relation (A5.2a) for $A(s, z_s)$ at fixed s, where N is the smallest integer greater than λ (again poles are neglected, and all subtractions are made at $z_s = 0$ for simplicity). Then we have, where z_0 is the branch point in the z_s variable,

$$A(s_0, z_s) = \sum_{n=0}^{N-1} c_n(s) z_s^N + \frac{z_s^N}{\pi} \int_{z_0}^{\infty} \frac{D_t(s, t'(s, z_s'))}{z_s'^N (z_s' - z_s)} dz_s' + \frac{z_s^N}{\pi} \int_{-z_0}^{-\infty} \frac{D_u(s, u'(s, z_s'))}{z_s'^N (z_s' - z_s)} dz_s'.$$

(The limits on that last term follow because $\cos\theta_s = 1 + \frac{2t}{s-4m^2} = -(1 + \frac{2u}{s-4m^2})$.) Inserting this into (A5.1a), the formula for partial waves, we obtain

$$A_\ell(s) = \frac{1}{2} \sum_{n=0}^{N-1} c_n(s) \int_{-1}^{1} dz_z z_s^n P_\ell(z_s)$$
$$+ \frac{1}{2\pi} \int_{-1}^{1} dz_z z_s^N P_\ell(z_s) \left(\int_{z_0}^{\infty} \frac{D_t(s, t'(s, z_s'))}{z_s'^N (z_s' - z_s)} dz_s' + \int_{-z_0}^{-\infty} \frac{D_u(s, u'(s, z_s'))}{z_s'^N (z_s' - z_s)} dz_s' \right).$$

If we restrict ourselves to $\ell \geq N$, the first term disappears. We now invoke the identity

$$\frac{1}{2} \int_{-1}^{1} dz P_\ell(z) \frac{z^N}{z'^N (z' - z)} = Q_\ell(z'),$$

where $\ell \geq N$ and $Q_\ell(z_s')$ is a Legendre function of the second kind.[38] We then obtain the *Froissart-Gribov* representation for the ℓth partial wave, which is

$$A_\ell(s) = \frac{1}{\pi} \int_{z_0}^{\infty} dz_s D_t(s, t(s, z_s)) Q_\ell(z_s) + \frac{1}{\pi} \int_{-z_0}^{-\infty} dz_s D_u(s, u(s, z_s)) Q_\ell(z_s). \tag{A5.3b}$$

The Froissart-Gribov amplitude shows clearly that, *in the region that ℓ is large enough*, there is no arbitrary polynomial in the corresponding partial wave amplitudes. Hence the discontinuities determine the amplitudes in this region (as is in keeping with the 'bootstrap' spirit). The only singularities of the Q_ℓ in ℓ are poles at the negative integers (beginning with $\ell = -1$) so they are holomorphic in the right-half ℓ plane. They also behave for large z as $Q_\ell \approx Cz^{-(\ell+1)}$, where C is a constant, even for complex ℓ. This ensures that the integrals in the Froissart-Gribov representation exist. It can therefore be used to *define* $A_\ell(s)$ as an analytic function of ℓ, in the region of $\ell \geq N$. In

[38]See Barone and Predazzi [2002], p73.

fact, it can be shown to be defined for all ℓ such that $\Re e(\ell) \geq N$.[39] Hence the Froissart-Gribov representation (A5.3b) is valid for all $\Re e(\ell) \geq N$.

It might be thought that there is no particular merit in this particular continuation since $A(\ell, s)$ only has physical significance for integer ℓ, and so any interpolation between the integers would be of equal value. However, it can be shown (by Carlson's theorem), that given that certain conditions are met by the continued partial wave amplitudes (such as that they tend to zero as $\ell \to \infty$), any continuation is the unique continuation.[40] It can also be shown that the continuation used here satisfies these conditions.[41] hese extended partial wave amplitudes we shall designate not as $A_\ell(s)$ but as $A(\ell, s)$ in order to emphasize the functional dependence on ℓ.

We have now seen that crossing symmetry suggests that the familiar partial wave amplitudes should be complex functions of ℓ, and we have found a representation of these amplitudes which is the unique analytic continuation to complex values. However, we know that it is only valid for $\Re e(\ell) \geq N$, where N is given by the power of the divergence of the Mandelstam representation. Thus it is by no means guaranteed that a partial wave of *arbitrary* ℓ admits this continuation. But, and as we shall shortly see, the question of whether or not a particular partial wave does or does not admit of this analytic continuation is of central importance to the question of the fundamentality of hadrons. I shall therefore continue to press questions regarding the range of ℓ over which the Froissart-Gribov projection is defined.

In order to proceed in pinning down the range of applicability of (A5.3b), another result will be needed. It is that the Froissart bound exists – a bound which puts a critical constraint on the asymptotic behaviour of the amplitude – and I will briefly describe how this is shown now.

A5.4 The Froissart Bound

It can be shown that, for amplitudes which satisfy the Mandelstam representation, unitarity in the direct channel limits the asymptotic behaviour of the

[39]On the above points, see Collins and Squires [1968], p54; Chew [1966], p53; Collins [1977], p64.
[40]Barone and Predazzi [2002], p89.
[41]Barone and Predazzi [2002], p94.

scattering amplitude in the physical regions of the crossed channels (that is, where s is the direct channel, in the regions of t and u with $t, u \leq 0$). The importance of this result cannot be over-emphasized, but I shall not prove it here.[42] Instead, I will simply quote the result. It is that, as $s \to \infty$ with $t \leq 0$,

$$|A(s,t)| \leq C.s \log^2 s,$$

where C is a constant. In the range of energies accessible to experiments, it is very difficult to distinguish a logarithmic dependence on s, and thus the assumption is generally made that such factors can be lumped in to the constant.[43] Thus in the physical region of the s-channel, the amplitude varies at most linearly with s, and cross sections tend to constant values.

This linearity result is very important in this context, as it means that *the power bound λ in the Froissart-Gribov representation can be taken to be as low as 1.* Hence, in the s-channel physical region of the variables at least, the Froissart-Gribov representation is defined for all $\ell > 1$, and there are no essential singularities of the partial wave amplitudes in that region of the complex ℓ plane. This result has been extended to apply all regions of the Mandelstam variables, so that the Froissart-Gribov amplitude is mereomorphic for all $\ell > 1$, and hence can be used to construct analytic partial waves for those ℓ.[44] This means in particular (as discussed in Section A1.5) that there are, in particular, no δ-function-type singularities in this region of the ℓ plane, and all singularities of $\ell > 1$ partial waves are by definition Regge poles (see Section A5.1).[45] This result is important. To see precisely why, we need to incorporate the postulate of *maximal analyticity of the second kind.*

A5.5 Maximal Analyticity of the Second Kind

While we now know that a (unique) analytic representation of partial waves exists for $\ell > 1$, we have not yet determined the status of the amplitude for these very lowest ($\ell = 0, 1$) partial waves.[46] The final question that re-

[42]See, e.g., Collins and Squires [1968], p51.
[43]Collins and Squires [1968], p53.
[44]See references in Collins and Squires [1968], p141.
[45]See also Gribov [2003], p57. See Chew [1966], pp44-45 for the derivational step.
[46]The same is true of the $\ell = 1/2$, but here I am focussing just on bosons.

mains, then, regards the singularities corresponding to these waves. Now, the Froissart-Gribov representation is only *demonstrably* defined for complex $\ell > 1$, since the existence of the Froissart bound cannot be brought to bear below that value. The *assumption* that the partial wave amplitudes admit analytic continuation for *all* ℓ is therefore just that: an assumption. But it is nonetheless an assumption worth making. For a start, there is no obvious reason why the assumption that the partial wave amplitudes should be analytic functions of ℓ is any less natural than in case of the linear momentum variables. Furthermore, such an assumption is required to efface the ambiguity in the Mandelstam representation, as I will now try to show.

Consider again the N-times subtracted dispersion relation (A5.2a) used in deriving the Froissart-Gribov amplitude (recall that the bound-state poles of the amplitude were neglected). The problem was that, since we now have a polynomial of degree $N-1$ in addition to the discontinuities, the amplitude is no longer determined by just those singularities required by unitarity. Now let us see what happens when the underdetermined polynomial is slotted into the Froissart-Gribov representation, so that the partial wave amplitudes are functions of this polynomial (recall too that we are now emphasizing the functional dependence in $A(\ell, s)$:

$$A(\ell, s) = f\left(\int_{-1}^{1} dz P_\ell(z_s) \sum_{n=0}^{N-1} c_n(s) z_s^n\right).$$

Suppose that $N = 1$, so that there is only one undetermined constant and no dependence on z in the polynomial. Then we get

$$A(\ell, s) = f\left(\int_{-1}^{1} dz P_\ell(z_s) c_0\right).$$

The Legendre polynomials are defined as

$$P_\ell(z) = \frac{1}{2^n n!}\frac{d^n}{dx^n}(x^2 - 1)^n,$$

and since the only Legendre polynomial independent of z is that for $\ell = 0$, then using the orthogonality relation

$$\int_{-1}^{1} P_\ell(z) P_{\ell'}(z) dz = \frac{2}{2\ell + 1}\delta_{\ell\ell'} \qquad \text{(A5.5a)}$$

we find that $A(\ell, s) = f(\delta_{\ell 0})$. Thus this subtraction contributes only to the S-wave ($\ell = 0$). Likewise, for $N = 2$ we get a contribution to the amplitude $A(\ell, s) = f(\delta_{\ell 1})$. Therefore, the subtractions, with their attendent undetermined constants, that are needed to ensure convergence in the Mandelstam representation correspond to Kronecker deltas in the ℓ-plane. But *if we postulate maximal analyticity of the second kind*, and hence postulate that there are no delta-function type singularities in the partial waves, then such terms are specifically excluded. *Thus second degree analyticity requires that all the subtraction constants are equal to zero, and the polynomial disappears.* By incorporating this assumption, we can therefore salvage the idea that the asymptotic behaviour of the scattering amplitude is fixed by that of its discontinuities, and hence by unitarity. Even leaving aside the solution it affords to the crossing problem discussed in Section A5.1, this seems justification enough. So let us invoke the sixth and final axiom of the S-matrix to the five adduced in Section A1:

6. *Maximal analyticity of the second kind*: The scattering amplitude has only isolated singularities in ℓ.

With the assumption of the analyticity of $A(\ell, s)$ in place, we can now set about extracting implications from its singularity structure. To do this, I will proceed in what may seem a rather oblique fashion. We first identify the *effect* of the poles in $A(\ell, s)$ on the scattering amplitude $A(s, t) = A(s, z_s)$, and express $A(s, t)$ in terms of these poles via the residue theorem. Armed with this expression for $A(s, t)$, we can then feed this expression back into $A(\ell, s)$ via the relation (A5.1a). This permits us then to uncover the structure of each individual pole in $A(\ell, s)$.

A5.6 Expression of the Amplitude in terms of Complex Angular Momenta

Let us start of by replacing $A_\ell(s)$ in the partial wave series for $A(s, z_s)$, (A5.1b), with its analytically continued partner:

$$A(s, z_s) = \sum_{\ell=0}^{\infty}(2\ell + 1)A(\ell, s)P_\ell(z) \tag{A5.6a}$$

I shall now examine the consequences of its analytic properties on the scattering amplitude, and in particular those of its Regge poles.[47]

First of all we show that this series can be expressed by the integral

$$A(s,z) = \frac{1}{2i}\int_C (2\ell+1)A(\ell,s)\frac{P_\ell(-z))}{\sin\pi\ell}d\ell \qquad \text{(A5.6b)}$$

taken along the contour C in the complex ℓ-plane which encloses all positive integers and (by hypothesis) no Regge poles.

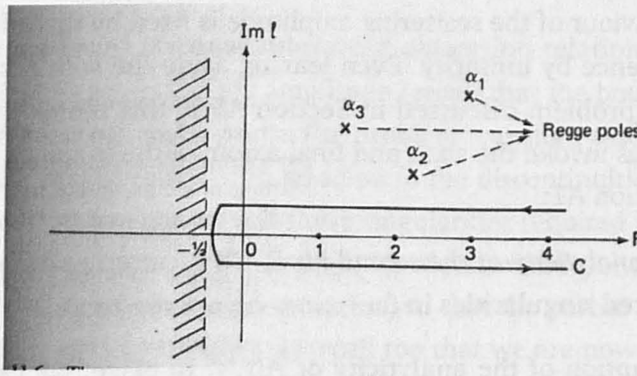

Figure 11.4: The contour C used in the evaluation of the integral on the RHS of (A5.6b). (From Joachain [1975], p265.)

The integral representation is then analytic for $\Re e\ell > -1/2$, [48] except for poles which are

i The Regge poles $\alpha_i(s)$

ii The poles at $\ell = n$ (arising from the vanishing of $\sin\pi\ell$).

The contour C by hypothesis encloses no Regge poles. Therefore, setting

$$g(\ell) = \frac{\pi(2\ell+1)A(\ell,s)P_\ell(-z_s)}{\sin\pi\ell} \qquad \text{(A5.6c)}$$

[47] This presentation is largely adapted from Joachain [1975].

[48] See Martin and Spearman [1970] and below for (brief) discussion of where this value comes from.

we have by the residue theorem

$$
\begin{aligned}
\frac{1}{2\pi i}\oint_C g(\ell)d\ell &= \sum_{n=0}^{\infty} \mathrm{Res}(g(\ell = n)) \\
&= \sum_{n=0}^{\infty} \frac{\pi(n+1)A(n,s)(-1)^n P_n(-z_s)}{\pi(-1)^n} \\
&= \sum_{n=0}^{\infty} (2n+1)A(n,s)P_n(z_s),
\end{aligned}
$$

where I have used $P_\ell(z_s) = -1^\ell P_\ell(-z_s)$ at $\ell = n$. Hence the series in (A5.6a) can be replaced by the integral (A5.6b) (modulo the postulate that $A(\ell, s)$ is indeed analytic everywhere in the complex ℓ plane). To exhibit the contribution of the Regge poles to the scattering amplitude, we deform the contour to form C' and enclose it at $\mathfrak{Re}(\ell) = -1/2$: Applying the residue theorem to this

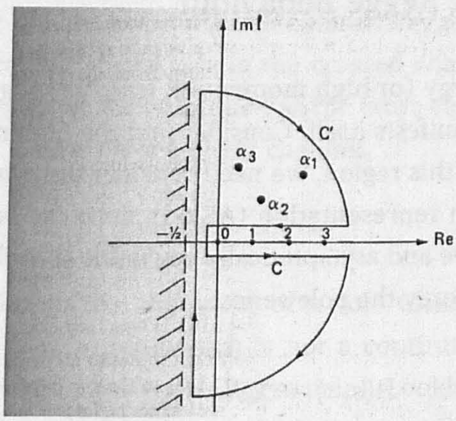

Figure 11.5: Illustration of the contour C'. (From Joachain [1975], p267.)

new contour we find

$$
\frac{1}{2\pi i}\oint_{c'} g(\ell)d\ell = -\sum_i \mathrm{Res}(g(\ell = \alpha_i))
$$

We may decompose the integral on the LHS as

$$
\frac{1}{2\pi i}\oint_{C'} g(\ell)d\ell = \int_C g(\ell)d\ell + \oint_{semi-circle} g(\ell)d\ell + \int_{-i\infty-1/2+\epsilon}^{i\infty-1/2+\epsilon} g(\ell)d\ell
$$

The first term is simply the scattering amplitude given above in (A5.6b). The second term may be shown to vanish for Yukawa-like potentials.[49] The third term is denoted the 'background integral' (BI). Then we can write

$$A(s,z) = \sum_i \frac{-\beta_i(s)\pi[2\alpha_i(s)+1]P_{\alpha_i(s)}(-z)}{\sin \pi\alpha_i(s)} + BI, \tag{A5.6d}$$

where $\beta_i(s)$ is the residue of the ith Regge pole of $A(\ell, s)$. This is known as the *Sommerfeld-Watson* representation of the amplitude, and it allows us to analyze explicitly the contribution to the total scattering amplitude from each Regge pole. Given the dependence of the denominator on α, it is clear that there will be poles in the scattering amplitude whenever the Regge trajectory passes through an integer.

A5.7 Asymptotic Regge Behaviour

It is in the high energy (or high momentum transfer) region that distinctive Regge behaviour manifests itself. Consider first the high momentum transfer (high z_S) region. In this region, we need consider only the pole series in the Sommerfeld-Watson representation (A5.6d) since the background integral may be shown to give and asymptotically negligible contribution.[50] Therefore, in the large z limit, only the pole series

$$A(s,z) \to \Sigma_i \beta_i(s) \frac{z_s^{\alpha_i(s)}}{\sin \pi\alpha_i(s)}$$

survives, where we have lumped all the other s-dependent and constant factors into β_i (the residue of the ith Regge pole). The dominant term in this series is the right-most pole, i.e. the one for which $\mathfrak{Re}(\ell)$ is the largest. So in this limit as $t \to \infty$, we can put

$$A(s,z) \to -\beta(s) \frac{z_s^{\alpha(s)}}{\sin \pi\alpha(s)}$$

where now this α denotes specifically the right-most pole (and β its residue). (Note that nothing can be said about the residue function $\beta(s)$.) This formula

[49] See Joachain [1975], p266.
[50] Chew [1966], p58; Barone and Predazzi [2002], p91.

may be shown to be valid up to sub-asymptotic corrections. Note that had we started not from the s-channel but from the t-channel, we would have obtained, as $s \to \infty$,

$$A(s,z) \to -\beta_i(t)\frac{z_s^{\alpha_i(t)}}{\sin \pi\alpha_i(t)}.$$

This function is not, however, as yet crossing symmetric. It can be shown that to accommodate convergence of the partial wave amplitude under crossing, we have to separate out even and odd values of ℓ and thus create amplitudes of positive and negative signature. The procedure turns out to be straightforward and the modification slight, and we obtain, as $t \to \infty$ for fixed s,

$$A(s,z) \to -\beta(t)\frac{(1+\xi e^{-\pi\alpha(s)})t^{\alpha}(s)}{\sin \pi\alpha(s)}.$$

We thus arrive at the simple, but fundamental, Regge theory prediction. It is that it is the leading singularity in the crossed channel that governs the asymptotic behaviour of the amplitude as the cross channel energy tends to infinity:[51] that is, where s is the direct channel,

$$A(s, t \to \infty) \propto t^{\alpha_s}. \tag{A5.7e}$$

Let us compare this to the behaviour of a pole which contributes to only one partial wave (i.e. a pole which is not a continuous function passing through various ℓ's and hence not a Regge pole). Looking at the Mandelstam representation (A3a) and referring to (A1.5c), we see that the contribution to $A(s,t)$ from a particle of spin ℓ_0 and mass m in the s (direct) channel is

$$g^2\frac{P_{\ell_0}(\cos\theta)}{m^2 - s}.$$

From the orthogonality of the Legendre polynomials, (A5.5a), together with (A5.1a) we know this will produce a delta function singularity in the partial wave amplitude,

$$(A(\ell, s) = g^2\frac{\delta_{\ell\ell_0}}{m^2 - s}$$

[51] Barone and Predazzi [2002], p92.

and hence is forbidden by maximal analyticity. But since by (A5.7a) the asymptotic behavior of this contribution as $t \to \infty$ is

$$A(s, t \to \infty) \propto t^{\ell_0}, \qquad \text{(A5.7f)}$$

we now have a difference in the asymptotic behaviour relative to (A5.7e) which can be used to test the validity of the postulate. Since

$$\alpha(s) \approx \alpha(m^2) + \alpha'(m^2)(s - m^2) = \ell + \alpha'(m^2)(s - m^2),$$

the two amplitudes (A5.7e) and (A5.7f) differ by a factor of $t^{\alpha'(m^2)(s-m^2)}$, and this is a difference which can 'readily be detected'.[52] Thus the postulate of maximal analyticity of the second kind, it turns out, has very precise empirical consequences.

To complete this tour of the mathematics of the S-matrix, what I want to do now is extract as much interpretation as we can about the nature of the poles which are generated under the assumption that the postulate of maximal analyticity is respected by nature. I will do so by returning to the function from which they came.

A5.8 The Physical Interpretation of Regge Poles

We now wish to go back to the physical partial wave amplitudes and uncover what we can about the contribution of the Regge poles. We can extract the partial wave amplitudes from (A5.6a), the 'continued' version of (A5.1),

$$A(s, z_s) = \sum_{\ell=0}^{\infty}(2\ell + 1)A(\ell, s)P_\ell(z) \qquad \text{(A5.6a)}$$

by slotting in the Sommerfeld-Watson transform (A5.6d) (minus the background integral, which disappears asymptotically; see Section A5.7). Quoting the property of Legendre polynomials (which holds when ℓ is a non-negative integer, m) that

$$\int_{-1}^{1} dz P_m(z)p_\alpha(-z) = \frac{2}{\pi}\frac{\sin \pi\alpha}{(\alpha - m)(m + \alpha + 1)},$$

[52]Squires [1971], p74.

we get as the contribution from the nth Regge pole to the mth partial wave[53]

$$A_n(\ell = m, E) = \frac{\beta_n(e)}{\pi(m + \alpha_n(E) + 1)} \frac{1}{(\alpha_n(E) - m)}$$

where we have replaced 's' – the square of the CM energy – with 'E' for energy to keep salient what is going on. This contribution to the $\ell = m$ partial wave will dominate the scattering amplitude if $\alpha_n(E)$ is close to the integer m, so that

$$A_n(\ell = m, E) \to \frac{\beta_n(E)}{\alpha_n(E) - m},$$

as $\ell \to m \cong \alpha_n(E)$. Let us now expand $\alpha_n(E)$ about the energy E_m at which $\mathfrak{Re}(\alpha_n) = m$. In general, we know that the $\alpha_n(E)$ are complex, so we write

$$\alpha_n(E_m) = \mathfrak{Re}\big(\alpha_n(E_m)\big) + i\mathfrak{Im}\big(\alpha_n(E_m)\big).$$

If we assume that the imaginary part is small, then we can expand $\alpha_n(E)$ about E_m as follows:

$$\alpha_n(E_m) = \alpha_n(E_m) + (E - E_m)\alpha_n'(E_m) + \ldots \equiv (E - E_m)\frac{d\Big(\mathfrak{Re}\big(\alpha_n(E_m)\big)\Big)}{dE}\Big|_{E=E_m} + i\mathfrak{Im}\big(\alpha_n(E_n$$

Now writing

$$q \equiv \frac{d\Big(\mathfrak{Re}\big(\alpha_n(E_m)\big)\Big)}{dE}\Big|_{E=E_m}$$

and

$$\Gamma = \frac{\mathfrak{Im}\big(\alpha_n(E_m)\big)}{q}$$

we find that

$$\begin{aligned} A^n(\ell = m, E) &\to \frac{\beta_n(E)}{m + (E - E_m)\Big(d\big(\mathfrak{Re}\big(\alpha_n(E_m)\big)\big)/dE\Big)\Big|_{E=E_m} + i\mathfrak{Im}\big(\alpha_n(E_m)\big) - m} \\ &= \frac{\beta_n(E)/q}{(E - E_m) + i\Gamma.} \end{aligned} \tag{A5.8a}$$

But this is just the Breit-Wigner formula, familiar from nuclear physics, for a resonance with mass E_m and width Γ. Specializing to the case where the imaginary part and hence the width is zero, this gives us the formula for a

[53]Here as throughout, the properties of the Legendre polynomials are given in Collins [1977], Appendix A.

bound state. Therefore the Regge poles, when evaluated at physical (positive integral) values of ℓ, *correspond to bound states and resonances*. In other words, they represent *composite particles*. This is the result that forges the connection between the extended analyticity postulate and the possibility of 'nuclear democracy'.

One way to visualize what is going on here is to view the poles as interpolated by a function $\alpha(s)$. Expanding $\alpha(s)$ as a power series about $s = 0$, we get $\alpha(s) = \alpha(0) + \alpha'(s)$, where $\alpha(0)$ is the intercept and α' the slope. Then, as we traverse up $\alpha(0)$ we can plot particles with all the same quantum numbers but their spin and mass at the value of s to which their mass corresponds.[54] This gives the *Chew-Frautschi plot*. The experimentally discovered linear

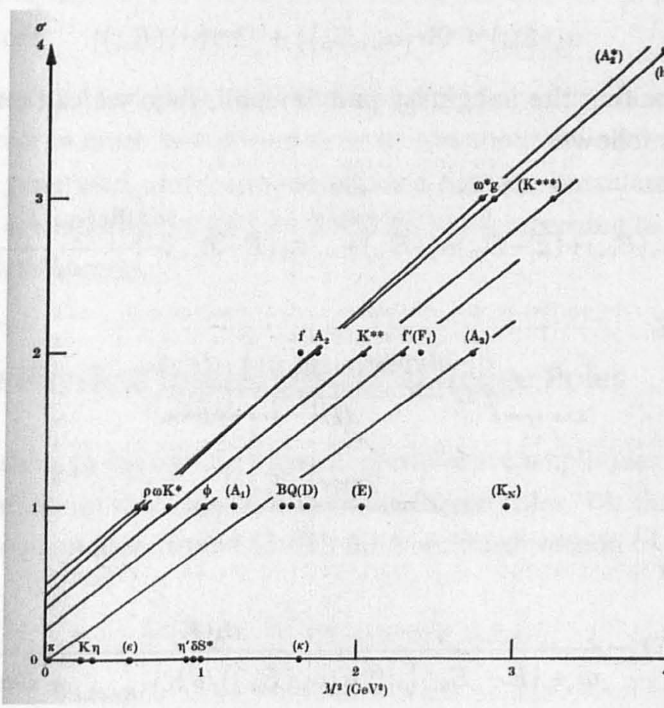

Figure 11.6: Chew-Frautschi-plot of $Re(\alpha)$ versus energy for mesons. (From Collins [1977], p145.)

behaviour is only expected *a priori* over a small region of the trajectory. It

[54] Actually, the spins on a given trajectory will differ by units of 2. This is because the partial wave amplitudes, to be analytically continued and crossing-compatible, must be separated into parts of even and odd signature (even and odd ℓ.) This is a complication I have overlooked, but is straightforward to implement.

is the Veneziano 'duality' model which finally explained the linearity, which extends over several GeV (and baffled theorists for years).[55] It is interesting to note that the various trajectories are approximately parallel so that the slope α' is approximately universal. Over time, it becomes identified with the *string tension* – the only free parameter in string theory.

This completes my tour of the basic mathematics of the S-matrix.

[55]See Collins [1977], p224.

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