Non-Empirical Modelling and Theorizing: Scientific Progress in Particle Physics

Cristin Cain Chall

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NON-EMPIRICAL MODELLING AND THEORIZING: SCIENTIFIC PROGRESS IN PARTICLE PHYSICS

by

Cristin Cain Chall

Bachelor of Science
Howard University 2009

Master of Arts
Virginia Polytechnic and State University 2011

Submitted in Partial Fulfillment of the Requirements
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College of Arts and Sciences
University of South Carolina

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Accepted by:

Michael Dickson, Major Professor
Michael Stöltzner, Committee Member
Richard Dawid, Committee Member
Richard Healey, Committee Member
Cheryl L. Addy, Vice Provost and Dean of the Graduate School
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Abstract

Particle physics (and other fundamental physics research, including searches for a theory of quantum gravity) faces a problem when it comes to acquiring experimental evidence. Many theories and models make predictions that cannot be tested with current, or even prospective technology. Yet these fields continue to develop, with new models and theories regularly being introduced, scrutinised, changed, and discarded. My project aims at examining the way theories and models are constructed, adapted, and assessed in fields that lack the empirical evidence that usually grounds such tasks. I will focus on two prominent examples: string theory and attempts to explain electroweak symmetry breaking beyond the standard model explanation provided by the Brout-Englert-Higgs mechanism. After a brief introduction to the physical and philosophical issues relevant to my arguments, I move on to the core chapters. First, I begin the task of constructing a new framework for understanding the dynamics of scientific change in particle physics by introducing a new concept, the model-group, in order to expand the methodology of scientific research programmes introduced by Imre Lakatos. I use two case studies from particle physics to motivate the use of Lakatosian research programmes. In the following chapter, unsatisfied by Lakatos’s account of scientific assessment, I modify his framework further by integrating it with Larry Laudan’s problem-solving conception of rational scientific growth. In the final chapter, I criticise one recent and noteworthy attempt to understand confirmation in the absence of experimental evidence, namely Richard Dawid’s non-empirical theory assessment scheme. I then conclude, discussing some possible future advancements and applications for my new hybrid framework of scientific progress.
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**List of Abbreviations**

BSM .................................................. Beyond the Standard Model
CERN ............................................ European Organization for Nuclear Research
CH .................................................... Composite Higgs
CMSSM .............................. Constrained Minimal Supersymmetric Standard Model
DM ..................................................... Dark Matter
EWSB ........................................... Electroweak Symmetry Breaking
GeV ................................................ Gigaelectronvolt
GR .................................................. General Relativity
IBE ............................................. Inference to the Best Explanation
LH ................................................... Little Higgs
LHC ................................................ Large Hadron Collider
MeV .............................................. Megaelectronvolt
MIA .............................................. Meta-Inductive Argument
MSRP .......................................... Methodology of Scientific Research Programmes
MSSM ........................................ Minimal Supersymmetric Standard Model
NAA ............................................... No Alternatives Argument
NMSSM ........................... Next-to-Minimal Supersymmetric Standard Model
OPE ............................................... Operator Product Expansions
pMSSM ............................. Phenomenological Minimal Supersymmetric Standard Model
pNGP ............................................ (Pseudo)-Nambu-Goldstone Boson
QFT ............................................... Quantum Field Theory
QG ................................................ Quantum Gravity
SM .......................................................... Standard Model
SMEFT ........................................ Standard Model Effective Field Theory
ST .......................................................... String Theory
SUSY .......................................................... Supersymmetry
TC .......................................................... Technicolor
TeV .......................................................... Teraelectronvolt
UEA ................................................... Unexpected Explanatory Coherence Argument
Chapter 1

Introduction

“The oldest and strongest emotion of mankind is fear, and the oldest and strongest kind of fear is fear of the unknown.”

– Howard Philips Lovecraft, “Supernatural Horror in Literature”

1.1 The Edge of Fundamental Physics

Progress is frequently seen as a defining feature of the sciences: scientists make progress in our understanding of natural phenomena, they make progress in experimental design, and technological progress emerges from both theoretical and experimental advances. There is a long history of philosophical discussion concerning the kinds of progress which science makes. Recent literature has explored how scientific progress can be interpreted through relations between the truth and scientific theories, their ability to solve problems, or their ability to increase knowledge or understanding of natural phenomena.¹ Although these debates concern scientific advancement generally, they become particularly pressing in the context of research into fundamental physics. In such research, physicists explore the ontology of space and time (or what these dimensions emerge out of), the unity of the known forces, and the most basic building blocks of matter. However, there is an increasing strain upon physicists’ practical ability to probe ever smaller length scales at their correspondingly higher energy ranges. The technologies and resources required to reach these

¹For a recent survey of these different conceptions of progress, see (Dellsén, 2018).
higher energies simply don’t exist, nor will they be on the horizon for many decades to come, if ever. In such circumstances, where theories of fundamental physics frequently outstrip our recourse to experimental verification, how are we to conceive of the progress of science?

Physicists are now able to test fundamental physical theories up to previously unreachable energies. The Large Hadron Collider (LHC), the world’s largest particle accelerator, has the ability to accelerate protons in a beam reaching 6.5 teraelectronvolts (TeV), and collide them with another similarly energetic beam, reaching collision energies of 13 TeV. From these collisions, showers of particles erupt, which can be measured directly in one of the seven detectors equipped at the LHC, or else are measured indirectly by modelling various processes that determine probable decay products, revealing missing energies, mass, and momenta. With this monumental machine, the massive experimental collaborations of ATLAS and CMS (each composed of more than 3000 physicists) have all but confirmed the one remaining missing element of the standard model of particle physics (SM), the Higgs boson. The Higgs boson is the particle associated with the Brout-Englert-Higgs mechanism, the spontaneous breaking of the SU(2) gauge symmetry, which is the SM’s account of electroweak symmetry breaking (EWSB). With this discovery, first predicted in the 1960s, the SM is both complete and empirically adequate. The SM is the best

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2ATLAS and CMS are the largest and most general detectors. ALICE and LHCb are designed to study specific phenomena: ALICE studies quark-gluon plasma and LHCb focuses on the matter/anti-matter asymmetry by studying b quarks. TOTEM and LHCf are positioned directly in the forward direction of the beam, TOTEM on either side of CMS, and LHCf on either side of the ATLAS detector. MoDEL operates near LHCb and is designed to detect the hypothesised magnetic monopole (see CERN, 2019).

3First proposed by Englert and Brout (1964); Higgs (1964a,b); Guralnik, Hagen, and Kibble (1964) before the SM was finalised, this mechanism became a central piece for establishing complete symmetries and renormalizability in the SM. This process is often called the “Higgs mechanism” because Peter Higgs was the first to suggest a physical boson should result from it. In what follows, I will refer to it as the Higgs mechanism for brevity’s sake, and in doing so I do not mean to imply that the other theorists who first described the mechanism do not deserve a share of the credit.

4For a philosophical take on the formulation of the Higgs mechanism, see (Karaca, 2013).
tested and most widely accepted theory of particle physics: all of its predictions have received empirical confirmations and so this physical account of particles and three of the fundamental forces (electromagnetism and the strong and weak nuclear forces) has achieved outstanding agreement with experimental findings.

However, the SM is not without criticisms and challenges, both from the theoretical and experimental directions. On the theoretical side, there are a number of fundamental questions for which the SM provides no answer. For example, there is no internal explanation for why there are three different particle generations in the SM, there is no SM explanation for certain quantities like particle masses and coupling constants to take the specific values that they do, there are 26 free parameters that must be entered in by hand, and there is no account of gravitational effects in the SM.

From the experimental side, there are several findings that the SM cannot properly explain. For example, though there is a theoretical symmetry between matter and anti-matter in the SM, we find an obvious imbalance between them, resulting in enough remaining matter to form stars, planets, and the rest of the observable matter in the universe. There are also cosmological observations that suggest that there is a massive amount of “missing” matter and energy we cannot observe directly, but this dark matter and dark energy play no role in the SM. Finally, the SM predicted

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5 The only exception being the masses of the neutrinos, which could still be largely accommodated by the SM. See below.

6 The first generation of particles includes the components of everyday matter: the electron, the electron neutrino, and the particles that make up protons and neutrons, the up and down quarks. In addition to these, there are two more charged leptons, the muon and tau, their associated neutrinos, and two more pairs of quarks, the charm and strange and the top and bottom. These additional generations fit into a mass hierarchy, such that each generation shares features, like spin and electric charge, with their counterparts, but differ greatly in mass. The electron, for example, has a mass of 0.511 mega-electronvolts (MeV), while the muon’s mass is 106 MeV and the tau’s mass is 1777 MeV.

7 This is the typical number of free parameters given for the SM with three generations and massive neutrinos (see Krämer, 2017). Without massive neutrinos, the number of free parameters falls to 19. The equations of the SM work well, once the correct values have been entered, but the values themselves must be found experimentally and cannot be derived from the theory itself.
that neutrinos would be massless. However, it was discovered, using cosmological observations and other techniques, that neutrinos have oscillations, which indicates that they do have mass, albeit very little. The discovery of neutrino masses has been seen as the first evidence for physics beyond the SM (BSM) (see, e.g., Bilenky, 2016), leading to a necessary correction of the SM to accommodate massive neutrinos.

Other challenges are related directly to the Higgs mechanism and its related particle. Although one of the features of the Higgs mechanism is that it directly gives mass to the weak force bosons and indirectly gives masses to other particles through their interactions with the Higgs field, it doesn’t provide an explanation for their exact values. The introduction of a Higgs field within the SM was viewed by some as either an ad hoc hypothesis (Friederich, Harlander, and Karaca, 2014), or an immoderate speculation (Wells, 2016), since, among other things, it was the introduction of a fundamental scalar field with an infinite range and a unique vacuum state, together with an elementary scalar boson (unique among all known matter) as its propagating particle. Furthermore, if there happened to be any new physics at higher energy ranges that coupled with the Higgs, there would be a high degree of fine-tuning in order to account for the electroweak scale being so much lower than that of the Planck scale, an issue known as the hierarchy or naturalness problem.

Finally, there are general issues of how far the SM can be applied. There is a huge energy range between the highest energies achievable by the LHC and the Planck scale, which remains unexplored experimentally and about which the SM is

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8The electroweak scale is the energy scale of about 246 GeV, which is the scale of processes described by the electroweak components of the SM. The Planck scale (in terms of energy) is the scale beyond which the predictions of physical theories like the SM and general relativity break down. The Planck energy is about $1.22 \times 10^{19}$ GeV, or 17 orders of magnitude greater than the electroweak scale.

9Mättig and Stöltzner (2019) argue that physicists treat the problem of fine-tuning, the hierarchy problem, and the naturalness problem as roughly equivalent, even though different technical understandings of each can be formulated. Their argument is reinforced by, for example, Wells (2017) treating the naturalness and fine-tuning problems analogously and Grünbaum (2012) treating fine-tuning arguments as conceptually dependent on considerations of naturalness.
silent. And since the SM does not include an explanation of gravitational effects, it is irreconcilable with general relativity (GR), our other primary theory of fundamental reality, which is particularly problematic in the limited domains in which the two theories should interact. These challenges have motivated physicists to find BSM physics in an attempt to provide explanations, unifications, or predictions that will resolve them.

Prospective BSM physics includes a number of extensions of the SM, including supersymmetry (SUSY), models with additional dimensions, and various modifications of the Higgs mechanism (including composite Higgs (CH) models and non-SUSY models that propose extensions of the Higgs mechanism). For more on this kind of model, see Chapter 2. Another category of BSM physics is composed of the various attempts at unifying GR (our best theory of gravitational physics) and quantum mechanics (our general theory of particulate matter at small distances, including quantum field theories (QFTs) like the SM). These quantum gravity (QG) theories hope to overcome the difficulties inherent in trying to reconcile the two strands of fundamental physics that emerged in the 20th century, but they typically describe physics at much higher energy ranges than can be currently reached at the LHC (since these are the energy ranges in which gravitational effects become strong enough to influence quantum phenomena). One of the most prominent examples of this kind of BSM physics is string theory (ST), which unifies all particles and forces under an ontology of one-dimensional strings or, more generally, $n$-dimensional objects called branes. For more on ST, see Chapters 3 and 4.

One of the central features all of these attempts at new physics currently share is that they each lack any experimental evidence bearing on their predictions of new phenomena. Moreover, the LHC has probed large swaths of the parameter spaces for many of the BSM models mentioned above, and has thus far come up negative. These circumstances, in which physicists use the LHC to discover the Higgs
boson, but find no evidence of BSM physics, has been described as a “nightmare scenario” (Cho, 2007). Until the LHC is upgraded to reach higher energies or greater precision,\textsuperscript{10} or new, higher energy detectors come online, the discovery of new data corroborating any current models of BSM physics is seen as increasingly unlikely from particle accelerator experiments, since much of the new areas to explore have already been searched for more obvious SM deviations. As a further complication, there are no strong theoretical preferences for specific energy ranges to examine, as there were with the LHC. In the case of QG theories, unique predictions are mostly expected near the Planck energy, which is far beyond what any feasible technology is capable of reaching. And so, physicists are left in a sort of crisis: new physics is anticipated, new models and theories are developed, but none have panned out within current experiments and new experiments are years or decades distant, and the predictions of higher-energy phenomena (like compact extra dimensions or strings) are well beyond presently conceivable technology. With these circumstances, how are we to understand and assess scientific progress? How are we to properly assess the rationality pursuit of theories and models when physicists’ expectations for new experimental insights are dim?

In the following chapters, I will argue that the best way of conceptualising non-empirical theory and model assessment can be found by hybridising the Methodology of Scientific Research Programmes (MSRP), introduced by Imré Lakatos, with Larry Laudan’s problem-solving account of scientific rationality, borrowed from his methodology of research traditions. In order to formulate this hybrid account, I first propose to modify Lakatos’s MSRP in order to accommodate collections of BSM models that share several important, narrative features in common, as research programmes in

\textsuperscript{10}The LHC is currently undergoing a high luminosity upgrade that is meant to increase its collision energy to 14 TeV and improve its precision by a factor of 10. Since luminosity is the ratio of the number of events detected in a certain time interval to the interaction cross-section, the higher the luminosity, the more data is available from the detectors. However, this upgrade will not be completed until 2026.
their own right. Since Lakatos’s account relies on a problematic notion of scientific rationality, the next move will be to replace it with something like the problem-solving notion employed by Laudan. This hybrid framework, I will argue, provides the best way of conceptualising the progress of non-empirical science, as it can account for the shifts at the very forefront of fundamental physics research (especially with regards to the developmental models employed in BSM searches at the LHC) by focussing on the ability of a research programme to solve conceptual problems and a certain understanding of empirical problems that doesn’t require immediate empirical adequacy.

My line of argumentation has several significant connections to more general discussions in the philosophy of science, as well as to general philosophical issues concerning the use of reason and evidence in making judgements about the world. The nature and import of experimental evidence is of obvious concern in the philosophy of science. Testing theoretical predictions using experiment is taken to be a hallmark of the scientific method. My hybrid framework shows that, even in sciences like physics, where evidence gained from controlled experimentation is of vital importance, we can argue that progress can be made even in its absence. My hybrid framework also impacts upon issues of scientific change, since it provides both a relatively “objective” method for evaluating scientific progress (even in the absence of experiment) and connects closely with the literature on “non-epistemic” values. This latter connection arises from the considerations of scientific communities and their internal practices necessary to properly evaluate a programme’s problems and their solutions.

More generally, my hybrid account mimics an extreme case of a very common type of everyday judgement. We are frequently left without complete information regarding some impending decision. Acquiring more information is frequently impossible in many everyday situations, particularly in cases where decisions are needed quickly, or where there are costs (epistemic or otherwise) to pursuing more information. My
hybrid framework replicates the philosophical basis of the decision-making strategy used to navigate these common, if thorny, situations. Although there are obvious dis-analogies between the epistemology of theory-assessment in the unique circumstances I focus on, and the epistemology of decision-making under uncertainty,\textsuperscript{11} it is easy to see how the two mirror each other in interesting ways. In both cases, agents need to assess the options available to them, take into consideration their various goals, and weigh the options based on how likely they are to fulfil those goals. The agents can always update their judgements concerning their efficiency in achieving those goals as more data becomes available to them.

1.2 State of the Art

In this section, I briefly cover the state of philosophical discussions relevant to the arguments presented in this dissertation. I focus on issues of conceptualising scientific progress, how discussions of models in science have developed, and the rise of non-empirical considerations of science. Finally, I briefly take a look at some of the philosophical issues particular to particle physics.

1.2.1 Scientific Progress

First and foremost, my hybrid formulation using Lakatos’s MSRP and Laudan’s problem-solving rationality is an account of scientific progress, which connects it to a rich vein of discussion extending throughout philosophical treatments of science. To better understand how my hybrid account fits into these discussions, I will begin with a brief look at notions of scientific progress in the literature. First, let me provide an understanding of what is meant by “progress” in this context. There are many ways in which science and its various subfields can be said to make progress. Some notions

\textsuperscript{11}For instance, scientific decision-making occurs within an extended network of individuals and institutions, with formal checks on the plausibility of speculations and the veracity of reported results, which ordinary decision-making lacks.
of progress are not typically considered relevant to the advancement of science itself, but rather act as additional goods associated with scientific progress. These latter notions include progress made in technology or economic developments (including those brought about by patenting new technologies and knowledge, but also by the number of scientists employed), which are considered ancillary to the actual progress of science itself.

The present work, however, will focus on the accumulation of cognitive improvements of our scientific theorizing and practice, and here I make no firm commitments about whether these cognitive improvements are strongly linked to some sort of truth about the world, or whether it is an instrumental notion. A naive realist assumes that science aims at achieving ever more accurate and truthful descriptions of the world. However, naive realism is not a common view among either philosophers or physicists (Niiniluoto, 2015). Rather, views of scientific realism, the position that scientific theories and explanations generally aim at truth, are typically more nuanced. For example, Leplin (1984, 1997) has argued for a notion of approximate truth, and argued that features like the ability to make novel predictions exemplify the ability of science to obtain it, arguments taken up in many realist arguments. Crafting theories with better and better approximations of truth (greater “truthlikeness”) would indicate progress. Likewise, both Psillos (1999) and Niiniluoto (1999) argue that our best theories have a sort of privileged epistemic status that grants knowledge of both the observable and unobservable features of the material world. Psillos claims that only sufficiently mature theories can obtain this privileged status, though it is also important that the theories aren’t created specifically to account for known empirical findings: that is to say, they must be mature and non-\textit{ad hoc}.

However, it has been argued that a direct link between theory and some notion of truth is not typically the goal of scientific inquiry, especially since establishing that

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12For classic instrumentalist takes on scientific progress, see (Popper, 1964; van Fraassen, 1980).
we have such a link with certainty would likely be impossible. Instead, the basic aim is that theories and models should exhibit some form of empirical adequacy, where empirical adequacy is defined as an agreement between theoretical claims and the observable outcomes of experiment and observation. The instrumentalist reading of progress has a long history in the philosophy of science, going back at least to Duhem (1906), who argued that theories should be treated as tools for organising observational statements and making predictions. Duhem compared scientific progress to a rising tide: individual waves will rise and fall, but the overall effect is an increase in the water level. However, Duhem believed that there was a “natural classification” of scientific laws, so that scientific progress was measured by the approach of successive theories towards this classification. Other instrumental readings of scientific progress are possible, which do not have this sliver of realism. For instance, van Fraassen (1980) argues that, though we can accept our best scientific theories (as empirically adequate), and therefore will have certain commitments towards them, we should deny that they are in fact true, or even that their truth is a scientific value towards which we should aim. For van Fraassen, a theory with a high degree of empirical adequacy is the best we can possibly hope for, and is the most we can have justified belief in. Theories engage in a form of natural selection, competing with one another so that only the fittest (most empirically adequate) survive.\textsuperscript{13}

Regardless of any considerations of the instrumentality (or lack thereof) of the notion of scientific progress through cognitive advancements, my hybrid framework is largely neutral concerning the realism question. Neither Lakatos, nor Laudan created methodologies that need to be read as necessarily realist or instrumentalist,\textsuperscript{14} and so

\textsuperscript{13}Other considerations naturally come into play in this kind of natural selection model of progress, especially in cases in which two theories have approximately equal degrees of empirical adequacy. These considerations include various theoretical virtues (simplicity, coherence with other theories, experimental promise, and especially the ability to make novel predictions).

\textsuperscript{14}It is true that Laudan believed that science shouldn’t aim at finding out the ‘truth’ of the matters it investigates, since this is an aim for which we have reasons to believe we will always
my hybrid framework can be used by those adopting either framework. The problem-solving account I introduce in Chapter 3 is concerned with cognitive advancements through better ability to find solutions to empirical and conceptual problems, rather than truth, but it can still be utilized by those with a penchant for approximate truth.\footnote{15}

However, my efforts to try to remain neutral on issues of scientific realism put my account at odds with the dominant accounts of scientific progress through cognitive advancement. These alternatives to the problem-solving account of progress include the truthlikeness account\footnote{16} (where progress occurs as theories become more truthlike), the epistemic account\footnote{17} (where progress is tied to increases in scientific knowledge, defined roughly as justified true belief), and the noetic account (where progress occurs as we improve scientific understanding, defined as the ability to provide explanations and predictions) (this categorization is guided by Dellsén, 2016, 2018). Each of these other accounts of progress in science relies, to a greater or lesser extent, on access to some kind of confirmation, on demonstrating and improving veridicality. The fail to achieve (Laudan, 1984). Furthermore, he argued against some of the key tenets of scientific realism and introduced the pessimistic meta-induction argument (Laudan, 1981a). However, he ultimately concludes that “nothing [in his arguments] refutes the possibility in principle of a realistic epistemology of science,” but that “it can only be wish fulfilment that gives rise to the claim that realism, and realism alone, explains why science works” (1981a, 48). He doesn’t think a realist interpretation of science is warranted, but he doesn’t discount that it is possible to maintain, at least in principle.

\footnote{15}Indeed, Niiniluoto (1984) has claimed that realists can accept science as a problem-solving activity by seeing the attempts at problem-solving as searches for true solutions. More recently, he has argued that focusing on the fact that scientific theorising aims at knowledge rather than truth can help move the debate about scientific progress away from this classic dichotomy (2014).

\footnote{16}This account is also referred to as the verisimilitudinarian account. Popper (1972) proposed an intuitive definition of the verisimilitude of scientific theories, though this definition was criticised by Miller (1974) and Tichý (1974) for being unable to compare the truthlikeness of theories which are, strictly speaking, false. A new approach, based on the assumption that the descriptions of more truthlike theories will be more similar to the description of the state of the actual world, was developed by Niiniluoto (1977, 1987) and Oddie (1986).

\footnote{17}The epistemic account of scientific progress gained new life with its reintroduction by Bird (2007), though it was first introduced by Cohen (1980) and Barnes (1991) in response to the problems of the truthlikeness account.
truthlikeness account obviously requires some way of checking the nearness to truth of scientific claims in order to assess progress. For similar reasons, the epistemic account needs experimental results both to measure truthlikeness and to act as justification for beliefs. Finally, the noetic account also relies on experiments to judge improvements in both explanations and predictions.\textsuperscript{18} My account is distinct from these other views on scientific progress, as are problem-solving accounts generally, since significant problems can be solved even without new empirical input. Problem-solving accounts are not necessarily even “moderately” realist, though again, they are not open only to instrumentalists. Thus, my account of progress will be the only one that is effective in describing the examples of non-empirical science mentioned above, where veridicality considerations will not assist in assessing scientific progress.\textsuperscript{19}

Other notions of scientific progress are concerned with advances in features that are not cognitive in the same sense. These other features include explanatory power, simplicity, and unification. Hempel (1965), for instance, combined the aim of increasing explanatory power with the requirement for theories to make and confirm new predictions, creating a notion of systematic power. Systematic power would then form one of the elements for appraising different theories (others include “the clarity and precision” of a theory’s formulation and logical relationships, its simplicity, and “the extent to which theories have been confirmed by the evidence” (117)).

Simplicity also becomes a mark of scientific progress, with the aim of finding simpler theories that account for the same empirical data. Foster and Martin (1966) argued that seeking the simplest possible theory is a consequence of underdetermi-

\textsuperscript{18}Dellsén (2016) refers to his noetic account as “\textit{quasi-factive—roughly} in the sense that the explanatorily/predictively essential elements of a theory must be true in order for the theory to provide grounds for understanding,” and therefore he offers “a moderately realist view of the aim of science” (73).

\textsuperscript{19}However, I reserve judgement concerning its superiority in the more typical cases of science, where empirical evidence is more readily accessible. As I will describe later, both Lakatos and Laudan’s larger projects have been subjected to serious criticisms, and so should not be applied to science in general without acknowledging and adapting to these criticism, a project beyond the scope of the present work.
nation by the evidence: with the “correct” theory underdetermined, we should prefer the conceptually simplest one of those available. Mach argued that “the simplest, most parsimonious theories economised memory and effort by using abstract concepts and laws instead of attending to the details of each individual event or experiment” (Banks, 2004, 23), which made simplicity essential for further developments in the natural sciences and technology.

Kitcher (1993) introduced a notion of consensus practices, which is a conceptualisation of scientific progress that recognises that science is done by individuals with various interests who all compete and cooperate with one another. Though he defends a more pluralistic approach, in which our notion of scientific progress requires a variety of conceptual classifications, Kitcher also argues that a good theory is one that offers a unification of empirical phenomena and various laws from different domains. The notions of progress through greater explanatory power, simplicity, or unification are only tangentially related to how I will be framing scientific progress in what follows, but they have certainly been influential in discussions about progress, and thus worth mentioning in passing.

There are many other important discussions of scientific progress that extend beyond what is possible in this brief survey. However, the primary focus of the account I describe below is centred around a particular line of philosophical argumentation. Some of the most influential thinkers on the issue of scientific progress and change are the methodologists of science, including Kuhn (1962, 1977), Lakatos (1978b,c,d), Laudan (1976, 1977, 1987), and Feyerabend (1975). The works of Lakatos and Laudan are particularly important for the hybrid framework that is developed in Chapters 2 and 3.

Prior to the developments of the methodologists of science in the 1960s and 70s, this pluralistic approach to scientific progress is also favoured by Cartwright’s (1999) “dappled world” and Longino’s (2002) emphasis on the importance of the different perspectives and values held by the members of various scientific communities.
the logical positivist tradition largely viewed science as a cumulative endeavour, with new theories building upon earlier works by adding formal structure and scope, in a largely linear account of progress. The methodologists ended up providing counterpoints to these views of the logical positivists, though their criticisms also turned towards each other’s arguments. Prominently displayed in Kuhn’s *Structure of Scientific Revolutions* in 1962, the methodologists moved away from a cumulative view of scientific progress by introducing and focusing on structures larger than theories, and by adding complex dynamics for the way these new structures behave over time. Kuhn, a former physicist turned historian and philosopher of science, aimed to describe “the quite different concept of science that can emerge from the historical records of the research activity itself” (3). He introduced the notion of a “paradigm,” a scientific framework with its own particular set of research foci and methods (what science in a particular domain should be about, and how that ontology should be explored). Each paradigm was accompanied by its own research culture: goals, standards, institutional norms, and even language are paradigm-specific. Within a paradigm, “normal science” occurs, which Kuhn equates with “puzzle-solving.” The details of the theories within the paradigm are explored, and anomalies are largely suppressed. It is in this phase that science progresses much as the positivist description says it does.

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21 There were precursors in analysing the philosophical relevance of scientific research and discovery from which Kuhn’s research arose. One particularly notable example is Hanson (1958). Ironically, Kuhn’s work originally appeared in the Vienna Circle publication, the *International Encyclopedia of Unified Science*.

22 It should be noted, however, that Kuhn used the term “paradigm” inconsistently throughout his work, famously addressing the criticism that he had used the term in at least two different senses in the revised addition of *Structure* (with Masterman (1970) listing 22 different senses). My description here is in line with Kuhn’s first sense, that of “the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community” (Kuhn, 1962, 175). The second sense, which Kuhn takes as the major source of criticism and misunderstanding of his work, is that the term “denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science” (175), that is, a paradigm is a shared example. For my purposes here, nothing crucial depends on the distinction between these senses.
However, when a paradigm’s anomalies become sufficiently troublesome, it enters a crisis phase where revolutionary ideas can arise. These new concepts can crystallise into another paradigm, and if it resolves or dissolves the anomalies of the old paradigm, it may gain sufficient popularity to supplant its predecessor during a “paradigm shift.” Any two paradigms are “incommensurable,” in the sense that there is a severe discontinuity between their various facets (including language), and so the scientific progress across a paradigm shift is not inherently cumulative, since most of the old paradigm will be discarded or reformulated in the new paradigm. Kuhn explicitly states that in some sense “the proponents of competing paradigms practice their trades in different worlds,” (1962, 150) and coming to see things through another paradigm is much like experiencing a gestalt shift.\(^2\) The incommensurability of research programmes led to some of the sharpest criticisms that Kuhn received, with Lakatos (1978b) remarking that “scientific change—from one ‘paradigm’ to another—is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the (social) psychology of discovery...Scientific change is a kind of religious change” (9). Other criticisms have been made of Kuhn’s notion of scientific paradigms. For instance, Laudan (1977) argued that Kuhn ignored the role of conceptual problems, didn’t resolve the relationship between paradigms and their theories, and didn’t allow for paradigms to be corrected by data. Laudan also held that Kuhnian paradigms run into problems because they are implicit rather than explicit parts of scientific practice (74–75).

In subsequent works, Kuhn moderated some of his stronger claims (particularly the incommensurability of paradigms and this talk of scientists “living in different worlds”), while also introducing new features, like his account of epistemic values relevant for making scientific progress, which remains influential today. The values he

\(^2\)Kuhn alludes to an image on paper that can look like either a bird or an antelope, but not both simultaneously. Another useful example is the Necker cube, in which a three-dimensional cube is drawn on a two-dimensional surface so that one can see one or another side as the cube’s front.
lists (accuracy, consistency, scope, simplicity, and fruitfulness) will be weighted differently by different scientists, which provides room for rational disagreements amongst scientists that go beyond empirical evidence (Kuhn, 1977). Kuhn’s explanation of rational (if somewhat subjective) reasons for disagreements between scientists was meant to answer the charge that his account of scientific revolutions described science as an irrational enterprise. However, despite softening some of his more extreme claims, the core insight that scientific progress is not a strictly cumulative gathering and refinement of knowledge has persisted in the philosophy of science literature, with later thinkers developing their own concepts of scientific change.

Lakatos objected to Kuhn’s work, claiming that if his view of scientific revolutions is right, “then there is no explicit demarcation between science and pseudoscience, no distinction between scientific progress and intellectual decay, there is no objective standard of honesty,” and so there would be no criteria “to demarcate scientific progress from intellectual degeneration” (Lakatos, 1978a, 4). Lakatos also disagreed with Kuhn’s objections to Popperian falsification, arguing that Kuhn only targeted a naive form of falsificationism, while missing the more sophisticated version Popper was actually advocating (Lakatos, 1978b). Lakatos disagreed with both Popper and Kuhn, arguing that it is wrong to think of scientific revolutions as “sudden, irrational changes in vision” as Kuhn does, and that there is no “instant rationality,” a la Popper (1959, 1964), since criticism does not eliminate a theory through refutation in the form of crucial experiments, in part because “there is no refutation without a better theory” (1978a, 6).24 Instead of either monolithic paradigms that cannot be

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24By “instant rationality” Lakatos means the ability to make an immediate determination of whether or not to continue to trust a theory once there is experimental evidence calling it into question. He contrasts the instant rationality of Popper, who argued in favour of a brand of falsificationism, with his own slow process of score-keeping to determine scientific progress, with the ultimate determination of whether or not an experiment was “crucial” or not being made through rational reconstruction long after the fact. Even when evidence detrimental to a research programme is encountered, Lakatos argues that it isn’t irrational to stand by that programme until it has a consistent record of failure not shared by its rivals, and maybe not irrational to remain firm even after that point.
challenged except in times of crisis, or theories that are continuously vulnerable to
crucial experiments, in work culminating in (Lakatos, 1970), he creates a compromise
between the sophisticated falsificationism of Popper and the paradigms of Kuhn by
proposing a new unit of scientific progress: the research programme.

A research programme is centred around a “hard core” of scientific laws, theses,
and assumptions. Each programme’s hard core is made immune to refutation by a
“negative heuristic,” which is a set of methodological rules describing the research
avenues scientists working in the programme must avoid, chief among them those
lines of research inconsistent with the hard core. The hard core is surrounded by a
“protective belt,” a combination of a series of theories along with the various models,
scientific methodologies, and auxiliary hypotheses that are open to refutation. The
protective belt is expanded and shaped by the “positive heuristic,” the methodological
rules determining which lines of research should be pursued by scientists within the
programme. Lakatos (1978b) points out that “the negative and positive heuristic
gives a rough (implicit) definition of the ‘conceptual framework’ (and consequently of
the language)” of a research programme (47), and together they direct the force of any
potential falsifying evidence away from the hard core and towards the protective belt.
These elements operate within the Lakatosian MSRP, which describes how research
programmes are to be assessed. Since I will discuss Lakatos’s MSRP in Chapters 2
and 3, I will not go into further detail here.

On the flip side of Lakatos’s demand for strict demarcation criteria and rational-
ity across scientific change is Feyerabend, who held that “anarchism helps to achieve
progress in any one of the senses one cares to choose” (18). He argued that only
through methodological anarchism can we ensure that we don’t miss opportunities to

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25Feyerabend is not concerned with defending a particular sense of progress that anarchism assists
in achieving. Just before the quoted passage he claims that the very concept of progress (or advance,
 improvement, etc.) is maximally pluralistic: “Everyone can read [progress] in his own way and in
accordance with the tradition to which he belongs” (18).
expand our knowledge: “[t]he only principle that does not inhibit progress is: anything goes” (14). Moreover, he argues that this is the way scientific development has always occurred, which was only noticed relatively recently through the discussions in the history and philosophy of science prompted by Kuhn and others. Feyerabend cites examples from the history of scientific development, like the introduction of atomism in the ancient world, the Copernican Revolution, the modern acceptance of atomism, the wave theory of light, and the introduction of a new kind of mechanical methodology by Galileo, all of which he claims couldn’t have occurred unless “some thinkers either decided not to be bound by certain ‘obvious’ methodological rules, or because they unwittingly broke them” (14). Indeed, the distinction between science and non-science disappears as a consequence of Feyerabend’s methodological anarchism, a result he took to be a humanitarian good, since it struck against scientific chauvinism. However, “anything goes” is contrary to the assumption that there are articulable epistemologies of science. Since I find that assumption compelling (a feeling that has a great deal of traction among other philosophers of science), in what follows I will not be addressing Feyerabend’s critique against the assumption that there is something more systematic or restrictive that can be said about the practice of science.

Like Lakatos before him (whose MSRP acts as a compromise between Popper and Kuhn), Laudan (1977) borrows from his predecessors’ arguments, but also significantly departs from them (though not to the extent Feyerabend did). Like Kuhn and Lakatos, he abandons the view that science progresses in a cumulative fashion. However, Laudan’s framework departs from theirs when he states that the purpose of scientific theorising is to solve problems. From this perspective, he introduces a new unit of scientific progress, the research tradition. A research tradition is characterised by a shared ontology (the phenomena the tradition is concerned with) and a shared methodology (how the study of those phenomena should be conducted) for
its collection of theories. Unlike Lakatosian research programmes, a research tradition’s core elements (its shared ontology and methodology) are malleable over time. Research traditions are assessed through an analysis of how many existing problems are solved by its theories. Using this problem-solving sense of rationality, Laudan is able to (in principle) say which traditions should be provisionally accepted as “true,” but also which it is rational to pursue. Traditions are pursued because they have offered more, and more significant, recent solutions to problems facing them than competitors, indicating fruitfulness. This notion of pursuit allows newer traditions to challenge well-established ones. In an important sense, Laudan’s problem-solving framework flips the usual understanding of the link between progress and rationality, as he claims that his strategy is the “blurring, and perhaps the obliteration, of the classical distinction between scientific progress and scientific rationality” (5). Since we have a much clearer model of scientific progress than of rationality, his proposal is that “rationality consists in making the most progressive theory choices,” rather than defining progress by accepting ever more rational theories (6).

Laudan’s account of progress and his criticism of Lakatos sparked numerous dis-

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26 This malleability raises the question of how a research tradition can retain its identity over time, if these elements central to its identity don’t necessarily remain consistent. Laudan is adamant that the ontology and methodology will change over time, and takes this feature to be an improvement over Kuhn and Lakatos, though he recognizes the trouble this causes for the continuous identity of his research traditions. His solution is that there is a relative similarity over time, preserved by a shared, continuous history that grounds a research tradition’s identity over time, along with a claim that “at any given time certain elements of a research tradition are more central to, more entrenched within, the research tradition than other elements” and it is these that are “unrejectable” (99). However, the members of the set of these unrejectable elements will change over time, and Laudan can only gesture towards a solution to the shifting of the dilemma this creates, though he notes historical instances which he claims show these changes occurring within research traditions. As we shall see shortly, Laudan later advances what he calls a reticulationist model of scientific progress in order to explain how only some of these core elements of a research tradition will change at once, preserving the identity of the tradition as a whole through time.

27 The distinction between acceptance and pursuit has been discussed since Laudan. For instance, Achinstein (1993) distinguishes the logic of pursuit of a theory from the deductive and inductive justifications it may otherwise lack. On his view, scientists can reasonably pursue a theory if they are justified in believing that it will answer a set of questions they have in a way which satisfies some constraints they have on scientific practice. Franklin (1993) focuses on the importance of establishing an independent notion of pursuit, based on tentatively accepting experimental results in order to ground further work.
cussions, including a debate with Worrall (1988, 1989), who defended the fixed nature of the MSRP and charged Laudan’s research traditions with promoting relativism. By then, Laudan (1984) had introduced his reticulated model of justification in science, which describes a tripartite hierarchy between scientific theories, the methods used to investigate and support those theories, and the various cognitive aims that motivated both the theories and the methods. His reticulationist model involved “a complex process of mutual adjustment and mutual justification” of each of these levels of the hierarchy, where “[j]ustification flows upwards as well as downward in the hierarchy, linking aims, methods and factual claims [theories],” such that each part of the hierarchy is intertwined with the others and no part is privileged (62–63). There is rarely a major change in all three levels of scientific commitment simultaneously (contrary to the complete change demonstrated by Kuhnian paradigm shifts). Therefore, the reticulationist model allows for an increased degree of fixity over his earlier account of research traditions, since Laudan argues that a significant change in either the goals, methods, or theories of a tradition are unlikely to lead to dramatic changes in the other two levels. Like Lakatos, Laudan relied upon the rational judgements of philosophers and historians looking back upon instances of scientific changes in order to assess them, adding to the problem by accepting that such viewers exhibit shifting axiologies. This re-introduces Worrall’s worry over relativism.

Laudan attempted to establish a program to study instances of actual historical changes in science at Virginia Tech (see Donovan and Laudan, 1988). However, it met with harsh criticisms from both historians (who objected to the philosophers taking the role of theoreticians making predictions) and philosophers, who “complained that Laudan’s metatheory of rationality did not match his first-order, problem-solving-progress theory of rationality” (Nickles, 2017). Details of Laudan’s account of problem-solving rationality will be explored in more depth in Chapter 3. For now, we will turn to a discussion of models in scientific practice.
1.2.2 Scientific Models

Since my modification to research programmes includes a more significant role for scientific models, it is important to review philosophical work on scientific modelling practices. Discussions of models have a less auspicious history in the philosophical literature than discussions of theory, though in recent years there has been a much greater emphasis on the roles models play in science.\textsuperscript{28} Models had long been treated as less important than theories, as entities barely worth commenting upon in their own right. The logical positivists adopted a syntactic view of theories, in which a theory is taken to be a set of sentences in a system of first order logic. Models acted as sets of semantic rules for interpreting the sentences of a theory, so that different models can provide different interpretations of a given theory (see, e.g., Campbell, 1957; Nagel, 1961). As Braithwaite (1953) put it, theories and models have the same formal structure, but have different “epistemological structures,” so thinking about theories in terms of models is using “as-if thinking” for which it is important to remember that it is useful to think of a target systems as if they matched the models’ abstractions only “if one remembers all the time that they are not” (93).

As a consequence of this reading of models, the syntactic view holds that models are largely superfluous to the advancement of science. Duhem, though not a logical positivist and pre-dating their movement, shared this dismissal of models in scientific practice. He claimed, for instance, that “the explanatory part [i.e., the model] has come to this fully formed organism [i.e., the descriptive element occupied by theories] and attached itself like a parasite” (Duhem, 1906, 32). Positivists like Carnap (1938) and Hempel (1965) argued that, at best, models had pedagogical or psychological value, but were otherwise not central to scientific developments as theories were.

This disparaging attitude towards models began to change, and more attention was given to models in their own right, with the rise of the semantic view of theo-

\textsuperscript{28}For more on the evolution of the perception of models, see (Bailer-Jones, 1999).

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ries. The semantic view reverses the order of priority from the syntactic view, with the central notion being that a theory is a family of models, making models central in scientific work. Suppes (1957), the founder of the semantic view, introduced the notion of scientific theories defined with set-theoretic predicates, rather than those of first-order logic. He further articulated the structure of theories as a “hierarchy of models” which “stands between the model of the basic theory and the complete experimental experience” (1962, 260), a hierarchy composed of models of theory, experiment, and data (see also, Suppes, 2002). Van Fraassen (1980), another prominent early promoters of the semantic view, criticised the earlier view of theorising as disconnected from actual scientific practice in a manner that frequently deceives us. Instead of viewing models as superfluous, he said that “[t]o present a theory is to specify a family of structures, its models; and secondly, to specify certain parts of those models (the empirical substructures) as candidates for the direct representation of observable phenomena” (64). However, van Fraassen’s view of models was more in keeping with their use in logic than in science, an interpretation that brings with it a formal requirement for isomorphism. Giere (1988) dropped the formal requirement of isomorphism for a less restrictive notion of similarity, though he noted that this requires a fleshed out account of what ‘similarity’ means. Other prominent semantic view proponents have further articulated the view (see, e.g., Suppe, 1989).

However, there have been more recent developments which suggest that even the semantic view fails to adequately capture the role of models in science. Instead of arguing that collections of models constitute theories, it has been argued that the best way of understanding scientific practice is to view some models as largely independent of both theories and data. One of the most important recent developments

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29 Suppes (2002) also argues that models should be at least partially isomorphic with their target systems.

30 Framing the issue in terms of the independence of models from theory raises the question of just what differentiates models from theory. This question is largely unaddressed in this portion.
in the philosophical understanding of models emerged with the publication of *Models as Mediators* by Morgan and Morrison (1999). This collected volume established the role of models as semi-autonomous mediators between theory and target systems. There are four elements in Morgan and Morrison’s account of models: construction, function, representation, and learning. Models gain a degree of independence from their construction “because they are made up from a mixture of elements, including those from outside the original domain of investigation” (15). Models can function as tools for theory construction, for exploring and experimenting on theories, as direct measuring instruments, and as instruments for the design and production of new technologies. According to Morgan and Morrison, models can represent either the theory, the target system, or both simultaneously. Finally, there are two opportunities to learn from models. First, we learn from a model’s construction by finding out what fits together and how. Second, models act as an epistemic resource that teaches us about the target system before we interrogate it directly.

The other contributors to the volume expand on the ideas introduced by Morgan and Morrison. However, there were earlier examples of the kinds of autonomy that Morgan and Morrison describe. For instance, Hartmann (1995) describes the role models play in theory construction, including developmental models behaving as

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31 Morgan and Morrison mean something different by ‘representation’ than is typical in the philosophy of science, where to represent something is to mirror it. They see representation as “a kind of rendering—a partial representation that either abstracts from, or translates into another form, the real nature of the system or theory, or one that is capable of embodying only a portion of a system” (27).

32 Of particular note for the present discussion is Hartmann’s (1999) contribution, in which he describes the need for a narrative thread within models, using examples from particle physics.

33 Leplin (1980) coined this term while describing how important models were in early quantum developments.
preliminary theories. Models also enter the picture when theories are too difficult to manipulate in a practical way. Redhead (1980) describes the case of quantum chromodynamics, in which the theory itself cannot be used to describe the interaction (hadrons interacting in an atomic nucleus) for which it is the fundamental theory. Cartwright (1983) describes this kind of situation as the standard way physics operates, where fundamental theories do not describe actual situations, leaving phenomenological theories or models to do the work of providing descriptions and making testable predictions.

There are many other important discussions of modelling that have arisen in recent years. For instance, Hughes (1997) offers the DDI account, in which physics models are argued to have three components: they denote some system (that is, they refer to it); models have an “internal dynamic whose effects we can examine,” allowing us to “demonstrate the results we are interested in” (S331–S332); and the demonstrated conclusion may be interpreted in terms of the model’s subject. Another important thread can be found in Humphreys’s (2002; 2004) discussion of computational models, which he uses to defend both his position of selective realism and the ability of the mathematical methods employed by such models to extend scientific knowledge beyond what unaided human senses allow. Knuutila and Loettgers (2016) expand on structuralist arguments that various equations and mathematical or computational methods found in some models are shared both within a discipline and across disciplines: some models will find applications in quite different domains from their original applications. This model transfer occurs even in circumstances where the model loses its original justification, though they note that such a loss might limit the insights that can be gained through these new uses. They trace the way the Ising model evolved from its original applications in physics, where its shifting applications were justified using renormalization group methods that kept it in the same universality class, to new utilizations describing socio-economic phenomena, where it had
no such justification. Finally, the literature on the way models represent is vast. Suárez (2010) provides a review of recent work on scientific representation, with a focus on the role it plays in modelling, though the details of this literature are beyond the scope of the present work.

In the arguments in Chapter 2, I will be interpreting models from the perspective of Morgan and Morrison, assuming models can be significantly independent from theory and their target systems. This view of models will allow me to talk about BSM models that are autonomous from the SM with respect to their notion of EWSB (and that are effectively meant to replace the SM as the dominant explanation of particle physics). I will also be borrowing Hartmann’s notion of developmental models used in theory construction, since becoming a new theory is the purpose of many BSM models. Hartmann’s discussion of the importance of stories for models will also be important, since such narrative throughlines are vital to interpreting the central concepts used by groups of BSM models as the hard cores of research programmes.

1.2.3 Non-Empirical Considerations

Since the situation in fundamental physics is often seen as a significant departure from “science as usual,” I will also consider the available literature on non-empirical theory assessment, largely driven by the works of Richard Dawid (2013). In *String Theory and the Scientific Method*, he defends the pursuit of string theory using non-empirical evidence acquired through three arguments concerning restrictions of scientific under-determination. Dawid argues that theories that have no alternatives and that have made unexpected explanatory connections between varied phenomena are likely to be successful, even if they have no empirical justification of their own. Theories that had these features in the past, and that later became empirically successful, provide

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34 In a recent article, Price (2018) has introduced the concept of the landing zone to help identify the conceptual tools used to initiate these model transfers across disciplinary boundaries.
meta-inductive evidence for the future success of theories that currently display those features but have no empirical successes. Dawid’s case study for these arguments is ST, which he claims has sufficient warrant for continued pursuit through these non-empirical arguments. Going further, Dawid also pushes for the acceptance of ST, arguing that its duality relationships\(^{35}\) provide reason to believe that any deeper, more fundamental theory will be conceptually equivalent to some form of ST. In later works, Dawid and collaborators formalise some of his arguments in Bayesian terms and argue that they increase the posterior probabilities of ST (see Dawid, Hartmann, and Sprenger, 2015; Dawid and Hartmann, 2018).

Dawid’s arguments led to a great deal of debate. Smolin (2014), while reviewing Dawid’s book, countered that the arguments worked just as well for loop quantum gravity as they do for ST. Since loop quantum gravity is a rival attempt at formulating a theory of QG, this undermines Dawid’s arguments because they cannot equally support two rival theories of the same phenomena.\(^{36}\) Camilleri and Ritson (2015) argue that Dawid’s arguments portray an overly simplistic view of the controversy over empirical evidence, and thus ignores the important critical role of heuristic assessment. Cabrera (2018) takes a similar tack. He claims that Dawid’s arguments aren’t necessary, since the current situation in fundamental physics is the result of a failure to properly distinguish between the contexts of pursuit and of justification. Since the controversy over the pursuit of ST would largely dissolve if we applied the norms proper to the context of pursuit to ST, rather than focusing on conflating pursuit-worthiness with epistemic justification, Dawid’s arguments should be understood within that context, instead of as a new methodology for understanding

\(^{35}\)String dualities are relationships between the mathematical descriptions of physics at the string scale and those at a higher energy such that the higher energy descriptions are informationally equivalent to those at the lower scale. As a result, the higher energy scales are redundant. See (Witten, 2001) for more on these duality relationships.

\(^{36}\)Dawid would argue in response that loop quantum gravity is not a true rival to ST, since it covers a more expansive energy range than the latter theory.
non-empirical theory assessment.\textsuperscript{37} The effects of Dawid’s book has also led to “Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics,” an interdisciplinary workshop held in Munich in 2015. Contributions to that workshop can be found in (Dardashti, Dawid, and Thèbault, 2019). My own criticisms of Dawid’s arguments comprise Chapter 4.

1.2.4 Philosophy of Particle Physics

Finally, there is a wealth of literature from both the humanities and the sciences that discusses the philosophical issues of QFT, the mathematical and conceptual framework of particle physics.\textsuperscript{38} Although QFT underlies the SM and BSM models that I examine with the hybrid framework of scientific progress I present here, much of the philosophical work only tangentially relates to my project, and that will be my primary focus in this review. Only a cursory look over the subject can be made here, in order to provide a broader context on the sorts of issues that arise in the literature surrounding the subject of my present work.

Since QFT is, as its name suggests, a field theory, but one that leads to a highly precise description of particle physics, a significant debate concerns its proper ontological interpretation. Is QFT about particles, or about fields? Physicists have long been divided on this question, including some of the founders of QFT: “Dirac, the middle Heisenberg, Feynman, and Wheeler thought particles should be the starting point in the formulation of the theory, whereas Pauli and the early as well as the later Heisenberg, Tomonaga, and Schwinger favoured fields” (Landsman, 1996, 512).\textsuperscript{39} There are a number of philosophical arguments in favour of a particle inter-

\textsuperscript{37}I formalise the shift to the context of pursuit and divorce of epistemic justification from pursuit-worthiness in Chapter 3.

\textsuperscript{38}Technical introductions to QFT, aimed at graduate students of physics, can be found in (Kaku, 1993; Peskin and Schroeder, 1995; Weinberg, 1995).

\textsuperscript{39}Indeed, one of the most common ways of modelling particle interactions is through the use of Feynman diagrams, which, to the naive observer, represent these interactions pictorially as the paths
pretation. Particles have a number of features that they do not share with fields, including their discreteness (Teller (1995) introduces a notion of primitive thisness to ground the individuality of particles): they are localizable, they have finite degrees of freedom, they operate only through local action, they are massive, and they are impenetrable (Kuhlmann, 2018). However, there are criticisms of the appearance of these features in QFT, or that the features themselves are indicative of a particle ontology. For instance, there are reasons to doubt that quantum particles can be properly individuated (see French and Krause, 2006). Bain (2011) has argued that classical notions of localizability don’t work in relativistic theories, including QFT. Fraser (2008) examines methods for integrating the particle concept found in free systems into interacting systems, finding them all unsatisfactory, and therefore concluding that the particle interpretation is not supported by the formalism of QFT.

There are also several interpretations of QFT that utilise a field ontology. Kuhlmann (2018) lists four: Teller’s (1995) interpretation of physical quantities emerging from a consideration of both the quantum field operators and the state of the system; the vacuum expectation value interpretation of Wayne (2002), who uses the expectation values of the quantum fields at a certain point in the vacuum state to recover the quantum field operators at that point; the wave functional interpretation, where quantized fields are interpreted in the same way as quantized one-particle states (see, e.g., Huggett, 2000); and a version of a non-localizable theory formulated by Baker (2009) that acts as a modification of the wavefunctional interpretation. Baker introduces this last interpretation of QFT in response to finding the other wave interpretations lacking. In particular, he argues against the basic wavefunctional interpretation, since “wavefunctional space and Fock space turn out to be equivalent” (589), so it

of discrete entities. Philosophical discussions of the way Feynman diagrams are used and interpreted can be found in (Wüthrich, 2012) and (Stöltzner, 2018).

40Classical notions of particles also come with a variety of conceptual difficulties: see Lange (2002) for a detailed description of issues concerning particle interactions in classical dynamics, as well as arguments that such difficulties require the addition of a field ontology.
is vulnerable to Fraser’s (2008) argument that “no Fock space can be defined for the interacting field” describing the interactions of free particles (605). Here, Baker is using arguments against the particle interpretation to also undermine the field interpretation of QFT.

Of course, there are other possible ontologies that go beyond the dichotomy of particles versus fields. For instance, Lupher (2018) argues for a modified field ontology, where determinables\(^{41}\) are assigned to open bounded regions of a Minkowski spacetime, rather than to spacetime points. He makes this case by borrowing notions from Algebraic QFT. Another prominent example is provided by Healey (2007), who argues for a holistic account of the ontology of gauge theories, based in part on the consequences of the Aharonov-Bohm effect.\(^{42}\) Healey’s “holonomy interpretation” is holistic in the sense that, instead of being localized, the fundamental physical quantities exist in closed, loop-shaped spacetime regions. Like Lupher, Healey’s account also invokes Algebraic QFT.

Other discussions in particle physics focus on its experimental practices. Franklin (2013) discusses the experiments in high energy physics, their data collection, and the way results are interpreted. For example, he begins by examining the history of the practice of using a five sigma standard for determining whether a discovery has been made.\(^{43}\) The use of this confidence level was brought into focus with the Higgs boson discovery in 2012, when the combined statistics of multiple channels at the ATLAS and CMS experiments were able to reach the benchmark (though Dawid (2015) notes that combining statistics in this way invokes the look-elsewhere effect

\(^{41}\)A determinable is a property like “having mass” which has no particular value. These were taken to be indicative of a quantum field ontology by Teller (1995), as opposed to determinant properties, like “having a mass of three grams.”

\(^{42}\)The Aharonov-Bohm effect is the effect on the complex phase of quantum particles by electromagnetism, even in regions of spacetime with no measurable electromagnetic field.

\(^{43}\)A five sigma confidence level is equivalent to having 99.99994% confidence that the effect is not an artefact.
when analysing the data\textsuperscript{44}).

Franklin (2002) has also argued that we must be wary of two problems associated with believing experimental results: the selectivity of data or analysis procedures (which are significant challenges for processes as complicated as the hadron collisions produced at the LHC and with machinery as complicated as the LHC detectors) and the resolution of discordant results. On a more fundamental level, Franklin et al. (1989) consider the possibility of using a theory-laden measuring apparatus to test the very theory used to build it.\textsuperscript{45} They conclude that such a device can be fruitfully used, so long as there is a way to independently calibrate it using standards that are independent of the theory used in building it.

The Higgs mechanism itself is of considerable philosophical interest. The SM account of EWSB arose as an analogy to developments in solid state physics that explain the phase transitions of superconductivity. However, the spontaneous symmetry breaking of superconductivity, described by Landau (1948), does not take the extra step of reifying, and making fundamental, the order parameter (in this case the Higgs field) involved. It is this extra step, adding “[t]he hypothesis that the Higgs field of the Higgs mechanism is fundamental, meaning that it has no constituents and

\textsuperscript{44}The look-elsewhere effect is a feature of statistical analyses of data in which a large parameter space can lead to observations that seem statistically significant, even though they do not represent real effects. In particle physics, as exemplified by the Higgs boson discovery, the large energy range examined for Higgs signatures increased the likelihood that a statistical fluctuation would rise to a relatively high level of significance, simply because there is a small but finite chance of such a fluctuation at each examinable energy level. Dawid cites the look-elsewhere effect as part of the origin of the five sigma standard, which is otherwise an extremely high bar for a discovery claim not adopted by other sciences, since no five sigma effect in a particle physics experiment has ever turned out to be a statistical fluctuation.

\textsuperscript{45}The theory-ladenness of the experimental apparatus can be seen as a significant problem in particle physics, where our most fundamental theory of matter is used to build detectors to probe the fundamental nature of matter. Franklin (2013) has provided many reliable strategies for circumventing the theory-ladenness in particle accelerator experiments, including exploiting physicists’ familiarity with the detector and the background processes, and establishing strict rules for data analysis. Beauchemin (2017), on the other hand, takes Franklin’s arguments for the reliability of particle accelerator experiments further by taking theory-ladenness as a positive feature that can be exploited to expand and validate our background knowledge through analysing the degree of systemic uncertainties in an experiment.
is as ‘real’ and indivisible as an electron or a top quark” to the hypothesized Higgs mechanism, which leads Wells (2018, 39) to call the Higgs boson an “immoderate speculation.” The SM Higgs mechanism has also been charged with being an ad hoc explanation since it introduces the only fundamental scalar of the SM, it includes a conceptually problematic vacuum pervaded by a non-vanishing Higgs field, it describes a spontaneous symmetry breaking (rather than the dynamical breaking much more prevalent in physics), it leads to a large number of unexplained independent parameters, and it suffers from a hierarchy problem (Friederich et al., 2014). These concerns were part of the impetus in constructing BSM models to describe alternative mechanisms of EWSB. Stöltzner (2014, 2017) describes many different kinds of BSM models of EWSB, their explanatory frameworks, and their motivations, while Borrelli (2012) discusses in detail the motivations behind, and theoretical adjustments of, CH models in particular, and Chall et al. (2019) discuss the state of BSM models since the Higgs discovery, primarily from the perspective of their confirmation status and their persistence in the face of an empirically successful rival.

A great many philosophical issues in particle physics remain, but one that is important for both the case studies I present in Chapter 2, and for future work on the present topic (outlined in Chapter 5), is the concept of naturalness. According to ’t Hooft (1980), who introduced it into QFT, “[t]he naturalness criterion states that one such [dimensionless and measured in units of the cut-off] parameter is allowed to be much smaller than unity only if setting it to zero increases the symmetry of the theory” (135). The technical formulation of naturalness states that “at any energy scale \( \mu \), a physical parameter or set of physical parameters \( \alpha_i(\mu) \) is allowed to be very small only if the replacement \( \alpha_i(\mu) = 0 \) would increase the symmetry of the system”

Friederich et al. argue that the description of the Higgs hypothesis as ad hoc is an important instance that “helps [in] understanding how scientists re-evaluate the hypothesis considered ad hoc in the light of this novel evidence” (3915), now that a particle closely matching the SM Higgs prediction has been found.
Wells (2015) discusses both technical naturalness, and a version of naturalness that ’t Hooft alluded to in his own formulations, that of Dirac’s naturalness, when discussing the concept’s usefulness in QFT. Wells holds that “[a] theory possesses Absolute Naturalness (or Dirac Naturalness) if its Lagrangian can be written with no very small prefactors, no very small dimensionless parameters, and no very small ratios of dimensionful parameters” (104).

There are multiple interpretations of what exactly naturalness means in particle physics (and the EWSB sector specifically), but one generally accepted conclusion is that, because scalar particles are affected by quantum corrections, there must be some explanation for the fine-tuning that separates the scale of EWSB from the Planck scale, or else the observation of such a light Higgs mass would look “unnatural.” However, the degree to which such fine-tuning is problematic is often considered to be an aesthetic judgement (see, e.g., Grinbaum, 2012) or an “artifact of convention” (Rosaler and Harlander, 2019).

Naturalness has been key to many of the BSM models that have been posited as a replacement to the SM mechanism of EWSB. Chapter 2 illustrates several instances of naturalness guiding model building in the SUSY and CH research programmes. Mättig and Stöltzner (2019) detail how views on the naturalness problem have been a primary motivating factor for physicists, and how the discovery of the Higgs boson shifted it from “a virtual into a real problem” (74). They focus on physicists’ beliefs about issues like naturalness at the time of the Higgs boson discovery, using questionnaires and interviews to understand the principles that guide BSM model building. They note that, though the questionnaire responses concerning the importance of naturalness remain consistent both before and after the Higgs boson discovery, inter-

47 For example, Williams (2015) notes that “there is a remarkable discordance of opinion” (82) in the physics literature on naturalness.

48 Williams (2015) disagrees with this assessment, arguing that “naturalness is a well-motivated expectation in particle physics whose apparent failure requires a significant revision of our understanding of the effective field theoretic description of nature” (83).
views reveal that “the absence of a cure for the naturalness problem of the SM has made some physicists wonder whether it is actually a deep problem or whether one should simply accept fine-tuning as a fact about nature and accept models that violate naturalness” (89). Wells (2015) argues that, even as confidence in naturalness as a guiding principle wanes, it is possible to come up with \textit{a posteriori} justifications for it (arguing that an uncompromising application of naturalness will, through a number of inference steps, lead from quantum electrodynamics to the SM, even without empirical input). However, he later argues that we should adopt a more sceptical, “moderate naturalness position,” which holds that “generally speaking, theories are not finetuned and they are natural, even though perhaps there is a small fraction of cases where there is some very high tuning” (2019, 19). This position holds that unnatural theories are rare, so searching for natural ones is still a “valid enterprise.” Finally, Rosaler and Harlander (2019) argue that the problem of naturalness for the Higgs boson mass is no more severe than finding an explanation for other SM parameters, so we should not be more bothered by the fine-tuning of the Higgs mass.

As this brief summary shows, the principle of naturalness has had an important role in guiding theorising in particle physics, but that role may very well be changing in response to the discovery of a SM Higgs. More work is needed to see how naturalness will influence model building in the future, as it is seen as less of a problem than before, and the direction of that work will be outlined in the Conclusion.

1.3 Structure

To build a new hybrid framework that accounts for scientific progress in epistemic situations where new models and theories have little or no experimental accountability, I will begin by describing in more detail the situation particle physicists found themselves in after the Higgs boson discovery. Chapter 2 outlines these circumstances, highlighting the stubborn persistence of various BSM alternatives to the SM Higgs
mechanism. In order to describe this behaviour in terms of the rational analysis employed by the philosophy of science, I invoke Lakatos’s MSRP, particularly the notion of a research programme. However, since the MSRP is so heavily focused on theories as the primary elements of research programmes, it appears that it cannot adequately accommodate the situation in particle physics, as the SM is the only (largely) uncontroversial theory available, and all its alternatives currently take the form of models that treat it as an effective field theory or a low-energy limit. Yet, there seem to be core ideas that are not found in the SM but remain consistent through time, such as the new fundamental symmetry invoked by SUSY, or the new strong sector of the CH models that leads to dynamical EWSB. These core ideas represent speculative new ontologies for particle physics which haven’t been empirically confirmed, but they do drive a great deal of theoretical work and model building. It is the persistent cores of the BSM models in the EWSB sector that lead to my first modification of research programmes, the introduction of the concept of the model-group as a new kind of scientific research programme.

Model-groups provide stability and a narrative (in the sense of Hartmann (1999)) for describing new physics. Once model-groups have been introduced, I explain how they can be incorporated within the MSRP, with individual models exploring the parameter space opened by the core ideas, and the core ideas themselves being shielded from refutation. The fit is quite remarkable, with model-groups in particle physics even demonstrating Lakatosian requirements like the existence of multiple competing research programmes. Lakatos describes newer programmes as incorporating aspects of older, more successful research programmes until they have the empirical support to stand on their own, just as the model-groups of particle physics incorporate the SM (absent the Higgs mechanism). In order to cement the utility of research programmes as a description of new physics models in the EWSB sector, I provide a pair of case studies that show how the protective belt around the SUSY and CH model-groups
changed in reaction to the discovery of the Higgs boson.

The MSRP is not ideal, however, since it supplies some unfortunate philosophical issues. In Chapter 3, I expand on some of the criticisms of Lakatos, with a primary focus on his account of scientific rationality, by examining the shortcomings of the Lakatosian analysis of ST provided by Johansson and Matsubara (2011). They apply numerous assessment schemes from the history of the philosophy of science to ST, but they focus on a Lakatosian analysis. However, their reading demonstrates the trouble of using Lakatos’s criteria for assessing ongoing research programmes, a problem that is highlighted by their invocation of Hacking’s (1983) paraphrase of those criteria. In citing Hacking, Johansson and Matsubara emphasise Lakatos’s view that a programme can be determined to be progressive or degenerative only long after the fact, using the standards of historians and philosophers who look back and engage in a rational reconstruction of the programme’s developments. Therefore, Lakatos’s own arguments hamstring Johansson and Matsubara’s attempt at a Lakatosian assessment, since any judgement of ST’s progressiveness will be premature until after the fate of ST has already been settled. By highlighting the nature of the retrospective reasoning in the MSRP, they provide a window into criticising Lakatos’s notion of rationality, opening an avenue for replacing it.

My second and more significant revision to Lakatos’s MSRP, then, is to replace his assessment of the progressiveness of research programmes with Laudan’s (1977) more prospective, problem-solving rationality. After introducing the basics of Laudan’s account, I argue that it is a better way of assessing research programmes in situations where empirical evidence is lacking for technical or conceptual reasons, since it offers a method that is (at least in principle) quantitative and amenable to pursuit, with the additional benefit that it can be applied to ongoing research programmes. With Laudan’s problem-solving approach, philosophers of science can assist scientists in analysing the problems and solutions of a research programme, and
therefore in appraising whether that programme is worthy of acceptance or pursuit. With something akin to Laudan’s view of rationality, assessment of a programme can be done on a much accelerated timetable compared to what Lakatos allows, and thus provides room for a philosopher of science to offer some degree of normative guidance to the scientists working on the frontiers. However, I do not advocate the adoption of Laudan’s entire methodology, since Lakatosian research programmes fit better in the contemporary situation of particle physics. By incorporating this one aspect of Laudan’s account, many of the objections to the MSRP are answered, including most of Laudan’s own. One of the remaining criticisms Laudan offers of the MSRP, that research programmes are too restrictive, I will argue, is actually a positive feature, considering the way BSM models are treated in particle physics in this post-Higgs-discovery landscape.

However, my hybrid framework can still be supplanted by other accounts of scientific progress if we can improve the confirmation of theories in non-empirical science. As mentioned above, Dawid (2013) offers a way of using non-empirical evidence to provide confirmation to various theories, a crucial step in accounts of scientific progress which require a notion of truthlikeness or some degree of veridicality. Dawid’s case study is ST, and he argues that it has no alternatives as an explanation for its range of phenomena, coupled with its ability to provide unexpected and unsought explanatory connections between various phenomena. Dawid argues that these features constrain the scientific underdetermination ST is subject to. Since other theories, like the SM, had similar restrictions on their underdetermination and were ultimately empirically successful, Dawid argues that ST receives meta-inductive support for its eventual empirical success. Despite offering a novel approach to assessing theories lacking experimental support, I ultimately find Dawid’s arguments unsatisfying. In Chapter 4, I raise objections to all three of his central arguments, focusing upon the third, his meta-inductive argument from the success of other theories. Because my arguments
undermine Dawid’s framework of non-empirical theory assessment, it loses its value in understanding scientific progress in these kinds of cases. This leaves only my hybrid account as a viable description of, at the very least, the situation in which particle physicists find themselves: theoretical developments are being made, but there are no current prospects for empirically confirming any attempts at new fundamental physics due to technological limitations and conceptual difficulties in generating predictions.

Finally, I conclude by briefly summarising the argument from the previous chapters. Because my hybrid framework leaves some remaining open questions, particularly concerning its applicability beyond high energy particle physics and how to properly conceive of the problems and solutions necessary to implement a brand of Laudan’s scientific rationality, I will also use the conclusion to point towards future research opportunities.
Chapter 2
Model-Groups as Scientific Research Programmes

2.1 Introduction

The standard model (SM) of particle physics is one of our best tested and confirmed theories. However, the SM has well-documented problems (including the hierarchy problem and a lack of explanation for dark matter (DM), gravity, neutrino masses, or matter-antimatter asymmetry) and physicists hope that probing the electroweak symmetry breaking (EWSB) sector at the Large Hadron Collider (LHC) may provide clues for resolving some of them. Alternative mechanisms of EWSB going beyond the SM (BSM) were proposed long before a particle closely matching the properties of the SM Higgs boson was discovered in July 2012. The SM account of EWSB is the Brout-Englert-Higgs mechanism, which describes the spontaneous breaking of the SU(2) gauge symmetry resulting in the masses of weak force bosons and other fundamental particles. It was accompanied by worries over naturalness and fine-tuning, and involved the introduction of a fundamental scalar field unlike anything

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1 Chall, C. Submitted to The European Journal for the Philosophy of Science, 03/02/2019.

2 As originally described by Englert and Brout (1964); Higgs (1964a, b); Guralnik, Hagen, and Kibble (1964).

3 The naturalness problem is roughly understood as the large, surprising, and unexplained difference in scale between some important parameters in the SM. Fine-tuning in physics is a measure of the precision of adjustments made to various parameters of a model to accommodate experimental observations. However, it is important to note that the nature and application of fine-tuning arguments in physics (and other sciences) are matters of contention, and the view that naturalness is a required criteria of physical theories has recently come under attack. For discussions of fine-tuning
else previously described by particle physics.⁴

These worries over the perceived faults of the SM account have driven BSM model-building. Even as the new particle’s properties were better determined and a consensus reached that it is indeed a Higgs boson (with a shrinking parameter space for alternatives to the SM account), alternative mechanisms of EWSB are still common in the literature, with work from several varieties of BSM models still regularly appearing in publication and the online preprint archive, arXiv.org. Though none of the EWSB models have received what can be considered unambiguous empirical support from data collected at the LHC and other particle accelerators, they haven’t been entirely excluded either. Work on these models continues, despite a lack of direct evidence. But the question remains: with a well-established competitor and growing reasons for doubt, what motivates the physicists pursuing these alternative models to continue? I will argue that a suitably modified reading of Lakatosian research programmes describes this persistence. In order to demonstrate this modification in action, I will provide case studies composed of the strategies employed by supersymmetry (SUSY) and composite Higgs (CH) models during the Higgs discovery.

First, I will give a brief overview of Lakatos’s methodology of scientific research programmes (MSRP). To accommodate BSM models of EWSB, my argument requires a modification of the MSRP, in order to better suit the current philosophical treatment of scientific models and their use in contemporary particle physics. This modification, the introduction of the concept of a ‘model-group’, will allow the BSM alternatives to be given a proper Lakatosian analysis. The final element of this article will be two in-depth case studies, focusing on the years around the Higgs discovery, where I will

⁴See (Friederich, Harlander, and Karaca, 2014) for an overview of the worries with the SM Higgs mechanism and (Wells, 2016) for an argument that the Higgs boson was an “immoderate speculation.”
show model-groups acting as research programmes, preserving their hard cores. By analysing the conceptual moves made within these model-groups during an episode of unfavourable empirical discovery, I hope to motivate both the need for, and the power of, the framework of research programmes in capturing the model dynamics of particle physics.

2.2 Research Programmes

2.2.1 The Methodology of Scientific Research Programmes

Lakatos’s (1978b) MSRP is meant to rationally reconstruct the history of science and show the growth of knowledge. A research programme consists of a series of successive theories, with new theories rising to replace theories discarded due to problematic experimental results and theoretical critique. The changes in response to challenges occur within the “protective belt” of the research programme, those auxiliary hypotheses, models, and other elements that can be treated as disposable. The specific bounds of possible changes to the protective belt are described in the programme’s “positive heuristic,” which details how the belt adjusts to problems. The positive heuristic also describes potential avenues for future development.

Conversely, the central tenets of the programme form its “hard core” and are insulated from critique. Once it is established, the hard core is protected by the programme’s “negative heuristic,” which mandates that the hard core cannot be challenged by experimental results:

“The negative heuristic of the programme forbids us to direct the *modus tollens* at this ‘hard core’. Instead, we must use our ingenuity to articulate or even invent ‘auxiliary hypotheses’, which form a *protective belt* around this core, and we must redirect the *modus tollens* to these” (48).

Lakatos did not clearly describe what sorts of elements make up a programme’s hard
core. His examples ranged from physical laws (the hard core of Newtonian physics, for example, consists of “the three laws of mechanics and the law of gravitation” (1978a, 4)) to postulates (Lakatos describes the hard core of Bohr’s research programme of light emission as consisting of five postulates, some of which encompass laws, but none of which are identical to laws (55–56)) to generalised conjectures (the hard core of the Proutian programme is given as “the atomic weights of pure chemical elements are whole numbers” (1978c, 118)). One thing that is apparent, however, is that the hard core involves ontological commitments that must be displayed by the theories composing its research programme.

A research programme is not determined to be ‘true’ or ‘confirmed’ or ‘acceptable’: rather, it is judged ‘progressive’ or ‘degenerative’. A programme is progressive if “its theoretical growth anticipates its empirical growth, that is as long as it keeps predicting novel facts with some success” (1978c, 112). A programme is degenerative if empirical progress outpaces its theoretical growth, leading to post hoc accommodations rather than predictions. A single negative experimental result is not sufficient to determine that a research programme is degenerative, since Lakatos explicitly rejects the notion of a crucial experiment, except when seen with hindsight. For Lakatos, an experiment can be seen as crucial only once it is clear that there can be no recovery from the disconfirming empirical evidence. According to Lakatos, there are three ways to resolve problems that arise for a research programme:

[B]y solving it within the original programme (the anomaly turns into an

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5 As we shall see in Chapter 3, Lakatos’s method of research programme appraisal has its downsides. A programme is only progressive in the sense that each new theory increases its empirical content over that of its predecessors, either by making new predictions (theoretical progressiveness) or by empirically corroborating some of those predictions (empirical progressiveness). Otherwise it is degenerating (1978b, 33–34). The MSRP also lacks any heuristic value in guiding scientific work (which is especially apparent in his response to the criticisms of Kuhn and Feyerabend, see especially (1978c, 116–117)).

6 To be more accurate, Lakatos here follows Kuhn (1962) in referring to puzzles, “a phenomenon which we regard as something to be explained in terms of the programme” (1978b, 72). More on problem-solving in the Lakatosian framework will be discussed in Chapter 3.
example); by neutralising it, i.e. solving it within an independent, different programme (the anomaly disappears); or, finally, by solving it within a rival programme (the anomaly turns into a counterexample) (1978b, 72).

An adjustment made to a programme to resolve a problem is known as a problem-shift.

Since it may take a while to determine the ultimate result of a problem-shift, there is generally a slow process of selection among competitors, which allows a research programme to safely go through periods where it makes no progress or accumulates anomalies (or both). It is not irrational to continue working on such a programme, since there is always the possibility that it will surpass its rivals in progressiveness sometime in the future. Thus, the proper assessment of research programmes involves rivals operating simultaneously, requiring some form of scorekeeping to track their progressiveness, while demanding humility and patience from those both making and observing scientific developments.

As noted above, Lakatos’s account of research programmes emphasised (series of) theories as the unit of analysis, and he placed models firmly in the protective belt. For Lakatos, a model is a simulation of reality with “a set of initial conditions (possibly together with some of the observational theories) which one knows is bound to be replaced during the further development of the programme, and one even knows, more or less, how” (1978b, 51). However, his view of models is outdated, and doesn’t match the practice of model building in particle physics. In the following sections, I will address some of the ways the view of models has changed in recent years, and how the MSRP can be adjusted to accommodate this shift in philosophical perspective.

There are other elements of the MSRP that might cause one to be ‘Lakatos intolerant’ (all credit and blame for the pun belong to Chris Smeenk and his presentation at the ‘Particle Physics at the Crossroads’ summer school in Wuppertal, Germany in 2018). However, for reasons of pertinence, I will focus in this chapter only on Lakatos’s poor treatment of scientific models. In Chapter 3, I will explore other criticisms of the MSRP.
2.2.2 Theories vs. Models

Using recent discussions in philosophy, supplemented by the common practices of particle physicists, I will propose a rough distinction between theories and (at least some) models. This distinction opens the door for the modification of the framework of Lakatosian research programmes, which will allow for a better understanding of the model dynamics of EWSB.

A great deal of recent discussion about models has concerned their autonomy from both theory and experiment. Morgan and Morrison (1999) argue that both the construction and function of models lead to a degree of independence from theory and data. In addition to this autonomy, Hartmann (1995) provides a taxonomy of models within what he calls the “Diachronic View,” which covers dynamic elements of science such as theory construction. These two viewpoints illustrate the dual purposes of this section: establishing some distinction between theory and model; and motivating the function of models within theory (or better, research programme) construction.8

There are many cases where models cannot function if they are too dependent on theory, such as when the theory is too complex and only a much more simplified model allows explanations or predictions to be extracted from it. Hartmann (1995) calls these “models as substitute for a theory”. Some models are constructed solely from a theoretical core and were never intended to match experimental results. They can still teach us valuable things about the phenomenology of the target system or the theoretical ecosystem, however, so Hartmann also emphasises the role of these “toy models” in theory construction.9 Finally, there are models used in cases where there are no theories at all, like what we see in BSM physics, where some models are frequently treated as preliminary theories (SUSY, for example, is sometimes described

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8For more on incorporating BSM models into the Models as Mediators approach, including Hartmann’s (1999) discussion of stories in particle physics, see Stöltzner (2014).

9Though he notes that the primary function of toy models is pedagogical.
as a theory) or extensions of more grounded theories (BSM models in general extend beyond the more empirically-grounded elements of the more general quantum field theory of the SM), though this situation occurs wherever there is no overarching theory available. Hartmann refers to these as “developmental models”, and argues that they play a critical role in theory construction. Each of type of model Hartmann describes appears in particle physics today, though it is this last type of model that will be crucial in modifying the MSRP, since it is most prominently displayed in BSM searches.

Since I am modifying the MSRP to accept collections of models in addition to series of theories, more needs to be said about what distinguishes models from theories in the first place. However, distinguishing models from theories is problematic since they share many of the same general features and functions. Therefore (and because there is such a wide variety of things referred to as models), my distinction will be limited in scope to models and theories as they appear in particle physics, and will possess a somewhat permeable boundary. I don’t find this limitation problematic, since my goal is to expand the understanding of research programmes to accommodate additional elements that are already related to theories, so finding an exact boundary between the two concepts is unnecessary so long as I justify the inclusion of things that are unambiguously models.

Consider the SM itself, which (as its name implies) began as a model. The current consensus is that it has become a theory in its own right.10 Morgan and Morrison (1999, 18) distinguish models as “account[s] of a process that is less certain or incomplete in important respects” while a theory “account[s] for more phenomena and has survived extensive testing” (18). This distinction between model and theory is “rough and ready” and cannot be applied universally, but it approximates what occurred in

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10Iliopoulos (2014) has suggested, for instance, that with the discovery of the Higgs boson, the SM is now complete and should be referred to instead as “The Standard Theory.”
the case of the SM’s transition from model to theory and aligns nicely with Hartmann’s class of developmental models. Using Morgan and Morrison’s distinction, the SM is now a theory, since it accounts for a wide swath of physical phenomena and has survived decades of empirical testing, though it was once a model. The various BSM alternatives we will discuss below count as models, since they lack the crucial empirical support. But, because they aim to go beyond the bounds of our present theories by describing phenomena at energy ranges that no present theory accounts for, they are specifically developmental models.

It is easy to see why this distinction cannot apply to all models. There are many roles models play that prevent them from being considered developmental models. Some models are designed to approximate a specific phenomenon described by a theory, some are meant to simplify a complex theory so empirical consequences can be derived, some are pedagogical, and so on. But it is the developmental models that we are presently concerned with. Developmental models have the potential to become full-fledged theories, a potential solely determined by the results of experiment, and thus the “rough and ready” distinction made by Morgan and Morrison applies to them. For our purposes, this is distinction enough to carve out new space in the MSRP: things that are not (yet) theories, but since they function almost as theories and have the potential to become theories, they are treated as scientific research programmes.

2.2.3 Model-Groups

Various BSM notions of particle physics have been developed, leading to numerous models of EWSB. Generally, these models introduce additional content to the SM, covering a wider range of phenomena (usually higher energy ranges or filling a known explanatory gap of the SM). Under our distinction between developmental models and theory, the constructs generated through these BSM strategies are classified as
models, since they lack the experimental support necessary to eliminate the significant uncertainty in their empirical adequacy required to reach theory-hood. We can classify many individual models as members of larger groups based on their commonalities, since each is constructed using a small number of common concepts, ontologies, or methodologies. In effect, there are several clusters of these common elements, with each cluster formed by requirements of consistency, and groups of models containing these elements constructed around them.

This natural grouping of models combines the way particle physicists typically organise these models with the framework of Lakatosian research programmes. By seeing the central tenets of these models as the hard cores of research programmes, we can introduce a new concept: the model-group. A model-group is composed of developmental models (along with conceptual techniques and mathematical tools) created to explore and test the consequences and empirical adequacy of a pre-theoretic hard core. My argument is that model-groups should be treated as research programmes on par with the series of theories that Lakatos described, each with their own hard core, protective belt, and heuristics. Only minor adjustments are required to the MSRP to accommodate them.

Let us flesh out the idea of model-groups as research programmes. The hard core of the model-group guides the construction of its individual members,\textsuperscript{11} each demonstrating different general concepts (like dynamic EWSB or a symmetry between fermions and bosons, for example) and methods for exploring the group’s parameter space, making predictions, and describing phenomena. In effect, the hard core provides a narrative for a new description of physical phenomena, in the sense outlined by Hartmann (1999). Individual models are created with the conceptual accounts and methodological tools that have been incorporated into the model build-

\textsuperscript{11}Borrelli (2012) offers a precursor to this idea, utilising the concept of “theoretical core” introduced in (Morrison, 2007) to distinguish models incorporating different BSM strategies.
ing strategies that make up the group’s positive heuristic. These strategies determine how the model-group will adapt to known or predicted challenges, both empirical and theoretical. Among other things, the individual models instantiate the parameters that are relevant to achieving the ideal set up by the hard cores, and what values these parameters will take. They then form the protective belt of the model-group. In a sense, the hard core provides the generalised concepts that each individual model of the group then specifies in different ways. Individual models have the potential to increase the programme’s empirical content because they make novel predictions that are open to testing, but this power comes with a liability. It is the individual models that are falsified when they do not match the data, while the hard core survives to generate new models: the specific instantiations of the concepts forming the hard core may be falsified, but the general account can still be instantiated in other ways.

The individual models in the protective belt must retain all ontological elements of the hard core (for example, the compositeness of the Higgs particle, or the symmetry between bosons and fermions of SUSY) while differing from one another in other ways. Typically, a model will have parameter values in its Lagrangian, which distinguish it from other models in the group; mass values for a predicted particle may be quite different in two members of a model-group, for instance. Different models from the same group may posit different energy ranges for symmetry breaking, or one may require new particles to remain mathematically consistent, while another doesn’t need them because it retains consistency through some other mechanism. The variability between models within a single group has two primary constraints: the hard core and the remaining available parameter space. The hard core constrains individual models exactly as expected: certain features will be present in each member of the model-group because they are constructed using the elements of the hard core. Only features that do not conflict with the inclusion of the group’s hard core are allowed. The parameter space itself is constrained by both theoretical considerations
and experimental findings. Models must operate within the bounds of the parameter space as it is defined by the best experimental evidence available. Thus, if a model describes phenomena within a section of the parameter space that has been determined to be excluded by experiment, then the model must be discarded or reworked, since its parameter values are no longer considered viable (though certain conditions have to be met: for example, there must be a consensus that the experimental data is trustworthy and that it excludes the region in question with a high enough degree of confidence).

The hard cores themselves can be non-specific, describing general ontological features but leaving the details to the individual models to fill in. In part because of this lack of specificity, it is unlikely that the entire parameter space for any of these BSM model-groups will be completely eliminated (it is also the case that they all extend to energy ranges beyond what is currently testable using existing accelerators). Therefore, it is possible that some new test will vindicate a programme. However, insulation from experimental exclusion is a double-edged sword: on the one hand, it provides the research programme with stability, preventing the core ideas from being discarded with every unfavourable result: physicists can continue their attempts to solve the problems of the SM without having to start from scratch with every failed prediction. On the other hand, it can lead to dead ends and charges of artificiality, in other words, to degeneration. Some corners of the parameter space are seen as much more promising than others, more in keeping with various pragmatic considerations (they are directly testable or are more natural, for instance). When these segments are steadily eliminated by negative experimental results, the creation of models to probe less promising avenues will seem increasingly ad hoc. Once the entire energy range open to the LHC has been probed, there will be nowhere else to look until new accelerators are built, a complicated, time-consuming, and increasingly expensive prospect. It may still be rational to pursue research programmes whose
parameter spaces have become increasingly marginalised, but we must honestly assess their virtues and failings, labelling them degenerative when appropriate.

One thing to note is that a diversity of model-groups is encouraged in the MSRP. The variety of rival programmes follows directly from the difficulty in concretely eliminating a programme, leading to long intervals where many programmes compete over the same range of phenomena. Indeed, Lakatos says that “[t]he history of science has been and should be a history of competing research programmes” and that “the sooner competition starts, the better for progress” (1978b, 69, my emphasis). Therefore, the number of competing BSM model-groups (at least five, as we will see in the next section) meshes quite well with their behaviour as research programmes.

There is a certain amount of permeability between model-groups, which is a necessary consequence of the fact that physicists don’t work exclusively within the boundaries of a single model-group: cross-pollination is bound to occur as physicists move between BSM projects, collaborate with colleagues, or peruse the preprint arXiv for novel ways to advance their work. For instance, you can find composite Higgs models that also contain extra dimensions (and thus blend two quite different strategies to account for EWSB). Supersymmetric variations of practically every other BSM model-group are common. Cross-pollination can occur even when aspects of the

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12 Lakatos is not explicit about why competition is better for progress. From the context, it likely stems from the way programmes are assessed: since programmes are only assessed in hindsight, competition increases the likelihood that some programme will be increasing its “heuristic power” during any particular timespan. Lakatos also remarks that without a rival, a scientist may feel a “hypersensitivity to anomalies and a feeling of a Kuhnian ‘crisis’” (1978b, 68). And without a more successful rival, Lakatos doesn’t deem it terribly rational to abandon an existing research programme, even a degenerative one. Of course, one could also see a sort of evolutionary account of the competition between different theories and models, like that described by van Fraassen (1980).

13 As an example, see the work by Da Rold et al. (2013), which was added to the arXiv in the August after the Higgs discovery. They combined the frameworks of both composite Higgs and Randall-Sundrum models of warped extra dimensions in order to predict deviations from SM predictions in flavour physics.

14 For example, Buyukdag et al. (2019) consider a supersymmetric model that uses the notion of partial compositeness (see Section 2.3.2) to explain the lack of supersymmetric particles found at the LHC, the mass hierarchy of fermions, and even provide a dark matter candidate. Likewise, Delgado et al. (2016) embed supersymmetry into a five-dimensional theory in order to accommodate
two research programmes are inconsistent, as long as those inconsistencies at least have the appearance that they can be resolved.\textsuperscript{15} Lakatos explains this permeability in the context of two rival programmes: since eliminating a rival is a long process, it sometimes becomes rational for a proponent of one to pursue a rival programme if possible, allowing her to further develop a vague programme “in order to show up its weakness” (1978c, 112). More plausibly, we can explain this willingness to work in multiple programmes by appealing to physicists’ agnosticism towards any model or theory without adequate supporting data. At least anecdotally, most physicists refuse to express even weak commitments towards any theoretical approaches that lack significant experimental evidence. Naturally, physicists who work within multiple model-groups will bring helpful conceptual tools and phenomenological across conceptual boundaries. What is important to note with this cross-pollination is that elements taken from rival programmes are incorporated into the protective belt, not the hard core, since the hard core of a settled programme is resistant to change. Thus, the composite Higgs models mentioned above incorporate extra dimensions not as a core postulate, but within the protective belt as part of some subset of individual models, which leads to some new predictions which can be tested without posing a risk to the CH hard core.

As I have shown, only a small adjustment is necessary to incorporate model-groups into the MSRP. We need merely to add to Lakatos’s understanding our expanded account of scientific models. Once the wider features and functions of models are incorporated into the MSRP, model-groups can be accommodated as research programmes by focusing on the role they play in theory development. They function analogously to research programmes composed of series of theories. Given the role of developmental models, this adjustment updates the MSRP, instead of acting as naturalness with a 125 GeV Higgs boson.

\textsuperscript{15}Lakatos describes how whole research programmes can be “grafted on to older programmes with which they are blatantly inconsistent” (1978b, 56), a process he referred to as ‘competitive symbiosis.’
a robust conceptual re-working of Lakatosian ideas.\footnote{Such a re-working begins to take place in Chapter 3, however.} In the next section, we will explore two examples of model-groups as they appear in particle physics in order to see what this adjustment looks like in practice.

2.3 **PARTICLE PHYSICS MODEL-GROUPS: TWO CASE STUDIES**

Articles discussing the merits of composite Higgs and supersymmetric models are still very common in the physics literature. However, both model-groups are experiencing pressures, largely due to the discovery of a Higgs boson that matches SM predictions and the non-discovery of new, non-SM particles. This situation raises the question of why these models persist in the face of unfavourable empirical data. The case studies I present below will show that our Lakatosian framework offers the best description of what is happening in the study of EWSB.

In what follows, I will provide a quick overview of the landscape of models for electro-weak symmetry breaking. The CH and SUSY model-groups will receive a more thorough treatment, including a review of the problems that arose with the Higgs discovery and the problem-shifts these research programmes underwent to protect their hard cores.

2.3.1 **THE EWSB LANDSCAPE**

Using recent work examining the landscape of models in the EWSB sector during the Higgs boson discovery (see, e.g., Borrelli and Stöltzner, 2013; Stöltzner, 2014), we can distinguish the primary model-groups within the EWSB sector: supersymmetry, non-SUSY Extended Higgs models, composite Higgs models, and Extra-Dimensional models.\footnote{A technique that has become popular within the physics literature is operator product expansions (OPEs) in the framework of SM effective field theories (SMEFTs). This technique is often referred to as a model-independent search strategy, which raises the question of whether...} These four model-groups do not exhaust the BSM EWSB alternatives, but
they comprise the majority of current SM alternatives. Only a cursory look at the non-SUSY extended Higgs and extra-dimensional model-groups will be provided here, and more detailed analyses of the CH and SUSY model-groups will follow in Sections 2.3.2 and 2.3.4.

The standard model itself remains the dominant research programme in particle physics, so a brief recounting of some of its features is presented here. The hard core of the SM, at least as far as the EWSB sector is concerned, is that there is a universal scalar field (the Higgs field), which causes spontaneous breaking of the symmetry of the electroweak force by condensing below a certain energy scale. Before symmetry breaking, all elementary particles are massless, but below a certain temperature three of the field’s degrees of freedom are “eaten” by the particle carriers of the weak force, thus generating the masses of the W and Z bosons. The fourth degree of freedom becomes the Higgs boson. Elementary fermions gain mass through a different type of interaction with the Higgs field. This process is the simplest working method posited for electroweak symmetry breaking. The SM does not provide a firm prediction of the mass of the Higgs boson, the particle associated with this field. Since, the particle’s other properties depend on its mass, the SM protective belt consisted of the values of the dependent properties, exactly calculated for each possible mass value of the Higgs (or at least for a number of fine-grained ranges of potential Higgs mass values). Now that physicists have found a particle matching predictions for Higgs boson properties at 125 GeV, the protective belt consists of a detailed accounting of its properties, along with predictions that require higher precision testing. The limits

SMEFT/OPEs are a model-group (or otherwise count as a research programme), or if they belong in some other category. On the one hand, there are consistent methods within this approach, and it still produces models in the semantic sense (see, e.g., Dawson, 2017). However, I don’t include SMEFT/OPEs in my collection of EWSB model-groups, since the technique is more of a broad mathematical searching strategy, without an easily discernible hard core, suggesting that the technique is a strategy for generating new research programmes, rather than a single programme in its own right. Determining whether it can be properly classified as a Lakatosian research programme is beyond the scope of the current discussion.
of LHC experimental sensitivity, then, determine the prospects for discovering new physics.

The BSM model-groups incorporate all of the experimentally accessible predictions of the SM into their hard cores. Each model-group acts as an extension to the SM, largely adding its own features to the SM Lagrangian or otherwise accounting for non-SM effects. This connection to the SM follows from the need for each BSM model to recover (or otherwise accommodate) the experimental results obtained at lower energies, which all accord with the SM. Lakatos states that a fledgling research programme can incorporate pieces of a well-established rival, even if those pieces are later found to be inconsistent with the new research programme. This process is necessary, since the established programme already has access to theoretical and empirical successes, which are resources the new programme will need in order to attract researchers. Since all the model-groups I consider here share the bulk of the SM elements, I will omit mention of them when discussing their hard cores and protective belts, focusing solely on their BSM elements.

The non-SUSY extended Higgs sector adds additional Higgs multiplets (usually in the form of two-Higgs-doublet or triplet models) to the SM’s sole doublet (which express the Higgs boson’s degrees of freedom). These models handle EWSB similarly to SUSY models, in the sense that they add more Higgs particles, but are significantly different because they don’t double the number of SM particles. The hard core of the extended Higgs sector consists of the extension of the SM to increase the number of Higgs multiplets, in order to preserve certain theoretical constraints, like naturalness, and provide an explanation for the asymmetry between matter and antimatter. Its protective belt consists of various models that adjust the parameters of the differ-

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18Or, more accurately in the case of the model-groups in our present discussion, most of these predictions are incorporated, with the SM Higgs mechanism being the notable exception. The EWSB sector is where the biggest divergence between the SM and BSM models can be found at lower energy scales, with all BSM model-groups discussed here introducing significant changes to the SM Brout-Englert-Higgs mechanism.
ent multiplets, indicating where to find new physical scalars and where to look for deviations in the properties of the boson discovered in 2012.

Extra-dimensional models explain EWSB through boundary conditions that lead to broken symmetries at higher dimensions (see, e.g., Csaki et al., 2004). A consequence of this method is that there is generally no particle of EWSB predicted, fundamental or composite (see, e.g., Csaki et al., 2004). The protective belt thus includes various models accounting for the boson discovered in 2012, usually in the form of claims that it is not actually a boson associated with EWSB at all. These individual models also describe how the extra dimensions can be probed and the specifics of how the boundary conditions break electroweak symmetry. The upshot of this model-group is that there is no need to introduce a fundamental scalar particle and there is a solution to the naturalness problem, which is a goal of most BSM models.

This quick overview of the EWSB sector shows that there is a wide variety of research programmes at play in particle physics. What follows is a deeper examination of the CH and SUSY model-groups (which is by no means aiming to be comprehensive). The CH model-group is situated roughly in the middle of the range of complexity of particle physics model-groups. It lacks the widespread appeal that SUSY enjoys, but has a longer history than the groups mentioned above. SUSY is the most prominent programme in BSM physics, and some physicists refer to it as a theory in its own right. Both of these model-groups have long histories, which include numerous evolutions, but I will restrict my examination (beyond an abbreviated history of each) to the timespan of the Higgs boson discovery.

2.3.2 The Composite Higgs

The composite Higgs model-group is a broad class of models that introduces the existence of a strong interaction at a high energy, which leads to strong, dynamical (as
opposed to spontaneous) EWSB. The group originated from the search for the mass generation of W and Z bosons, much as the SM account did, utilising a framework borrowed from superconductivity. Composite particles, including a composite Higgs, arise from the dynamically broken symmetry. The first examples of dynamical EWSB were intended to act as alternatives to spontaneous symmetry breaking (Jackiw and Johnson, 1973; Cornwall and Norton, 1973). A composite particle of EWSB is first mentioned by Goldman and Vinciarelli (1974), though it was soon expanded by Susskind and Dimopoulos, who borrowed from the “color” theory of the strong nuclear force, leading to the first Technicolor (TC) models. TC models introduced a local gauge symmetry representing a new interaction at the TeV scale. The gauge bosons acquire mass by coupling to Technihadrons, including a scalar that could serve as a Higgs impostor.

Susskind’s popularisation of the idea of dynamic symmetry breaking leading to a composite scalar boson of EWSB sparked many model building efforts. Mass generation by a composite system attracted lots of attention, since it avoids the ‘immoderate speculation’ of an elementary scalar boson arising from EWSB, as Wells put it. This explanation was later seen as providing a potential solution to the naturalness problem, since the addition of a compositeness scale, \( f \), creates some wiggle room to avoid fine-tuning certain parameters. Some variations included an account of new types of matter not covered in the SM by predicting new, undiscovered particles that would later act as DM candidates.

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19 Indeed, it originated as a toy model to demonstrate the feasibility of a dynamical approach: “It will be evident that this model is not intended as a realistic theory of weak or electromagnetic interactions. Rather, it is only an example of what we feel is probably a large class of theories in which the spontaneous symmetry breaking derives from general features of an apparently symmetric interaction” (Cornwall and Norton, 1973, 3338).

20 See (Susskind, 1979; Dimopoulos and Susskind, 1979).

21 It should be noted that some TC models are able to account for EWSB dynamically and without a resulting scalar, and are thus considered ‘Higgsless.’ Higgsless TC models are now highly excluded, due to the discovery of a boson that couples to other particles very closely to the predictions for the SM Higgs.
With the 1980s came the introduction of the notion of dynamical breaking of a global symmetry including a new strong interaction. Models using this notion could accommodate a light (pseudo)-Nambu-Goldstone boson (pNGB), generating the Higgs potential through radiative corrections. Since then, multiple variations have emerged, including varying iterations upon the TC theme (Extended TC, Walking TC, Topcolor, etc.) and the ‘Little Higgs’ (LH) models (which allowed for large, non-derivative interactions, particularly the Higgs quartic interaction (see, e.g., Agashe et al., 2005; Arkani-Hamed et al., 2002)). This history provides a host of examples of the CH research programme’s protective belt undergoing problem-shifts in response to empirical and theoretical pressures, but I will restrict my case study to the Higgs discovery. With its long history, its explanation of EWSB without elementary scalars, and its ability to solve perceived problems of the SM (like naturalness and the existence of DM), it is easy to see why the CH model-group has interested many physicists.

Turning from the brief historical overview to our Lakatosian analysis, the hard core of the CH model-group can be summarised as ‘EWSB is attributable to a strong dynamical process caused by new gauge interactions at high energy scales, so that the particle associated with EWSB (if any) is a composite, rather than a fundamental, scalar.’ The positive heuristic provides the various model building strategies that create the protective belt, including strategies and techniques to sustain model building against anticipated empirical and conceptual challenges. The protective belt is made up of various models describing the properties of the new gauge interactions and any physics associated with them, including predicted values for the parameters of any composite scalars and other particles associated with new strong interaction scales.

22For example, experimental findings involving flavour-changing neutral currents and the mass of the top quark motivated the creation of many of the TC variants long before the Higgs boson was discovered (see, e.g., Lane, 2002).
2.3.3 CH Problem-Shifts

The discovery of the Higgs boson created numerous problems for the CH model-group. First, as the search progressed, the upper limit for the possible mass of the new particle became lower than many CH models comfortably predicted. Second, the discovery of a Higgs candidate put immense pressure on Higgsless TC models. Third, its branching ratios, couplings, and flavour measurements very closely conformed (within LHC precision) to SM predictions. Since the SM Higgs is predicted to be a fundamental scalar, these results were taken as evidence that it was not a composite particle. Finally, no other new particles have been yet been discovered at the LHC, despite featuring prominently in the predictions of CH models. Each of these problems needed to be addressed to prevent the Higgs discovery from becoming a significant counterexample to the CH model-group. Too many counterexamples and no expansion of empirical content marks the CH model-group as degenerative, leaving the SM as an empirically successful (albeit flawed) rival that seems a more promising avenue of research.

Let’s first consider the mass. In TC, the vacuum expectation value, $v$, approximates the compositeness scale, $f$, implying the mass of the Technipion is large, and therefore requires significant adjustments to accommodate a Higgs as light as pre-LHC experiments were indicating. Despite the name, Little Higgs models also predicted a mass that was too high without suppressing its quartic coupling, so these models were already disfavoured by the time of the 2012 discovery announcement. After it was announced in December 2011 that there was an excess of 125 GeV in some detector channels, there was an increased urgency in the efforts to accommodate a light Higgs within CH models.\(^2\)

With the discovery of a 125 GeV Higgs candidate announced the following July,

\(^2\)See, e.g., Redi and Tesi (2012) for a discussion of possible light composite Higgs particles following this announcement.
a large portion of the CH model-group’s parameter space was excluded, since the mass was too low for many CH models to accommodate. There were two obvious ways forward: first, the boson still needed to be checked for signs that it was a “Higgs imposter” (a particle sharing many of the properties of the SM Higgs, without actually matching all SM predictions); and second, model building could focus on the remaining parameter space supporting a low-mass composite Higgs. Naturally, with the discovery of a SM Higgs candidate, proponents of all Higgsless models focused on the first strategy.\textsuperscript{24} Attempts were made to explain the existence of the boson using Higgsless TC models, arguing that it arose from a newly posited gauge field (see, e.g., Eichten et al., 2012), though this strategy, already somewhat \textit{ad hoc}, became increasingly unrealistic as more data arrived that continued to match SM predictions.

A close examination of the boson’s couplings and branching ratios was already underway, with many physicists hoping for an anomaly to indicate the Higgs search wasn’t over. One initially promising observation was the observed relative signal strength in the di-photon channel of both the ATLAS and CMS detectors, which didn’t quite align with SM predictions for the Higgs (see, e.g., Peskin, 2012). Early talk considered the implications of this excess, whether it would reveal that the boson was not the SM Higgs, or whether it wasn’t a particle associated with EWSB at all. There were efforts to accommodate it within CH models: Chala (2013), for example, utilised the excess to create a CH model that introduced new pNGBs that could both explain the excess and provide a DM candidate. However, with further analysis the excess in the di-photon channel’s signal strength disappeared. Aside from the initial di-photon reading, the data demonstrated a remarkable match with SM predictions: for example, Ellis and You (2012) argued that the new particle did “indeed walk and

\textsuperscript{24}In a presentation shortly after the Higgs announcement, Pomarol (2012) displayed a graphic of a tombstone labelled ‘Technicolor Models’ and declared Higgsless models were dead. However, he anticipated the Higgs-imposter strategy, with the next slide showing a zombie emerging from behind the tombstone.
quack very much like a Higgs boson,” and as a consequence some of the CH model-group’s parameter space was excluded with a high degree of confidence. Accordance with the SM has only strengthened with time, though the limits of LHC precision ultimately underdetermine the particle’s precise properties, and therefore its exact nature. Among the proposals for next-generation particle accelerators include plans for high luminosity detectors capable of probing the Higgs boson’s properties very precisely, which would test for deviations from the predictions of the SM research programme.

One interesting solution to multiple problems for the CH model-group appears in the form of partial compositeness. Unlike TC or LH, partial compositeness is not a type of model, but rather a conceptual and mathematical tool for explaining the origins of fermion masses and flavour structure in CH models generally, while also accounting for the observed mass hierarchy in particle physics. Therefore, it acts as part of the positive heuristic of the CH research programme, directing model building in a potentially fruitful direction. Originally introduced in Kaplan (1991) as a response to problems with the top quark in TC, partial compositeness establishes a new heavy particle for each SM particle, so that each becomes a linear combination of elementary and composite states. The hierarchy of masses observed among fundamental SM particles is explained by each generation having a different degree of compositeness, with the lightest particles being mostly elementary and the heavier particles being more composite (fermions acquire mass because their composite sector constituents participate in EWSB, so particles that are more composite in nature would be heavier because they couple more strongly to the composite particle of EWSB). Indeed, this hierarchy also helps explain why no SM deviations have been observed, since the first two (and more precisely measured) generations have lower degrees of compositeness, suppressing BSM effects (see, e.g., Redi and Weiler, 2011). The flavour structure in models with partial compositeness does not preclude a fundamental scalar, so adding
a new elementary scalar does not become a fatal empirical problem for the CH model-group, though there is still the theoretical distaste brought about by including such a particle in a class of models that generally goes without them.

As the possible mass range of the EWSB particle lowered, CH models using partial compositeness to explain a light Higgs became more common (see, e.g., Azatov and Galloway, 2012). Since partial compositeness explained the lightness of the Higgs and the lack of SM deviations, it is no surprise that its prevalence in the literature expanded rapidly after 2012, as it acted as powerful problem-shift and protected the hard core of the CH research programme. Searching the physics preprint arXiv for CH entries citing (Kaplan, 1991) reveals that twelve such articles were posted prior to 2012, while more than 120 have appeared since, a ten-fold increase.25 These entries even follow the Lakatosian tradition of making risky predictions to expand empirical content, as shown in (Harnik et al., 2017), which provides models with predictions testable at the LHC. If the CH model-group is to have any hope of being progressive, such phenomenological predictions need to accompany the theoretical moves made to preserve the programme in the face of problematic experimental evidence. The CH research programme has made numerous shifts in addressing the problems introduced by the Higgs boson discovery, but the lack of any confirmatory evidence suggests that historians of science will ultimately deem its post-Higgs period as a degenerative one.

2.3.4 Supersymmetry

Supersymmetry represents a major unconfirmed symmetry of the Poincaré Group, that of bosons and fermions. SUSY includes a supersymmetry generator, Q, which

25 The search was conducted using the search term, ‘find c Nucl Phys B365 259 and d 1991->2011 and (k “Higgs model: composite” or k “Higgs particle: composite”),’ in order to find all papers citing Kaplan’s original research article prior to 2012, and ‘find c Nucl Phys B365 259 and d 2012->2017 and (k “Higgs model: composite” or k “Higgs particle: composite”),’ to find all the articles citing it after the December 2011 announcement by ATLAS and CMS that a Higgs boson candidate had been observed. This search was conducted in December of 2017.
mathematically describes the conversion of half-integer spin particles (fermions) into integer spin particles (bosons), and vice versa. This symmetry requires new particles, at least one corresponding to each SM particle.\footnote{Many CH models also predict heavy partners for all of the SM particles, associated with a new gauge field. The primary difference between the CH partners and SUSY’s superpartners is that the latter have spins different from their SM counterparts, while the former have identical spins.} Since none of these ‘superpartners’ retain all the properties of their SM counterparts (for instance, their masses must be different, or we would have discovered them alongside the known SM particles), we know that SUSY describes a broken symmetry. SUSY has many benefits over the SM: it solves the hierarchy problem by naturally removing the massive fine-tuning from the Planck scale,\footnote{The hierarchy problem, previously mentioned in Section 1.1, arises because the Higgs boson’s mass is so much lighter than the Planck mass, which is surprising because it was expected that the Higgs boson mass would receive quantum contributions from every particle it couples with, making its mass comparable to the scale of new physics (either the Planck or grand unification scale, both requiring much higher energies to probe than available at the LHC) without a fine-tuned correction of the order of $\sim 10^{30}$. Since SUSY provides a symmetry between fermions and bosons, and the quantum contributions to the scalar mass from superpartners have opposite signs, SUSY contributions cancel out a lot of the contributions from SM particles and a light Higgs mass matching what was observed is expected.} unifies the gauge couplings at high energies, and some of its superpartners can act as DM candidates (see, e.g., Martin, 1997).

Gol’fand and Likhtman (1971) and Volkov and Akulov (1973) independently discovered the earliest SUSY variations, with a fermionic extension of the Poincaré Group and an analysis of neutrinos in 4-dimensions respectively. These early developments included the introduction of what became known as the superalgebra, which established the commutation relations of SUSY generators. The renormalization features of a quantum field theory linking fermions and bosons together were provided by Wess and Zumino (1974a,b,c). After that, SUSY was quickly seen as a serious contender for a viable alternative to the fledgling SM (see, e.g., Fayet and Ferrara, 1977), though it increased the number of unknown parameters in the SM range, since at least its most natural versions must be broken at relatively low energies. The most generic extension, first proposed by Dimopoulos and Georgi (1981), is
the minimal supersymmetric standard model (MSSM), which was meant to include only the minimum number of new parameters necessary to recover SM phenomena. The MSSM has had its own offshoots, including an even more constrained variation (cMSSM) and a more open version, the next-to-minimal supersymmetric standard model (NMSSM). One of the more recent extensions, the phenomenological MSSM (pMSSM), utilises all the empirical data so far gathered, which act as constraints on a 19 parameter model.

SUSY EWSB occurs much the same as it does in the SM, though the Higgs couplings and branching ratios differ since they are affected by the existence of SUSY’s additional higher energy particles. Since an additional Higgs doublet is needed for consistency, SUSY also predicts additional scalar particles. As previously mentioned, the Higgs doublet provides four degrees of freedom, which produce the Higgs boson and the masses of the weak force carriers. MSSM is a two-Higgs-doublet model, and so predicts five physical Higgs bosons instead of just one: a light and heavy CP-even \( h \) and \( H \), a CP-odd \( A \), and two charged scalar bosons \( H^\pm \). These BSM particles are produced by the extra four degrees of freedom from the additional Higgs doublet. Because of the symmetry between bosons and fermions, each of these Higgs particles is also associated with a new matter particle, the so called “Higgsinos.” Other SUSY models may introduce additional singlets or doublets.

The part of SUSY model-group’s hard core relevant to EWSB can be summarised as follows: “Spontaneous symmetry breaking leads to the mass generation of both the SM particles and their SUSY counterparts. Additional scalar bosons, the fermionic Higgsinos, and differences from the SM Higgs couplings are consequences of the symmetry between fermions and bosons.” The positive heuristic provides the techniques and tools for setting and adjusting the parameters of the SUSY particles as experiments rule out certain values. The protective belt is composed of models with various settings for these parameters, such as the MSSM and its various modifications
(cMSSM, NMSSM, pMSSM, etc.). These models make predictions of the expected SUSY differences in the finer properties of the Higgs boson, and of the energy ranges in which to find supersymmetric particles, with the lightest detectable at the LHC, at least in theory.

2.3.5 SUSY Problem-Shifts

Like the CH model-group, proponents of the SUSY model-group understood the problems posed by the Higgs boson discovery. First, the mass of the newly discovered particle, while still within the range predicted by the MSSM, was high enough to impose severe constraints. Second, the boson’s branching ratios and couplings were found to fit quite well with SM expectations, but not so well with SUSY models that were most SM-like. Finally, no superpartners have been discovered, nor have any additional Higgs bosons been found. Once again, a failure to solve these problems poses a significant risk to continued trust in the existing SUSY models that describe a low energy symmetry breaking. As we will see, even if suitable adjustments can be made, they likely undermine one of the underlying motivations for favouring SUSY in the first place: namely that it solves the hierarchy problem.

More so than with the problems facing the CH model-group, the problems of the more natural forms of SUSY are interconnected. For example, the mass of the Higgs proved immediately problematic for the MSSM (see, e.g., Arbey et al., 2012). The MSSM Lagrangian indicates that the (lightest) Higgs mass would be

\[ m_h^2 \approx M_Z^2 \cos^2 2\beta + \delta_t^2 \]

where \( \cos^2 2\beta \) is related to the ratio of the two Higgs doublets’ vacuum expectation values (\( \tan \beta \)), \( M_Z \) is the mass of the Z boson, and \( \delta_t^2 \) is the quantum loop correction from the stops (the top quark’s superpartners).\(^{28}\) In order to accommodate a mass

\(^{28}\)I’ve borrowed this formulation from Hall et al. (2012), since their work is utilised by many
of 125 GeV, it is necessary for $\delta_2^2$ to be quite large (just a bit under 90 GeV—near the Z boson’s mass), since all other values (besides $\tan \beta$) are experimentally fixed.

There are two ways to achieve sufficient corrections in MSSM.\textsuperscript{29} The first is to make the stops much heavier, since their mass depends exponentially on the mass of the Higgs. To properly correct for the observed Higgs, the stop mass would need to be at least a few TeV, though such a high mass reintroduces the sort of hierarchy problem SUSY was meant to solve, since it (along with the lack of superpartners discovered at the LHC) implies that SUSY particles are much heavier than the electroweak scale. Such heavy stop masses would also suggest that SUSY particles are out of the accessible range of the LHC. The second way of correcting for the 125 GeV Higgs in MSSM is to have a high degree of stop mixing.\textsuperscript{30} Maximal mixing allows the stop masses to be lighter, but requires the quantum loop correction to be very precisely fine-tuned. This much fine-tuning is seen as quite unnatural given the remaining parameter space, so this option also has a significant theoretical (and aesthetic) downside. In either case, the mass of the Higgs boson requires adjustments to the predicted masses of SUSY particles and the SUSY breaking scale, which in turn, mandates shifts in the expected Higgs couplings, shifts that have so far not appeared in the data. The overall effect was to create conceptual problems for the MSSM, since the rationales for pursuing it (simplicity, testability, and ability to neatly solve the hierarchy problem) were undermined. Thus, the use of the MSSM to resolve the problems of the SM and address the new problems introduced by the Higgs discovery has largely been a failure. Its place in the SUSY model-group is being increasingly ruled out, since it is no longer seen as a sufficient defence for the SUSY hard core.

\textsuperscript{29}See Hall et al. (2012) for more details on the following discussion.

\textsuperscript{30}``Mixing’’ refers to the linear combination of two or more mass eigenstates. Here, it refers to the way the stop couples with other particles, particularly the Higgs.
Many of the other theoretically well-explored SUSY models had to make similar adjustments. The NMSSM introduces a new singlet field, $\lambda$, that couples with the two Higgs doublets of the MSSM. Since the singlet contributes to the Higgs mass, NMSSM requires fewer adjustments to accommodate a 125 GeV mass. However, it was apparent even before the July 2012 announcement that there would still be fine-tuning involved, of about 5–10% if the mixing isn’t maximal (Hall et al., 2012). This fine-tuning requirement severely restricts the NMSSM parameter space.\footnote{It is unclear to what degree the squeezing of the available parameter space of a model-group contributes to issues like confirmation (see, e.g., Chall et al., 2019). However, as a practical matter, as the parameter space shrinks, there will be fewer possible models that can still conform to the data and theoretical constraints. It is also possible to exclude the parameter space within regions that are available for testing using our existing experimental devices, and there is still a great reluctance among many physicists (particularly experimentalists) to bother with models that cannot be tested for decades.} Similarly, Bechtle et al. (2016) use a frequentist analysis to show that the cMSSM should be excluded with a 90% confidence level. They base their analysis on available data from particle accelerators and astrophysics, which they combine with various toy models to obtain a meaningful $p$ value. The reason the remaining parameter space for cMSSM is under so much pressure is the tension between the model’s prediction of low mass scales for some SUSY particles on the one hand, and the higher mass scale preferred because of the observed Higgs mass (along with the lack of observed SUSY particles at the LHC) on the other. In the case of both the NMSSM and cMSSM, these well-explored models are not completely excluded, as Hall et al. (2012) and Bechtle et al. (2016) readily acknowledge. However, the focus seems to be shifting to more complicated SUSY models because of analyses like these. For instance, considerations of naturalness, one of the theoretical and aesthetic criteria that made simpler SUSY models attractive, are now being de-emphasised in some of the literature. Rather than the guiding principle of model construction it once was, some physicists are beginning to turn away from naturalness (see, e.g., Giudice, 2017), and so it no longer plays the same role in the positive heuristic of the SUSY research programme it once had.
These brief case studies reveal the attempts to solve problems raised by unfavourable experimental results at the LHC. No new particles or other BSM effects have been observed since the Higgs boson, despite the 13 TeV upgrade of the LHC and renewed focus on finding BSM physics. Though the SUSY and CH model-groups have been able to overcome many of these challenges conceptually, neither has truly expanded its empirical contents.\textsuperscript{32} However, there is remaining parameter space for the various BSM model-groups that is inaccessible at the LHC and necessitates waiting for the next generation of accelerators, which will achieve greater energies or higher precision. In the meantime, the spectre that these model-groups are degenerating research programmes looms. Until some historical distance has been achieved for the rational reconstructions necessary for a proper Lakatosian assessment of this period of scientific development, the Higgs discovery cannot yet be declared a counterexample to either the SUSY or CH model-groups on Lakatosian grounds. But the lack of scientific momentum may be sufficient to make these research programmes less appealing to the practising scientists. Still, new models are being explored by physicists all the time as the LHC continues its search for standard model deviations.

2.4 Conclusion

Lakatosian research programmes, as modified to include model-groups, capture the continued construction of BSM models of certain types, even in the absence of convincing empirical evidence for any BSM phenomena, and even in the presence of experimental evidence that is highly problematic for many specific BSM models. Physicists are able to set aside missing and contrary data for a time, until they are either able to explain previously damaging results (using the positive heuristic) or the programme collapses from lack of interest, starved for new empirical content.

\textsuperscript{32}The particulars of adapting to the Higgs discovery are different, but this assessment is true for the non-SUSY extended and extra-dimensional model-groups as well.
Within this Lakatosian framework, physicists have warrant to pursue promising research avenues and their own pragmatic interests, without falling prey to charges of scientific irrationality. With the introduction of model-groups, the framework of research programmes can be used to describe the current state of the EWSB sector. This modification requires an acknowledgement that there are clusters of models that are created using a consistent set of core ideas, constructed as potential avenues for finding and describing new theories of physics. By updating the MSRP with modern philosophical understandings of scientific models, incorporating the ‘models as mediators’ approach from Morgan and Morrison and the classificatory scheme introduced by Hartmann, we increase the utility of the MSRP within the realm of particle physics.

As the case studies of supersymmetry and the composite Higgs models show, the MSRP is a potent tool for understanding the process of scientific change and knowledge generation, even in situations where the empirical data is incomplete, hard to come by, or unfavourable to the lines of research that draw significant interest. However, the use of the MSRP to describe the model dynamics of BSM searches focused on EWSB is liable to leave one feeling unsatisfied. After all, Lakatos provides no real mechanism for assessing the progressiveness of an ongoing research programme, leading to a sort of vague feeling that the various BSM model-groups may be degenerative, but without being able to articulate exact reasons within the MSRP without rational reconstruction through hindsight. This problem, at least in my view, arises from Lakatos’s notion of scientific rationality. The inclusion of model-groups into the MSRP offers a potent first step in providing a rational assessment of physics research that requires significant non-empirical consideration for the foreseeable future. But a more comprehensive understanding, one that doesn’t rely on us analysing the his-

33This lack of assessment for ongoing research is addressed in the next chapter, where I resolve it by replacing the MSRP’s historical assessment with the problem-solving account introduced by Laudan (1977).
historical record, requires an account of rationality that addresses the flaws in Lakatos's work. The next step is to make a more significant adjustment to the MSRP, one that will allow us to make more immediate judgements about the amount of progress a research programme is making, and perhaps even allow the philosopher of science to employ her skills in offering normative guidance to scientists.
Chapter 3

String Theory, Lakatos, and Laudan

3.1 Introduction

String theory (ST) has played a prominent role in recent discussions of philosophical issues like confirmation and scientific progress. The question of how to treat scientific advancement in cases like ST, where empirical evidence is unavailable due to the physical limitations of our instruments, or in cases where a theory is incapable of making novel predictions that might be tested at all, is becoming more important in both fundamental physics and disciplines that study science. Empirical testing, after all, has almost universal status as a necessary benchmark of science and scientific progress, but theory is increasingly advancing beyond the technological limits of experiment. With so many understandings of progress being tightly bound to increasing the truthlikeness (or veridicality) of our theories (see, e.g., Dellsén, 2018), it is increasingly important to provide an account of scientific progress that doesn’t rely directly on experiment in the non-empirical contexts frequently found in fundamental physics and the historical sciences.

In the fundamental physics research represented by ST, we encounter two difficulties in assessing its empirical promise: the energy range where we expect to find unique ST effects is the Planck scale, which is far beyond the reach of the most

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2 For example, the 2015 Munich workshop “Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics” (and accompanying edited volume (Dardashti et al., 2019)) brought together both philosophers and physicists to discuss these topics.
powerful particle detectors currently feasible; and unique low energy predictions are
difficult for ST.\(^3\) One recent example of an evaluation of the scientific merits of ST
is provided by Johansson and Matsubara (2011), who examine ST through various
philosophical lenses.\(^4\) Here I will focus on their Lakatosian analysis, since aspects of
the Lakatosian model of theory progression prove particularly useful in cases where
empirical testing is unavailable.\(^5\)

However, in conducting their analysis, Johansson and Matsubara highlight the
problems that stem from using this form of assessment on an ongoing research pro-
gramme: Lakatosian methodological assessments are necessarily retrospective, rather
than prospective. Lakatos specifically aimed his methodology at historians and
philosophers working after the fact, which precludes any advice that can be offered
to scientists concerning which programmes it is rational to pursue. In what follows,
I will argue that Johansson and Matsubara’s Lakatosian analysis is premature, since
it will only truly be meaningful once there is some historical distance and the ST
programme has run its course. I will introduce a new approach to scientific progress
using the methodological strategies of both Imre Lakatos and Larry Laudan, taking
the familiar and versatile framework of Lakatos’s methodology of scientific research
programmes (MSRP) and combining it with the explicit and actionable problem-
solving rationality offered by Laudan. This new framework will have the benefits
of being applicable to ongoing research programmes and, since it takes conceptual
progress into account, allowing the philosopher of science to offer practical advice
on which programmes are pursuit-worthy in cases where no experimental checks are

\(^3\)It has been argued that ST makes no predictions whatsoever (see, for e.g., Woit, 2006). However,
it should be noted that Woit’s is a minority position among ST commentators.

\(^4\)Cartwright and Frigg (2007) and Camilleri and Ritson (2015) provide similar analyses of ST
from varied philosophical perspectives. Dawid (2013) provides a new philosophical assessment of
ST, aimed specifically at using non-empirical evidence to provide significant confirmation for it, but
also to argue for its implications for scientific progress and realism (see Chapter 4).

\(^5\)The application of Lakatosian arguments to developments in particle physics is introduced in
(Chall et al., 2019), and expanded on in Chapter 2.
possible. My hybrid approach, therefore, will offer a more complete understanding of the scientific progress made with ST, as well as the progress made by rival quantum gravity (QG) theories, or even other lines of research outside of physics which lack recourse to experimental evidence.

I will begin by briefly describing enough of ST and the MSRP to understand Johansson and Matsubara’s assessment of ST. Next, I will discuss that assessment and show how it highlights problems with Lakatosian assessments in general, especially when considering ongoing research programmes. As a brief aside, I will discuss Richard Dawid’s (2013) critical response to Johansson and Matsubara’s work, which, while similar to mine, leads Dawid towards a different resolution. My own solution to the problems with Lakatosian assessments will be to replace his account of assessment with Laudan’s problem-solving account. Once I have established my new hybrid framework for understanding scientific progress, I will use the elements of ST relevant to Johansson and Matsubara’s analysis in order to judge the programme’s pursuit-worthiness. As we shall see, under certain assumptions, there is a rational case to be made for the continued pursuit of ST on my account, though I won’t offer the kind of significant confirmation Dawid proposes.

3.2 A (Very) Brief Primer on String Theory

This section provides a brief overview of some elements of string theory and its history relevant to Johansson and Matsubara’s assessment. It is not meant to be a detailed accounting of ST, which would be beyond the scope of the present discussion. Instead, I will merely cover what is required to follow some of the philosophical claims that follow. For the curious reader, many overviews of ST are available: a classic textbook is (Green et al., 1987), while Polchinski (2005a,b) provides a more recent graduate-level introduction; a non-technical introduction can be found in (Greene, 1999); a prominent popular critique of ST is found in (Smolin, 2006); finally, a recent detailed
history of early ST, from its origins to the superstring revolution in 1984, is provided by Cappelli et al. (2012).

The original formulation of ST describes all matter and forces as one-dimensional extended objects ("strings") whose properties are determined by their vibrational states, instead of the point-like particles or fields that appear in other spatio-temporal theories. Strings themselves can be closed (having no end points) or open (having two end points), and have a characteristic length on the scale of the Planck length, or about $10^{-35}$ meters, the scale at which it is believed the effects of quantum gravity will become apparent. At this size, because the inverse relation between a particle accelerator’s energy and the length scale it can resolve, strings are far too small to detect with any experiment currently in operation, nor with any that are planned with available or anticipated technologies.

The first version of ST arose in the early 1970s. To be consistent, it required 26 dimensions and only described bosons. Supersymmetry (SUSY), which linked bosons and fermions through a symmetry of the Poincaré Group, allowed ST to deal with both kinds of particles, reducing the number of necessary dimensions to 10.

The six additional dimensions, which aren’t observed at the macroscopic level, are “compactified”: they have the topology of a cylinder or high-dimensional torus, and in the limit in which their radii approach zero, spacetime effectively appears four-

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6There are six different kinds of ST. M-theory, often described as the most fundamental ST, describes $n$-dimensional objects called “branes” (named because a 2-dimensional brane is a membrane). Through duality relationships, it is implied that all string theories are different, but physically equivalent, formulations of the same underlying theory, perhaps M-theory itself.

7The Planck energy is approximately $10^{19}$ GeV. Since the LHC can reach 13 TeV, our most powerful accelerator is 15 orders of magnitude away. The conceptual design report for the Future Circular Collider, a proposed successor to the LHC requiring billions of euros, several technological advancements, and a 100 km long tunnel, would reach energies on the order of 100 TeV, still 14 orders of magnitude from the Planck energy.


9This is Johansson and Matsubara’s reading. Actually, Neveu and Schwarz (1971) and Ramond (1971) describe the consistent Lie algebraic structure that was later used by Gervais and Sakita (1971) in one of the earliest examples of the spacetime symmetry now known as SUSY.
dimensional. As a consequence of the small scales required for spacetime to appear as it does, these extra dimensions are as hard to detect at low energies as the strings themselves.

Attempts to understand the uniqueness of ST led to an examination of the collection of possible parameter choices that determine the theory’s stable and meta-stable vacuum states (the quantum states with the lowest possible energies). There was hope that physicists would find a small number of ST vacuum states, at least one of which would lead to the observed cosmological constant, and thereby act as a retrodiction and explanation of a known physical constant, and thereby provide some degree of confirmation for ST. Instead, these efforts revealed that there is a great deal of freedom in constructing viable versions of ST (see Kachru et al., 2003). As a result, there is an immense number of possible vacuum states, with typical estimates ranging from $10^{10}$ to as high as $10^{500}$. With so many possibilities, finding the unique vacuum state giving rise to our observed physical constants is practically impossible, leading to a massive failure in an important retrodiction. Following Susskind (2003), many string physicists adopted the view of a string “landscape” and use the large number of possible ST solutions in an anthropic argument. The argument introduces a multiverse in which each universe emerges from one of these vacuum states. Thus we have a kind of explanation for the value of the cosmological constant from ST: we naturally find ourselves in one of the universes with initial conditions conducive to sustaining our existence, situated in a multiverse of other ST vacuums. As (Dawid, 2013) remarks, string theorists turned “a problem into a blessing and use[d] it for explaining the fine-tuning of the cosmological constant” (16).

ST is an ambitious project that represents a possible way of finding solutions for many problems in particle physics and beyond. To paraphrase Johansson and Matsubara, the three problems in physics that ST addresses are 1) the unification of the standard model (our best theory of fundamental particles, electromagnetism,
and the nuclear forces) and general relativity (our best theory of spacetime structure and gravity, and which is currently irreconcilable with the SM); 2) an explanation of the large number of particles we observe in nature, and whether or not they are fundamental; and 3) an explanation of free parameter values that must currently be put into the SM by hand (200-201). At the same time, ST faces fairly severe theoretical and experimental difficulties. Theoretical developments in ST have not improved empirical promise, as “no real breakthrough has been achieved that would allow specific quantitative calculations of observables from the fundamental principles of string theory” (Dawid, 2013, 17). There are scant prospects for testing ST because the scales involved are beyond what we can presently achieve, though there are experiments that could provide corroborative, rather than direct, evidence for ST. For instance, finding evidence for SUSY would be seen as a good sign, since ST needs the supersymmetric relation between fermions and bosons to be a consistent theory with universal scope.

The core ontological elements of ST, the existence of strings and compactified dimensions, count as testable predictions since they are discoverable, at least in principle. But it is impossible to conduct a search for them in the foreseeable future, since the energy scale required for such an endeavour is so high.10 These prominent difficulties have led critics to question whether physicists who pursue ST are behaving appropriately as scientific actors, whether the theory is even open for reasonable pursuit, and whether there can be any sense of progress, especially after more than four decades without an empirical payoff.11 With such prominent experimental problems, hopefully a philosophical assessment can shed light on the status of ST’s contributions to the advancement of science.

10 There are models of exotic extra dimensions that are within the LHC’s reach, but their prediction is not unique to ST (see, e.g. Tanabashi, 2018, 776-782), so again we’d have corroborating evidence at best.

11 For an expanded look at these criticisms, see (Penrose, 2005; Smolin, 2006; Woit, 2006; Hedrich, 2007).
3.3 Lakatos and String Theory

Before discussing Johansson and Matsubara’s assessment of ST, we must understand Lakatos’s methodology of scientific research programmes.12 As we saw in Chapter 2,13 the MSRP offers a way to rationally assess scientific research programmes, the unit of analysis Lakatos emphasized. A research programme is composed of a series of theories, one succeeding the next as they are refined in the face of empirical results, along with the auxiliary hypotheses and models that are necessary for the practice of science within the context of those theories. Research programmes are characterised by their protective belts and hard cores, as well as their corresponding positive and negative heuristics. The positive heuristic anticipates challenges and describes how some parts of a programme may be changed, for example, by tweaking model parameters or introducing auxiliary hypotheses. The mutable portion of a programme, composed of the various auxiliary hypotheses, models, experimental background, and so forth that make up a large part of scientific work, is called the “protective belt.” These elements protect the hard core from empirical challenges. The negative heuristic describes certain parts of a programme as off-limits to critique, at least once the programme has been established. The elements protected by the negative heuristic form the programme’s “hard core,” which defines the research programme by providing the basic ontology and structure represented in each of its theories.14

A research programme is assessed as progressive or degenerative (rather than true, empirically adequate, etc.). Progressive programmes have theoretical developments that precede empirical ones: theory guides experiment by making (ideally successful)

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12 The MSRP is a wide ranging project, primarily explained in (Lakatos, 1978b).

13 A large part of the discussion in this section reiterates the earlier discussion of the MSRP, though here more emphasis is placed on the assessment of research programmes, the rationality of science, and Lakatos’s view of scientific progress.

14 As we saw in Section 2.2.1, Lakatos is unclear about what exactly makes up a hard core: laws of nature, scientific postulates, and general conjectures about physical reality are all raised as possible hard cores for different programmes.
predictions. Degenerative programmes reverse this order: unanticipated empirical results are obtained first and must be accounted for afterwards to the extent they can be. Lakatos mentions the possibility that a research programme may be either theoretically or (inclusively) empirically progressive: theoretically progressive programmes are those in which each theory in the series expands on the empirical content with novel predictions, while empirically progressive programmes are distinguished by having some of these new predictions confirmed experimentally.\footnote{This way of categorizing research programmes ignores other kinds of theoretical progress that are typically claimed of theories like ST, like unification, the expansion of explanatory power, and so on. For instance, Lakatos states that making reductionist arguments doesn’t advance a research programme if it “does not produce new empirical content, let alone novel facts,” since a reduction “represents a degenerating problemshift—it is a mere linguistic exercise” (59). We will come back to this point in later sections.} Using this distinction, Lakatos establishes a demarcation criterion: research programmes that are at least theoretically progressive are scientific, while those lacking theoretical progressiveness are pseudoscientific (33–34).\footnote{Lakatos distinguishes these two forms of progressiveness in a section discussing Popperian falsification. However, since he takes the MSRP to be an extension and refinement of Popper, he is prone to previewing his own views when discussing sophisticated falsificationism. Because he discusses the two kinds of progressiveness in the context of a series of theories, it is clear that he is making his own distinction, rather than describing Popper’s views.}

The MSRP doesn’t require that (empirically) degenerative research programmes be immediately abandoned. Lakatos holds that it is rational for scientists to continue working on a degenerating programme, since it may one day become progressive. It is largely the scientist’s discretion that determines how long she works in a degenerative programme, even when a more progressive programme is available. Lakatos’s view of scientific rationality is driven by the actions of scientists, to such an extent that he rejects any methodology that does not account for the historical behaviour of scientists with regard to scientific progress.\footnote{Laudan (1987) argues against Lakatos’s meta-methodology, since scientists of the past will always have different background beliefs and axiologies than we do. Even if their actions seemed rational at the time, we should have no expectations that our current methodologies will find them so. Therefore, Laudan argues, we should emphasise the decision-making criteria that are actually being used in each case of theory assessment, which features in his problem-solving account.} Lakatos maintains that the final determination
of when a programme has superseded its rival can really be made only after a long
and difficult process, in which all support of a degenerative programme has dried
up. Only hindsight can reveal crucial experiments, and even this process of historical
reconstruction can be called into question, so such experiments are never the deciding
factor in abandoning programmes, leaving the decision to be made in the face of an
overwhelming accumulation of evidence and consensus against a programme.

3.3.1 Johansson and Matsubara

Though they cover a variety of theory assessment frameworks, Johansson and Mat-
subara prefer the MSRP, which they see as “the most reasonable analysis of scientific
development” (205). After reviewing the basics of the MSRP, they describe what
they take to be the hard core, protective belt, and heuristics of ST. Their take on
the hard core is too general to be very controversial: “(i) The fundamental objects
are not point particles but extended objects (strings or branes). (ii) Accept the basic
assumptions of quantum mechanics as given. (iii) Require supersymmetry of the the-
ory” (204). There isn’t much to say about ST’s negative heuristic, since it is merely
a restatement of the definition, i.e. “Don’t allow any modus tollens argument to be
directed against the hardcore” (204). However, their other assignments introduce
some controversy. Two elements they propose for the protective belt (that different
versions of ST are merely different formulations of the same theory and that string
physicists should explain the constants of nature using the string landscape) actually
appear to be elements of the positive heuristic of ST, since they are statements that
guide the construction of the actual components of the protective belt: the theories,
models, and auxiliary hypotheses that insulate the hard core from empirical findings.

18 In addition to the MSRP, they also discuss theory assessment proposed by the logical positivists,
Popper, and Kuhn.

19 I would be inclined to replace “quantum mechanics” with “quantum field theory” (QFT) or “the
standard model” in (ii), and strictly speaking, supersymmetry isn’t necessary for the consistency of
ST, just for STs covering both bosons and fermions.
The third element (that compactified dimensions cannot be observed with current accelerators) is a contingent statement, rather than an element of the protective belt or a heuristic for model and theory building. In fact, besides not necessarily being true, this third element undermines the point of the Lakatosian positive heuristic, since it rules out a possible test of ST, which forestalls assessments of progressiveness. Johansson and Matsubara state that the positive heuristic is comprised of ways of developing ST in order to offer a fundamental explanation of the variety of particles, to derive the natural constants, and to unify physics. However, the role of the positive heuristic includes being the storehouse of instructions for handling specific challenges, and none of the elements Johansson and Matsubara include fulfil this role. They may be intentionally describing the ST research programme in an abstract fashion, since the specifics are liable to change rapidly and will be esoteric for non-experts.

After describing how ST works as a Lakatosian research programme, Johansson and Matsubara turn to assessing it. They claim that ST, in comparison to rival quantum gravity theories, “has been progressive in a more general sense” (204) because it has attracted more researchers. We should resist understanding Lakatosian progressiveness this way, since his assessment criteria make no definite connection between the number of researchers who work on a programme and its progressiveness. Despite having a large group of dedicated scientists working on it, it is still quite possible for a programme like ST to be degenerative.

However, Johansson and Matsubara acknowledge Lakatos’s view of progressiveness is different from this “general sense.” Instead, they focus on the ability of ST to make and test predictions, particularly the failed attempts of string physicists to retrodict physical constants from theoretical principles, which led to the introduction

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20 See Section 3.2.

21 Lakatos (1978d) does make the points that “[i]nstitutionalized science is not participatory democracy” and that “[s]cientific decision cannot be based on majority vote” (154), which is the closest he comes to describing how we should view a research programme’s popularity.
of the string landscape. Since the string landscape is not part of the hard core of the programme, and since it can be seen as a post hoc rationalisation of an otherwise unfavourable development, they see its introduction as a sign of degeneracy. Johansson and Matsubara conclude that ST is a degenerative research programme since “the empirical facts against which string theory is tested was [sic] known in advance, no new testable empirical predictions have been made, and the mismatches that have been found have been a driving force in its development” (205). But without a progressive rival, they see no reason for physicists to abandon ST research.

Despite their negative assessment, Johansson and Matsubara go on to argue that comparing ST’s progressiveness to that of its rivals is impossible, since that would require comparing theoretical developments to empirical developments that do not exist. Instead, they discuss the distinction between theoretical and empirical progressiveness and how ST obviously possesses the former and not the latter. String physicists explain their continued pursuit by considering one axis of progressiveness, arguing that ST is worthy of pursuit because of its theoretical advances towards the unification of all the fundamental forces and matter particles, its ability to explain the constants of nature using the landscape, and the various mathematical applications derived from it. Meanwhile critics attack ST by pointing out its deficiencies along the empirical axis, claiming that more than forty years is too long to pursue a theory without any experimental corroboration, especially when there is no reason to expect the situation will change in the near future. Johansson and Matsubara also note the additional dimensions of progressiveness discussed by Cartwright and Frigg (2007), including practical benefits like the development of new technologies, theo-

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22 They do not mention that this distinction appears in (Lakatos, 1978b), perhaps because, as I mentioned before, Lakatos introduces it while explaining sophisticated falsificationism, and therefore it might not appear to be part of the MSRP. In any case, their view on theoretical progressiveness is different from Lakatos’s, who claimed it was an increase of the empirical content (i.e. phenomenological predictions) of the theories in a research programme, which is quite distinct from the framing Johansson and Matsubara provide.
retical advancements (like having wide applicability and solving difficult problems), and aesthetic notions (like elegance), though they underscore that these dimensions of progressiveness go beyond Lakatos’s own conception.

Finally, they contrast two perspectives on ST: internalist and externalist. The internalist perspective uses the theoretical applications of ST, as well as the mathematical developments it initiated, to explain what is attractive about working in ST. The externalist perspective, meanwhile, highlights the reasons to pursue ST besides potential theoretical advancements. These external reasons include the kinds of sociological pressures raised by Smolin (2006), such as the large number of university positions already held by string physicists, the amount of funding granted for ST research, and the number of physics students who work in ST, all of which give the continued pursuit of ST considerable inertia. But, since string physicists lack a more progressive programme to pursue, Johansson and Matsubara reiterate that, on their reading, string physicists are hardly being irrational in their persistence.

3.3.2 Criticisms

Before describing the issues with their Lakatosian analysis, it will be informative to discuss Dawid’s response to Johansson and Matsubara. In *String Theory and the Scientific Method*, Dawid charges them with “explicitly denying the ‘meta-paradigmatic’ character of the changes associated with the evolution of string theory” (28). What makes the case of ST meta-paradigmatic, according to Dawid, is that there is a disagreement between ST proponents and ST critics concerning the “meta-level ques-

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23 This is similar phrasing to Lakatos’s (1978c) own demarcation between “normative-internal” and “empirical-external” historical views of scientific growth, though again, Johansson and Matsubara don’t explicitly acknowledge the connection.

24 Dawid discusses Lakatosian research programmes only in reference to Johansson and Matsubara, though elsewhere in the book he discusses Kuhnian paradigms and Laudanian research traditions. However, Dawid has recently discussed Lakatosian ideas specifically, in, for instance, his presentation at the “Naturalness, Hierarchy, and Fine Tuning” workshop in Aachen, Germany in 2018.
tion of the choice of viable criteria of scientific theory assessment” (27). Dawid argues that Johansson and Matsubara evaluate ST to be degenerative because they use canonical views of theory assessment without considering these kinds of meta-theoretic shifts. Dawid appreciates their discussion of the differences between their own assessment of ST and that of string physicists themselves, though he diagnoses this as a problem that cannot be resolved unless they can “do justice to the arguments presented by the scientists in the field” (27). Dawid is essentially arguing that Johansson and Matsubara’s Lakatosian analysis is necessarily flawed because it labels ST degenerate based on one set of criteria for scientific argumentation (Lakatos’s), even though string physicists see it as progressive because their criteria of assessment have changed.

Here, Dawid gestures towards one of the criticisms of Lakatosian assessment, a criticism that will be discussed further in Section 3.4.1: they are insufficient in any understanding of scientific progress that includes research that cannot currently be checked by experiment, since they rely on the expansion of empirical content combined with some degree of confirmation of that expanded content. Dawid’s meta-level concerns, along with other criticisms (some of which I discuss below), necessitate a re-evaluation of classic notions of theory assessment and scientific progress that put all the emphasis on empirical progress. However, Dawid’s proposed supplement to these classic notions uses arguments that purport to provide non-empirical evidence, giving a significant degree of confirmation to theories that haven’t yet been empirically tested. This move has been criticized (see Chapter 4), but it is also unnecessary, since there are existing understandings of scientific progress that do take into account the conceptual progress that can be made in non-empirical cases and involve scientists’ own justificatory practices. The framework I propose below borrows Laudan’s

25 The prospect of a meta-level dispute between different approaches to scientific rationality across time has previously arisen in, for instance, (Feyerabend, 1975) and (Laudan, 1987).
problem-solving account of rationality, and in so doing, resolves Dawid’s problem with Johanssson and Matsubara’s assessment by offering non-empirical reasons for pursuing a line of research.

Two other comments are worth making. First, although Dawid complains that they don’t do justice to string physicists’ arguments in favour of ST, Johansson and Matsubara go out of their way to provide internalist justifications for continued work in ST. They discuss how it is tempting to consider ST theoretically progressive because of the great conceptual strides it has made. Johansson and Matsubara also introduce a distinction between the internalist and externalist perspectives on ST, covering many of the internal justifications for ST research while criticising Smolin’s externalist critique. They even reiterate Lakatos’s separation of Popperian ‘falsification’ from ‘rejection’, the claim that a research programme should be rejected “only if there is a better one to replace it” (Lakatos, 1978d, 150). In ST’s case, a charge of degeneracy doesn’t amount to much—ST is basically the only game in town, as far as QG theories with a universal scope are concerned. This reasoning actually anticipates one of the arguments Dawid uses in ST’s meta-level defence: the No Alternatives Argument (see Section 4.2.1).

Second, Dawid objects to the label of “degeneracy,” even though, from the Lakatosian perspective, working within a generally degenerative research programme is hardly a mark of irrationality or lack of scientific rigour. One of the criticisms Lakatos levies against Kuhn and naive Popperian falsificationism is that their methodologies assume an instant rationality. But in actual practice, “it is not dishonest to stick to a degenerating programme and try to turn it into a progressive one,” (Lakatos, 1978a, 6) at least so long as one is keeping the scorecard of empirical success and failure open and public. Dawid echoes Johansson and Matsubara by highlighting that string physicists and critics alike are well-aware of the shortcomings of ST. Lakatos acknowledges the differences in time frame between theoretical progress (which can
come as fast as new ideas are introduced) and empirical progress (which may take a frustratingly long time and involve many seeming refutations), and so he suggests instead that we consider “an intermittently progressive empirical shift” where “[w]e do not demand that each step produce immediately an observed new fact” (1978b, 49).

The label of degeneracy is not a value judgement about a programme’s pursuit-worthiness, nor the character, skill, and rationality of its practitioners. In the context of ST, with its technologically restricted experimental prospects, this assessment is merely a conclusion drawn from the lack of confirmation for the possible expansion of empirical content ST offers. So long as physicists take ST to be capable of making progress, Lakatos would defend their rationality, just as he would praise the efforts of physicists developing rival programmes. Although the connotation of the term ‘degeneracy’ is quite negative, when used in Lakatos’s technical sense, such a negative reaction to the term is largely unwarranted.26

Dawid’s criticism aside, the primary problem with Johansson and Matsubara’s assessment lies not with their determination that ST is degenerative, but from the fact that they make the assessment at all. Lakatos quite explicitly states that assessing research programmes is something done by historians and philosophers of science after the fact, through rational reconstructions. The problem with making an assessment of the progressiveness of an ongoing research programme like ST is emphasised by Johansson and Matsubara’s invocation of Hacking (1983), who enters their discussion of Lakatos’s defence of the rationality of choosing whether or not to abandon a degenerating programme. Hacking’s reconstruction of Lakatos, as presented by Johansson and Matsubara, emphasises that the MSRP is not meant to “give methodological prescriptions for the individual scientist,” but rather for judg-

26Granted, ‘progressive’ and ‘degenerate’ are value-laden terms and Lakatos’s use of them over value-neutral alternatives is certainly deliberate, which puts it in tension with his leniency towards scientists pursuing degenerative research programmes.
ing a programme “rational or not rational by the standards chosen, post hoc, by the philosopher or historian of science, independently of the beliefs of those working in the evaluated discipline” (Johansson and Matsubara, 2011, 204).

Hacking’s reconstruction follows directly from several claims by Lakatos that explicitly show that the MSRP is not meant to guide scientific research, but only to explain it after the fact. For instance, Lakatos (1978b) makes a strong historical case against the use of crucial experiments and Popperian instant rationality. Both the cruciality of an experiment and the novelty of predictions can be judged only after the fact, limiting their use in determining which research programme to pursue until the issue has already been decided and scientists have moved on (68–90). In discussing the demarcation between what he refers to as the internal and external histories of scientific progress, Lakatos (1978c) makes clear that methodologies of science (including his own) consist of “rules for the appraisal of ready, articulated theories” (103), rather than those under current development. This methodological role arises from a shift he sees in the sense of the normative guidance that philosophy of science can provide, from “rules for arriving at solutions” to “directions for the appraisal of solutions already there” (103). When he later addresses criticisms concerning the rational dividing line between progressive and degenerative programmes raised by Feyerabend and Kuhn, he claims that they “conflate methodological appraisal of a programme with firm heuristic advice about what to do” regarding the abandonment of a failing programme (117). As a last example, Lakatos (1978d) claims that “philosophy of science is more of a guide to the historian of science than to the scientist,” since “philosophies of rationality lag behind scientific rationality” (154). Because of this lag, he isn’t optimistic that making improvements to the philosophy of science “will be of considerable help to the scientist; although no doubt it may help...those great scientists whose scientific judgment was warped by the influence of previous, worse philosophies” (154).
In sum, Lakatos’s methodology is not aimed at advising scientists, but rather towards those working on historical reconstructions of scientific progress and their attempts to determine the rationality of past episodes of scientific development. The advice the MSRP provides would be something like “reconstruct periods of history of science in such a way that scientists are depicted as normally giving up degenerative programmes in favour of progressive ones when the objective conditions are fulfilled, disregarding their beliefs” (Johansson and Matsubara, 2011, 204). This advice is not entirely useless to scientists, who, if aware of the arguments in the MSRP, might strive to avoid working in a research programme they deem to be degenerative. However, scientists would likely be hindered in following such advice, since they lack hindsight concerning an ongoing process, leaving a proper assessment to be “only effective for the historian” (204).

Why, then, do Johansson and Matsubara attempt to make a Lakatosian assessment of ST at all? By their own reading of Lakatos and the MSRP, assessing an ongoing research programme like ST is premature, since Lakatos himself argued that properly assessing a research programme can only be done historically: the only norms the MSRP provide are directed towards historians and philosophers of science looking back on historical cases. The situation in ST is currently developing, so a historical reconstruction at this stage is premature because we don’t have adequate temporal distance from the ongoing debate. Therefore, Johansson and Matsubara’s attempt to assess ST’s progressiveness (or lack thereof) cannot be a proper Lakatosian

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27 One of Feyerabend’s (1975) criticisms of the MSRP is that it doesn’t describe when one should give up a degenerative research programme, and so is of no use to scientists. However, if a scientist took the MSRP into consideration, she might come to the conclusion that the rational course of action is to avoid pursuing a degenerating research programme, even without a specific requirement that she do so: the mere evaluation of degeneracy might sway her judgement. She might then attempt to assess the progressiveness of the programmes open to her.

28 To be sure, the inability to “make recommendations about cognitive action” is one of the criticisms levied against Lakatos by critics like Laudan (1977, 77; see below), but this difficulty is highlighted in Johansson and Matsubara’s use of Hacking’s reconstruction in their description of the MSRP.
They follow Lakatos in deferring to the judgements of string physicists a great deal, and so avoid offering any strong normative stance that contradicts those judgements. They point out that philosophers have no “privileged stance from which to decide” whether or not a scientist should abandon a research programme (208). Johansson and Matsubara are partly correct: philosophers often lack the knowledge and experience of those who have years of scientific training and research; even when they have such knowledge and experience, they are typically removed from the cutting edge debates in ongoing research programmes. The understanding of the minutiae of a scientific field held by the average philosopher of science will pale in comparison with that of a practising scientist working in that field. But this doesn’t mean that we cannot offer advice based on our skills in the conceptual analysis. One of the roles of the philosophy of science is to rationally assess scientific progress, and the tools of philosophy can be brought to bear to help guide the decisions of scientists in the thick of it, as well as to help historians describe these episodes after the fact. The MSRP offers no room for such a role, and so Johansson and Matsubara’s acceptance of the MSRP as “the most reasonable analysis of scientific development” (205) is perplexing, given that they also claim that “[t]he role of philosophers of science is to be active participants in an ongoing discussion on science and scientific methods” (208).

Johansson and Matsubara’s Lakatosian assessment, through its prematurity and lack or normative guidance, reveals a more general issue: Lakatos’s strange view of scientific rationality is ultimately not ideal for explaining and justifying scientific progress. Hacking (1981), for instance, characterises Lakatos’s concept of rationality as one that “abolished the very idea of ‘being a reason for’” (128). Among the many charges levied against Lakatosian rationality: it is retrospective instead of prospective, it emphasises rational reconstructions that aren’t accurate or rational enough to be persuasive, and it applies to only a particular sort of reasoning that has
only been in use for a relatively short time.\(^{29}\)

To resolve the problems of the MSRP’s mechanism for assessment, based as it is on Lakatos’s retrospective rationality, I advocate modifying the Lakatosian framework of scientific progress, especially in cases where the boundaries of experimental testing bar us from using empirical evidence to adequately assess research programmes. I suggest incorporating the problem-solving approach to assessing scientific progress introduced by Laudan (1977), rather than reconfiguring Lakatos’s account of rationality or constructing a new one from scratch. Laudan’s method of assessment is prospective and will provide normative guidance in a way that the MSRP does not. Since a lack of normative guidance is one of the issues that arose in our examination of Johansson and Matsubara, I think incorporating something like Laudan’s approach into the MSRP improves both our assessment of ST, and the Lakatosian framework generally. An assessment of problem-solving ability can be conducted on an ongoing research programme, so we can also implement an assessment of ST without having to wait for the science to settle to do so. In the next section, I will introduce the basics of Laudan’s problem-solving approach and make the case for incorporating his criteria for assessing scientific progress into the MSRP.

\(^{29}\)Hacking (1981) argues that Lakatos’s reasoning “takes for granted what we may call the hypothetico-deductive model of reasoning,” even though that model may be “a local and recent phenomenon” (142), an argument that is also echoed by Laudan (1987). They argue that Lakatos’s account of scientific rationality is not relevant to other forms of knowledge production, and may fail to be relevant to scientific reasoning outside of a particular historical context. If the character of scientific reasoning changes in the future (as it has done in the past according to Hacking, Feyerabend, and others), then the MSRP may no longer be capable of explaining scientific progress, even if it does now. This particular criticism is unlikely to appeal to a traditional scientific realist, who would argue that something like the hypothetico-deductive model of reasoning has been in use since at least the Scientific Revolution and is not amenable to revision in anything we’d understand as scientific practice. However, such a realist is likely to have issues with the MSRP as well.
3.4 Progress and Its Promise

3.4.1 Research Traditions

The methodological account of progress presented by Laudan adds to, but departs from, his predecessors like Kuhn and Lakatos.\textsuperscript{30} He levies these six primary criticisms against Lakatos:\textsuperscript{31}

C1 He has a “conception of progress that is exclusively empirical.”

C2 He requires that successive theories in a research programme either add new assumptions or offer a semantic reinterpretation of existing terms, such that for any two theories in a research programme, one must entail the other.

C3 In order to measure progress, a comparison of the empirical content of all theories in a research programme is necessary.

C4 He cannot provide “recommendations about cognitive action” from his assessment of research programmes.

C5 The accumulation of anomalies doesn’t impact the assessment of a research programme.

C6 Research programmes are “rigid in their hard-core structure and admit of no fundamental changes” (1977, 77–78).

\textsuperscript{30}Dawid (2013) briefly considers Laudan’s problem-solving account. His arguments for ST are similar in sentiment to Laudan’s framework (that “the classical paradigm of theory assessment grossly underrates that assessment’s theoretical impact”(44), but Dawid relies on assessments of scientific underdetermination rather than problem-solving. Chapter 4 offers arguments against Dawid’s theory assessment scheme, providing background motivation for the creation of the following hybrid framework.

\textsuperscript{31}(Laudan, 1977) also mentions other criticisms of the MSRP, including that it shares many of the flaws of Kuhnian paradigms. He also discusses further criticisms of the Lakatosian view of scientific progress in (Laudan, 1976, 1981a,b, 1984, 1987, 1996), though many of these criticisms are repetitions or variations of what is listed here. Therefore, in what follows, I will focus on these six.
I will address these criticisms after I have integrated the two methodologies, to see if my hybrid account remains vulnerable to them. Note that part of my concern with Johansson and Matsubara’s assessment of ST is similar to C4 (the lack of normative guidance), while Dawid shares Laudan’s worry presented in C1 (since he argues we can fruitfully supplement empirical conceptions of assessment and progress).

Laudan’s criticisms of Lakatos lead him to introduce his own model of scientific progress characterised by ‘research traditions.’ All research traditions are partially constituted by a collection of theories, either contemporaneous with one another or in a temporal succession. What binds the theories together into a research tradition is their shared methodology and ontology: they are about the same things and share methods of investigation. A research tradition is likely to change considerably over the course of its lifespan, up to and including a shift in its central methodology or ontology.

For Laudan, scientific theories are devised to solve problems, so new theories are added to a research tradition in order to resolve some problem faced by that tradition. The assessment of research traditions, then, depends on the ability of their theories to solve problems. But unlike Lakatos, Laudan admits a taxonomy of problems: both the standard empirical problems, which are the typical focus of philosophical analyses of progress, and conceptual problems are relevant. An empirical problem is something about the world that scientists feel requires an explanation. Since they “arise within a certain context of inquiry,” they are “partly defined by that context” (15). Therefore, different contexts will present different empirical problems, with some phenomena being considered problematic by some traditions but not others. An empirical problem is solved by a theory when scientists within that context of inquiry no longer consider it a problem, “when they believe they understand why the situation propounded by the problem is the way it is” (22). Laudan explains empirical problem-solving as a relation between the theory and the problem, such that
“a theory may solve a problem so long as it entails even an approximate statement of the problem,” (22). Laudan’s sense of entailment here is not that of first-order logic, in part because he thinks the truth or confirmation status of a theory is irrelevant to its problem-solving capacities. He is unclear in what he means by “a statement of the problem,” but from his examples he seems to mean that a theory solves a problem by predicting “theoretical results” that approximately correspond to our “laboratory results” (23).

Conceptual problems consist of internal inconsistencies, assumptions made by a theory that run counter to general metaphysical assumptions, theories that violate the research tradition they are part of, or the failure of a theory to “utilize concepts from other, more general theories to which it should be logically subordinate” (1981b, 146). In other words, conceptual problems occur when a theory has internal inconsistencies or vagueness, or when a theory conflicts with theories in other domains that are already considered rationally grounded. A conceptual problem is solved by some theory “when it fails to exhibit a conceptual difficulty of its predecessor” (148), either clearing up the internal issues, or some way is found to reconcile the theory in question with others in the scientific background that conflict with it. Laudan holds that “the elimination of conceptual difficulties is as much constitutive of progress as increasing empirical support” (147).

Of course, not all problems are on equal footing. Laudan provides ideas for how scientists weight problems, since, to be successful, a model of scientific progress “must provide some guidelines not only for counting, but also for weighting, scientific problems on a scale of relative importance and cruciality” (1977, 32). Empirical problems can be inflated when a solution to them is discovered (solved problems are often worth more than unsolved ones) or through the creation of new archetypical empirical situa-

32 This is meant to be an analogy with a logician’s description of the relation between an explanans and its explanandum, though Laudan is clear that it is a loose analogy.
tions (where some new features become the primary focus that everything else reduces to). Empirical problems can be deflated by being reframed as pseudo-problems, or when the domain of analysis is shifted (as was the case when the physiology of the eye was de-emphasised in the study of optics in the seventeenth century). Conceptual problems receive relative weighting by comparing the degree of inconsistency between two simultaneously accepted theories, or by the length of time the tradition’s practitioners have sat with the problem while searching for a solution (longer lasting conceptual problems are often seen as less problematic than newer ones, especially as new practitioners for whom the problem has always existed enter the tradition). Although Laudan’s proposals provide a starting point for determining these weightings, he did not attempt a complete account of how problems are weighted in research traditions. He acknowledges that his brief suggestions of how problems are weighted are neither exhaustive, nor meant to cover the weighting created by what he calls “irrational grounds,” (i.e. external factors concerning “moral, social, and financial pressures which can ‘promote’ such problems to a higher place than they perhaps cognitively deserve” (32)). More work is necessary towards properly understanding problems, but this is beyond the scope of the present work.

There are two axes of research tradition assessment: adequacy and progress. One research tradition is more adequate than another if its theories have solved more, and more significant, problems than those of its competitors. Progressiveness, on the other hand, factors in a temporal component, requiring us to compare the problem-solving efficacy (which can be understood as the ability to solve more of the significant problems of a research tradition to the satisfaction of that tradition’s practitioners

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33Laudan (1984) has since discussed the role of these “external” values in understanding scientific progress in more detail. There have also been arguments that they play an essential role in scientific developments (see especially Longino, 1990; Douglas, 2009).

34Nickles (1981) provides an excellent account of problems and problem-solving behaviours, which goes into detail which is beyond the scope of the present argument. However, he de-emphasises conceptual problems to a large extent, undercutting some of the reasoning for applying problem-solving to ST in the first place.
and neutral observers) of a tradition’s theories over some span of time. There are
two measures of progressiveness: the general progress (which we find by comparing
the problem-solving efficacy of the earliest theories with that of the latest theories)
and the rate of progress, (“the changes in the momentary adequacy of the research
tradition during any specified time span” (Laudan, 1977, 107)). Problem-solving
efficacy, therefore, acts as an in-principle measure of the progressiveness of a research
tradition. General progress can be quite different from a tradition’s recent rate of
progress, with the recent rate acting as a possible indicator of future fruitfulness.

These two assessments lead to two different recommendations for action: acceptance and pursuit. Accepting a tradition is to “treat it as if it were true” (109),
while a scientist can pursue a tradition that she doesn’t accept by working within
it without having strong commitments to its ontology or methods. For Laudan, the
rational course of action is to accept the tradition that is the most adequate within
a specific domain. However, there are situations in which a scientist has reason to
work within a tradition that she doesn’t yet have warrant to accept. If a tradition
has a higher rate of recent progress, that may indicate its fecundity or that it has a
“fresher approach” than the tradition she accepts, which becomes a reason to pursue
it.

However, for our purposes the concept of adequacy is of limited use for understand-
ing scientific progress, since the problem-solving score is liable to change frequently in
an ongoing line of inquiry. Adequacy is especially ill-suited in cases where empirical
evidence is currently unobtainable, like our ST example, because we should beware
discussing accepting a tradition that hasn’t received significant empirical confirmations. Adequacy also brings with it additional worries: for example, when comparing
an established tradition with a newer one, by virtue of having existed longer, the
established tradition is likely to have more solved problems. For our purposes here,
I will focus on progressiveness and pursuit-worthiness as the assessment criteria of
The distinction between these forms of progress provides the heart of Laudan’s normative guidance. We no longer have to worry about how to comport ourselves towards ‘degenerative’ research programmes, since Laudan is only concerned with degrees of progressiveness. He rejects the supposition that a theory of scientific rationality must be bound up with issuing judgements of truth, or confirmation, or even corroboration with experiment, since there are reasons (philosophical and historical) to suspect that we will never be able to obtain lasting surety that we have any of these things. Instead, he argues that “the chief way of being scientifically reasonable or rational is to do whatever we can to maximise the progress of scientific research traditions” (1977, 124). Laudan provides a pragmatic account of what kinds of scientific projects scientists should find worth pursuing, one that is based on contemporaneous assessments of the ability of those theories to solve problems. The problem-solving account of scientific progress has the twin benefits of explaining behaviour that is inexplicable within other schemas and in providing norms for scientific practice that other scientific methodologies, like the MSRP, do not.

3.4.2 Problem-Solving and Lakatos

Though Laudan’s methodology improves on its predecessors in many ways, a wholesale adoption of it for analysing string theory (and other non-empirical cases like it) is problematic. One of Laudan’s criticisms (C6) is that Lakatosian research programmes don’t allow fundamental changes because their hard cores won’t admit it.

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35In any case, Nickles (1981) argues that “Laudan’s distinction between pursuit and acceptance is overdrawn,” since “many types of heuristic appraisal apply as much to ‘accepted’ theories as to ‘pursued’ theories and programs” (106).

36Here, Laudan is rejecting common realist arguments, using an early version of his argument from pessimistic meta-induction, expanded later in (Laudan, 1981a).

37Examples of such behaviour include working in a less successful tradition while there is a well established rival, or simultaneously working on two incompatible theories.
Research traditions, by contrast, are much more malleable, potentially changing even ontologies and methodologies, their central identifying features.\(^{38}\) However, the restrictions provided by hard cores, protective belts, and heuristics are much more helpful for conceptualising scientific progress, since they specify consistent and unchanging ontologies, making it easier to mark the boundaries and interactions of competing research programmes and track them through time, which in turn allows for a deeper understanding of how various scientific ideas have behaved historically.

There are also cases where the level of generality inherent in research traditions would be too high, so important nuance is lost. For instance, as we saw in Chapter 2, in particle physics there are general classes of models that act as extensions of the standard model (SM), going beyond it and covering new physics (BSM). It would seem that these BSM models should be included in the same research tradition as the SM itself, since they are effectively extensions of its Lagrangian and rely on roughly the same mathematical methods, despite including non-standard-model features and ideas. However, physicists typically treat these extensions as distinct from the SM, since it is hoped that one will eventually supersede it. Indeed, on the most straightforward reading of Laudan, it is doubtful that we can say that there is more than one research tradition within particle physics, since all these models share, at least in certain energy ranges, the same ontology as the SM.\(^{39}\) This reading washes

\(^{38}\) As mentioned in the Introduction of this work, Laudan (1984) later introduced his reticulated model of justification in science, which established a tripartite hierarchy including theories, methodologies for testing those theories, and the scientific aims motivating both theories and methods. On this model, there is “a complex process of mutual adjustment and mutual justification” intertwining the different levels (62–63). There can be major changes in any of these levels over time, but there will almost always be a continuity of at least one, which allows the research tradition to maintain a continuous identity through time.

\(^{39}\) At the very least, each BSM model would have to accommodate (see Section 4.3.2 for the appropriate sense of “accommodate” used here) the existing experimental findings of SM experiments. Certain parts of the SM’s methodology also carry over, largely regarding experimental techniques. Since Laudan allows for methodological change within a single research tradition over time, and since the issue of which (if any) of them will succeed the SM is ongoing, a change in methodology doesn’t yet indicate a change in research tradition. My main point here is that Laudan’s criteria for the identity of a research tradition are not sufficient to distinguish the SM from all the BSM
out the dynamic competition between BSM alternatives, making it is difficult to see how to assess their progressiveness.\textsuperscript{40} These potential difficulties would jeopardise the application of Laudan’s methodology to modern particle physics without some modification.

The degree of specificity present in Lakatosian research programmes, however, works well in cases like the BSM models. Yet we have already seen arguments against the MSRP, particularly with regards to assessing research programmes and the lack of normative guidance given to scientists. So we should not adopt the MSRP wholesale either. Instead, I propose a third path, which combines both Lakatosian research programmes and assessment through scientific problem-solving. Of the criticisms of the MSRP raised above, all can either be eliminated by incorporating Laudan’s problem-solving account of rational scientific progress, or mitigated by observing that the criticism is less of a problem than Laudan supposed it to be (at least for the uses I will suggest for this hybrid framework). The fusion I suggest will be to replace the notion of Lakatosian assessment with Laudan’s problem-solving account.

Problem-solving efficacy is, by itself, not a notion that adheres strictly to one methodology or another. Laudan introduces his account of problem-solving in the context of explaining theories as mechanisms for solving problems: only later does he introduce its role in assessing research traditions. The idea that problem-solving is important in theory assessment is not entirely foreign to the MSRP. Lakatos (1978a) describes the positive heuristic, the part of a research programme that formulates new auxiliary hypotheses, models, and theories in order to overcome problematic empirical findings, as “powerful problem-solving machinery, which […] digests anomalies and even turns them into positive evidence” (4). And as we saw in Section 2.3, Lakatos accounts at this stage in their development.

\textsuperscript{40}Since these models go beyond the data we have presently collected at the LHC and other particle accelerators, we would need something like progressiveness to assess them, since we cannot rely on experimental results to distinguish them.
(1978b) alludes to Kuhnian ‘puzzles’ in describing “phenomen[a] which we regard as something to be explained in terms of [a] programme” (72), which sounds very much like Laudan’s definition of an empirical problem, the only type of problem that Lakatos considers in his assessments of progressiveness.

Since Lakatosian research programmes are also constituted by (series of) theories, we can shift emphasis to the investigation of how well those theories solve empirical and conceptual problems. Laudan defines problem-solving (empirical or conceptual) in a way that relies on a successor theory improving in specific ways over its predecessors, which can easily apply to Lakatosian series of theories. As a new theory within a research programme becomes operative, we can determine which problems of its predecessors it solves according to the positive heuristic and the way the protective belt adjusts. Since the problem-solving analysis occurs at the level of individual theories, it doesn’t matter much how the higher-level organisation of theories is structured so long as it allows the formation of new scientific structures using the same set of core elements.

We can assess the adequacy of research programmes the same way that we would research traditions, by looking at how effective they are at solving problems overall. The progressiveness of a programme can be similarly established, with its general progress found by comparing the first and latest theory in the series, and the rate of progress determined by looking at the theories within some temporal interval of the series. Once we know the progressiveness of a set of competing research programmes, we can determine which we should accept and which we are justified in pursuing.

An even closer alignment between research programmes and research traditions can be provided. Laudan binds theories together by a shared ontology and methodology. These features could be read as analogues to the hard core and the heuristics

\[\text{In fact, it may be possible to examine the way the positive heuristic anticipates new problems when investigating the problem-solving efficacy of a research programme, possibly offering us new insights into the process of scientific development.}\]
of a research programme, which describe the key elements each of the programme’s theories must share, including what the theory is about and how to undergo theorising in it. The heuristics assert how successive theories can and cannot be changed from their predecessors, maintaining the crucial parts of the ontology and methodology of a programme. Like research traditions, research programmes last longer than any individual theory and can undergo significant changes over time (though not as significant as the changes Laudan allows). The relevant difference is in the level of analysis: research traditions generally operate at a greater scale, encompassing large swaths of the phenomena studied by science, while research programmes can (though not necessarily must) operate on a smaller, more localised scale, with smaller lines of inquiry within a particular scientific domain.

Before turning to the new assessment of ST available through this hybrid framework, a few final points need to be addressed. Assessment through problem-solving requires that we be able to quantify the number and weights of problems that a programme solves, and be able to compare those values to those of its rivals. As Camilleri and Ritson (2015) point out, there is disagreement between string theorists and other physicists over what even count as the relevant problems and solutions. Many of the difficulties faced by ST are not seen as problems by rival theories of quantum gravity. For instance, proponents of loop quantum gravity do not see the high number of vacuum states in ST as a problem they need to solve, though they will certainly point out that the introduction of the string landscape seems like an ad hoc hypothesis. Even if a problem exists across multiple rival programmes, a particular solution will not necessarily be viewed as acceptable to those working outside the programme giving rise to it. As another example, ST could be seen to have “effectively ‘solved’ the renormalization problem that plagued earlier theories of quantum gravity” (50), but many physicists are liable to disagree with this reading, because they are not integrated into the scientific context of ST.
Laudan is clear that problems exist within a particular scientific context, the same one that defines the relevant research tradition (or better, research programme). Therefore, solutions are primarily meant to satisfy practitioners of a given research tradition, rather than outsiders who are less acquainted with the tradition’s nuances or less convinced of the importance of its problems in the first place. But this response is not entirely satisfying, since it effectively removes the objectivity that we introduced the problem-solving criteria to provide. Adopting the Laudanian view puts the assessment of research programmes back into the hands of scientists, who are now more explicitly divided into different scientific contexts, as well as historians after the fact. So the question arises whether the problems of a programme are to be diagnosed internally or externally to that programme. Should we evaluate the adequacy and progressiveness of each programme from the perspective of their practitioners, or from that of a relatively objective party, who doesn’t share the various background assumptions of the practitioners, but who can understand those assumptions and use them to evaluate the programme’s problems and solutions? Answering these questions about how to view a programme’s problems and solutions is critical for conducting non-empirical theory assessment, and further work needs to be done on this issue to ensure that the hybrid framework accords with actual scientific best practices.\(^\text{42}\)

Although a fully developed answer is impossible here, as a first approximation, I would say that this is where the philosopher of science can offer her advice, since she can plausibly be sufficiently divorced from (or critical of) the relevant scientific contexts to offer assessments of problems and solutions that are both timely and (relatively) objective. Thus, contrary to Johansson and Matsubara, on my hybrid framework philosophers do have a privileged stance from which to evaluate research programmes: that of an outside observer who doesn’t share the commitments and aims of scientific practitioners, and therefore lacks a stake in the

\(^{42}\)Dawid clearly leans towards appealing to internal perspectives on problems and solutions.
weighting of problems and solutions.

Finally, it is important to note that, in all likelihood, neither Lakatos nor Laudan would look favourably upon this hybrid framework. Lakatos would have rejected Laudan’s problem-solving approach, since it provides grounds for prospective, rather than retrospective assessments of rationality. Laudan, as we have seen, had many problems with the Lakatosian framework. Of the six criticisms listed in Section 3.4.1, my hybrid account immediately resolves C1, C3, C4, and C5, since these are concerned with Lakatos's assessment scheme, which I have more or less replaced with Laudan’s. Of the remaining criticisms, I have already explained why C6 is more of a virtue than a vice, at least in certain cases (within particle physics, for example).

Only C2 (that successor theories must entail their predecessors) remains to be directly addressed. However, as a consequence of accepting certain of Laudan’s notions for how theories can solve problems, one must discard the Lakatosian requirement that successive theories be additive. Laudan, after all, claims that problems can be solved by decreasing a theory’s scope compared to its predecessor, which is specifically forbidden in the MSRP. As long as the hard core remains represented in each theory represented in a research programme, abandoning the additivity of successive theories should not be an issue in the hybrid framework. There remains a conflict between non-additive successive theories and Lakatosian notions of scientific progress, but I am already suggesting such a radical overhaul of the MSRP that this concession is not so significant an adjustment, nor does it conflict with the rest of my use of the Lakatosian structure.\footnote{In order to properly establish model-groups within the MSRP, the project begun in Chapter 2, the additivity of the members of a research programme would have to be abandoned anyway. Here, I am simply making it explicit, while providing the additional motivation of replacing the Lakatosian concept of scientific rationality.}
We can now take the first steps towards a new assessment of ST using this hybrid approach. As with Johansson and Matsubara’s assessment, we will not see much progress as a result of ST solving empirical problems.\textsuperscript{44} However, because of the way Laudan describes such problems and their solutions (as the desire for an explanation that can be solved with a theory entailing a description of the phenomenon), ST could provide viable solutions to empirical problems just by satisfactorily explaining problematic phenomena, even without experimental confirmations. However, the introduction of the string landscape creates an issue for empirical problem-solving that is unrelated to our lack of experimental access to the Planck scale. With the loss of predictive power following the discovery of the large number of vacuum states of ST, properly setting requirements for empirical problem-solving is difficult. For instance, we cannot properly assess a ST solution with regard to the empirical problem of providing a physical reason for the values of various constants (though the introduction of the landscape itself serves as a conceptual solution that lessons the weight of this empirical problem). The difficulty caused by the high energy scale of ST experimentation just compounds this issue with empirical problem-solving, since there can be few adjustments to ST in response to empirical findings.

String physicists do present a case that their programme still solves many problems, including empirical ones. Johansson and Matsubara note that ST addresses three significant problems of theoretical physics:

P1 The unification of the four fundamental forces of gravity, electromagnetism, and

\textsuperscript{44}Even ST champions like Dawid sound pessimistic about its empirical prospects, even at lower (and therefore more accessible) energy ranges:

As of today, it is not possible to derive any quantitative predictions from basic principles of string physics. Therefore, even to the extent that string theory may predict low energy parameter values, string physicists today are not able to specify and calculate those predictions. (2013, 19)
the strong and weak nuclear forces through some sort of unification of GR and QFT.

P2 Explaining the large number of observed particles and constants in nature.

P3 Explain the values of the free parameter values of the SM, which must currently be put in by hand. (2011, 200–201)

ST offers a compelling solution to P1, since it does feature a spin-2 particle corresponding to the graviton (the quantized force carrier of gravity), in addition to the other force carriers and matter particles. However, with the introduction of Susskind’s (2003) string landscape, prospects for providing a satisfactory solution to P2 and P3 are dim unless one finds the corresponding anthropic argument particularly compelling.

Dawid (2013) discusses additional problems that ST proponents purport to solve, ranging from providing insights into black holes to establishing a strong final theory claim through duality relations between different kinds of STs. As a possible theory of everything, ST offers a resolution to an issue that is frequently seen as an important conceptual problem (the unification of physics) that other, empirically successful programmes cannot. Furthermore, there are various problems that extend beyond the domain of physics, and so we see ST solving problems within mathematics. ST’s ability to solve conceptual problems allows us to assess it on rational (and at least somewhat objective) grounds, even in the face of arguments against its empirical progressiveness.

There are examples of recent developments in ST not covered by Johansson and Matsubara. For instance, Antoniadis and Cotsakis (2017) survey ways developing

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45 Indeed, in 1990 Ed Witten became the first and only physicist to win a Fields Medal, the highest prize in mathematics, in part for his contributions in ST. Incidentally, Stöltzner (2002) has argued that the MSRP can be applied towards understanding mathematical progress, specifically in the interactions between mathematics and theoretical physics. However, modifying the MSRP in this fashion still leaves it vulnerable to the other criticisms raised above.
string cosmology can resolve two of the more pressing problems of the standard model of cosmology: the cosmological constant problem (that the observed value of the total effective vacuum energy is much smaller than QFT estimate for the vacuum energy, leading to a significant amount of fine-tuning of the cosmological constant) and the singularity problem (that spacetime singularities are both generic predictions of GR, but also points beyond which no further description of the evolution of spacetime is possible). Since ST offers a unification of all fields and interactions, it may be able to describe the singularity of the early universe and explain the discrepancy in the values of the vacuum energies described by GR and QFT. ST proponents have also argued that they can explain the apparent missing matter observed in astrophysical observations, commonly referred to as the dark matter problem. Chen and Takhistov (2019), for instance, construct a cosmological model from moduli (multiple scalar fields) and other “string-inspired” elements that “provides a consistent framework for generation of radiation, baryons, and dark matter” and offers an explanation for “the coincidence between the baryon number and dark matter abundance” (19).

From this rough sketch we can see that ST has plausible claims at having solves some of the problems string physicists find pressing (P1, for example), though it has had limited success solving other problems. There is ongoing work to address additional problems, as shown by the attempts to solve cosmological problems mentioned above. Certainly many string physicists take this degree of problem-solving success to be sufficient to continue their pursuit. My hybrid framework provides a philosophical rationale for why, particularly since its formulation as a quantum gravity theory in the 1970s and the superstring revolution of the 1980s, ST has been consistently seen as a theory worth pursuing. In the absence of any empirically successful alternatives operating in a universal domain, and with the many possible solutions it has offered
in a relatively short time period, ST has a significant rate of progress among QG theories. However, the failure of ST to retrodict the values of various observed constants through the analysis of the vacuum states has left several significant problems unresolved. Susskind’s string landscape has failed to curry much favour from those outside the string community, though it does seem to have changed the weights of P2 and P3 for string physicists and ST proponents like Dawid. ST is still a large field of inquiry within physics, but it has lost some of its prestige since the mid-2000s, when it began facing sharp criticism from inside physics (see, for instance Penrose, 2005; Smolin, 2006; Woit, 2006). Nevertheless, the normative assessment available to us from my hybrid scheme tells us that, at the very least, ST is sufficiently progressive to be considered pursuit-worthy, and this would be the case even with a similarly progressive rival programme. This verdict is an advancement over the assessment provided by Johansson and Matsubara, who couldn’t provide any normative guidance to physicists or funding agencies regarding ST within Lakatos’s framework. There is now a rational basis for the pursuit of string theory, beyond the lack of a more progressive rival.

On a more general note, cases like ST are the best place to apply the hybrid framework. ST (and the BSM model-groups mentioned in Chapter 2) exemplify situations in which a well-developed programme exists for extending science beyond its current empirical limits, but which is stymied by technological limits from gaining supportive experimental evidence. This kind of situation seems ideal for the application of Laudan’s problem-solving account. Without the recourse of empirical evidence to act as the ultimate criterion of assessment, some other rationale for pursuit is necessary. Tracking the ability of a programme to solve empirical and conceptual problems provides an objective criterion for assessing them, for comparing them to each other and

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successful rival would have a tremendous advantage over ST, even if it weren’t as well-developed in other respects.
establishing which is most worthy of pursuit. After all, at this stage of development, “non-empirical” theories like ST make progress mostly through the process of solving the various problems that emerge from internal developments and the external challenges arising in other areas of physics.

The methodologies of Lakatos and Laudan have been criticised by historians of science as being unrealistic, since they are based on (possibly cherry-picked) case studies, for being overly reliant on rational reconstructions instead of actual practice, and for ignoring many of the non-empirical factors that are used in scientific decision making, and in many cases this criticism is valid. But in cases where we need some sort of non-empirical assessment of scientific progress to help justify the pursuit of research programmes by scientists who want to explore the intellectual limits of their fields, the hybrid framework I propose here takes seriously the perspectives of the scientific practitioners involved. The alternative would be, for example, for physicists to abandon work on ST until such a time that we determine we can investigate it empirically, an alternative that many physicists (even ST opponents) would find unacceptable. Among other detriments, such an alternative eliminates the potential for the kind of unexpected breakthroughs that have so often advanced our scientific understanding.

3.5 Conclusion

I began this article by noting the problems ST presents for ordinary notions of scientific progress, with its long history of theoretical advancement that lacks significant experimental confirmation. Johansson and Matsubara use classic methodologies of science for the task of assessing ST. However, their preferred approach, Lakatos’s MSRP, is ultimately unsuitable for two reasons. First and most significantly, it can only be brought to bear on analysing historical cases of scientific progress, not on

\[47\] See the concluding chapter for further details on these criticisms.
the ongoing developments we see in ST. Second, the MSRP only offers normative
guidance to the historian of science in reconstructing concluded cases of research,
providing no such guidance for scientists in determining the pursuit-worthiness of a
particular programme. The way forward, I have argued, involves analysing ST (and
other theories that lack an avenue to experimental progress) using Lakatos’s notion
of the research programme, but assessing the progressiveness of these programmes on
the basis of problem-solving efficacy, a technique borrowed from Laudan. Although
it could be argued that this calls for the adoption of the Laudanian notion of the
research tradition as well, I recommend sticking with the more narrowly defined con-
cept of the Lakatosian research programme, which can use a higher level of specificity
when examining the kinds of conflicts that arise in some important modern cases of
scientific progress, since it is more naturally fine-grained than Laudan’s research tra-
ditions. Thus, we have a framework for assessing scientific progress that is more
objective than the MSRP (since, at least in principle, it only requires us to compare
the weighted counts of solved problems of rival programmes), while also providing a
more stable, granular structure than a research tradition (so that it better captures
ongoing developments in high energy physics). Analysed this way, it can be argued
that ST has shown a significant rate of progress towards solving some of the problems
physicists have identified, and is therefore a rational programme for them to pursue.

With a combination of the MSRP and problem-solving assessment, the problems of
Johansson and Matsubara’s Lakatosian assessment of ST disappear. Problem-solving
efficacy can be monitored even in ongoing research programmes like ST, allowing us
to analyse the appearance of new problems and solutions, as well as the relative
weighting physicists give them. We can therefore offer an assessment of ST without
having to wait for the historical distance necessary for a rational reconstruction of
the entire episode of its development. Because an assessment can be made, and
because Laudan specifically uses problem-solving to assess pursuit-worthiness, my
hybrid framework can also offer a degree of normative guidance to physicists with regard to ST, something not available through an assessment using the MSRP. Since there will be disagreements about what counts as a problem across different scientific contexts, we can explain the sorts of rational disagreement that, for instance, leads one scientist to pursue ST, while another decides her efforts are better spent exploring an alternative account of QG. Further, this framework provides an avenue for the philosopher of science to offer her services in acting as a third party in analysing scientific practice to determine pursuit-worthiness, an avenue Lakatos did not provide. Thus, with this hybridization of the work of Lakatos and Laudan, we gain a framework of scientific progress that is much better suited for the purpose of assessing ongoing research beyond the technological limits of experiments into fundamental physics.

Of course, ST is not the only case where theoretical developments extend beyond our ability to seek empirical confirmation. There are other QG theories (loop quantum gravity is a prominent example that has been mentioned before, but there is also, for instance, canonical quantum gravity, causal set theory, geometrodynamics, etc.). These alternatives should also be examined through my hybrid framework, to get a better understanding of the overall degree of problem-solving efficacy, and to determine which ones are successful enough to be considered pursuit-worthy. Other examples of ongoing research that could benefit from a non-empirical assessment include the BSM searches for alternatives to the SM mechanism for electroweak symmetry breaking, discussed in Chapter 2. These BSM models attempt to resolve some of the open problems of the SM, while also addressing their own conceptual and empirical problems (the chief being the discovery of a very SM-like 125 GeV Higgs boson in July 2012). Departing from fundamental physics, additional research programmes that might benefit from my hybrid framework can be found in the historical sciences, like astrophysics, biology, geology, palaeontology, and so on. These sciences involve the search for historical traces of various causal mechanisms, but in many cases classic
experimentation is impossible or impractical. Therefore, they can also benefit from the problem-solving assessment of my hybrid framework.48

My hybrid account, based as it is on evaluating the problems and solutions of research programmes, provides a greater degree of guidance to scientific decision-making than relying on rational reconstructions of historical cases. There will still be disputes between physicists concerning what counts as a problem or a solution, so there remains room for considered disagreements. Many physicists working on the problem of QG choose to pursue ST, but many do not. Using my hybrid framework, it is inappropriate to charge string physicists with irrationality for pursuing ST, since it continues to offer avenues for solving problems, but we can also see that alternatives may also be available for reasonable pursuit. Furthermore, my framework provides room for the philosopher of science to provide her services in analysing these disputes and offering advice and normative guidance from her position as an outside observer. Although empirical evidence should remain the most important factor in determining which theories it is appropriate to ultimately accept, in situations where we must go without any such evidence, analysing the problem-solving capacity of a research programme is the best way to understand many otherwise troublesome instances of scientific progress.

48See Chapter 5 for more details.
Chapter 4
Doubts for Dawid’s Non-Empirical Theory Assessment

4.1 Introduction

In the previous chapters, I provided the first steps in examining theories and model-groups at the edge of the experimental limits of particle physics through the lens of a hybrid framework of scientific progress composed of Lakatos’s methodology of scientific research programmes combined with Laudan’s problem-solving criterion of scientific rationality. In this chapter, I critically examine the most prominent methodology of non-empirical theory assessment in the contemporary literature. Since it offers arguments which use non-empirical evidence to significantly confirm theories without experimental data, this methodology of non-empirical theory assessment could be used in alternative accounts of scientific progress. By showing that it fails in its aim of providing significant confirmation to programmes like string theory (ST) (i.e. those that are highly developed around core physical ideas, but which lack near-future prospects for empirical support), I motivate the further development of my hybrid framework as the only account of scientific progress which is available for non-empirical science.


2In particular, it could be used to make the case for the truthlikeness or veridicality of these theories of non-empirical science which are necessary for the truthlikeness, epistemic, and noetic accounts of progress. See Section 1.2.1.
In *String Theory and the Scientific Method*, Richard Dawid provides an account of non-empirical theory assessment, meant to compliment traditional, empirical methods of assessing theories.\(^3\) He provides three interdependent arguments for the continued trust in scientific theories that lack empirical evidence, but which have theoretical virtues and no available alternatives. These arguments are meant to act as sources of non-empirical evidence for the viability of, for instance, the ST research programme. Non-empirical evidence does not pertain to the phenomena described by a theory, but comes from observations about the research process leading to a theory’s formation. As discussed in Chapter 3, empirical evidence has been, and is likely to remain for the foreseeable future, elusive because of the energy scales and the massive number of possible vacuum states involved in ST.\(^4\) Despite ST proponents’ apparent violations of long held standards of empirically testing scientific theories, Dawid argues that string physicists remain warranted in their perception that ST is a viable research programme that will eventually yield positive experimental results, because it has strong *non-empirical* evidence. In this chapter, I argue that Dawid does not establish this claim, by showing that his arguments for non-empirical theory assessment are flawed, concentrating on one argument in particular: the meta-inductive argument.

Dawid claims that ST obtains this non-empirical evidence from the combination of what he calls the *no alternatives argument* (NAA), the *unexpected explanatory coherence argument* (UEA), and the *meta-inductive argument* from the success of

\(^3\)Dawid has two aims for his account: first, he wants to establish a new paradigm of theory assessment in order to show that theories with no prospects for empirical testing, like ST, are worthy of scientific pursuit; and second, in the final part of his book he attempts to provide additional confirmation for ST via arguments for scientific realism and ST’s status as a final theory. Since my goal is to create an account of pursuit-worthiness for this class of (non-empirical) scientific projects, I will focus on Dawid’s arguments concerning his first aim. However, it is important to note that he has a larger project in mind, one which I will not comment upon here, except to the extent to which it intersects with my own project.

\(^4\)For example, the Large Hadron Collider (LHC) is capable of reaching energies of about 14 TeV (\(\sim 10^4\) GeV). The best estimate of the energy needed to probe matter at the string scale is the Planck energy, or \(\sim 10^{19}\) GeV. Likewise, the number of possible vacuum states in ST is roughly \(10^{500}\), each representing a different possible physical state of affairs.
other theories in its research programme (MIA). Dawid is not arguing against the notion that decisive empirical evidence should be the ultimate mark of a successful scientific theory, but rather that research programmes like ST still have a rational, scientific basis through non-empirical theory assessment. Because non-empirical evidence can’t be predicted by a theory itself, his supplemental paradigm of theory assessment is meant to explain cases where scientists continue to work on a theory even though empirical evidence is hard to come by.

Although I am sympathetic to Dawid’s project, I believe each of his arguments are problematic. I will focus on the MIA, because the scope of my objection is widest against it. I argue that the MIA does not increase the available non-empirical evidence in ST’s favour because the constraints Dawid claims it provides to the conceptual landscape of successor theories are already provided within scientific practice distinct from his line of argument. Only those theories that have the right sort of relationship to the predictions of their successful predecessors are likely to be successful themselves, and this relationship supersedes the MIA, leaving it idle. My argument is not specific to ST, which is Dawid’s focus, and so casts doubt on his account of theory assessment generally.

4.2 Non-Empirical Theory Assessment

Dawid’s three arguments are defined in reference to what he calls scientific underdetermination. It is a distinct type of underdetermination that he suggests is particularly useful to consider in theory building. Scientific underdetermination, as defined by Dawid, is underdetermination by the currently available evidence (as opposed to all possible evidence) given some general assumptions that are deemed valuable or

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5In fact, establishing theories like ST has having a rational basis for pursuit is only part of Dawid’s project. He later uses his arguments as the basis for an attempt at establishing a high degree of confirmation for ST. His turn towards confirmation, and the problems with that turn, will be discussed briefly in Section 4.3.
necessary within a scientifically viable research programme (rather than across all logically possible circumstances). These “ampliative rules of [the] scientific method” include things like a principle of induction, a disregard for ad-hoc explanations, some form of Ockham’s razor, and specific rules for scientific practice within a given field. Scientific underdetermination is similar to the “transient underdetermination” described by Sklar (1975, 1981) and Stanford (2006), though Dawid notes distinctions in his formulation that make it useful in his account of theory assessment.

Dawid claims that understanding how scientific underdetermination is constrained is especially useful to understand theory construction and assessment, because it allows scientists to follow the generally accepted rules of their field while focusing on presently available evidence in order to strip away alternative accounts of the same phenomena. He makes use of unconceived alternatives: rather than highlighting underdetermination between existing theories that account for a phenomenon, Dawid’s scientific underdetermination applies across all possible theories that explain the phenomenon, even those to which we don’t have epistemic access. Claims of scientific underdetermination in a given context indicate that it could be possible to construct alternative theories that follow the rules and fit the presently available evidence of a scientific field, if only we had the proper epistemic standpoint.

Dawid’s core argument is that we can assess the range of possible alternatives to

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6 Unconceived alternatives are covered extensively in (Stanford, 2006). The concept is not without its detractors (see, for example, Chakravartty, 2008; Devitt, 2011; Godfrey-Smith, 2008).

7 A significant difference between Dawid and Stanford’s accounts is that Dawid ultimately takes a realist position on ST, in contrast to Stanford’s anti-realism. In so doing, he argues that it is possible to assess scientific underdetermination without having knowledge of all alternative theories. Dawid makes these arguments in Part III of his book, which I cannot properly address in the present work. Whether or not Dawid is successful in arguing that he can make assessments over the complete range of possible theories of some domain, including those that remain unconceived, is irrelevant for the purposes of my arguments below, which I take to sufficiently undermine Dawid’s account of non-empirical theory assessment and also, therefore, his arguments in favour of scientific realism and final theory claims presented in the final chapters of (Dawid, 2013).
a given theory, so he proposes methods to constrain the number of possible theories that provide acceptable explanations for the (presently) non-testable phenomena at hand. Without such constraints, “no correct predictions of new phenomena could ever be expected to occur” (48), because we’d have an infinite number of theories that adequately describe the observed phenomena, with no way to distinguish them besides actively testing their predictions. Since testing the predictions of such a set of theories is effectively impossible because of constraints on time and resources, our experimental apparatuses are not be capable of all such tests at the moment, and we haven’t conceived of all possible alternatives. The three arguments that form the core of his account of non-empirical theory assessment, however, act as constraints on scientific underdetermination, shrinking the conceptual space of theories for a given phenomenon, and thereby making it more likely that a single theory is the one scientists should pursue.

4.2.1 The No Alternatives Argument

The NAA begins with a consideration of the conceptual landscape of possible theories when only one theory actually exists. There are two ways of interpreting the persistent lack of alternatives to a controversial solution to a scientific problem. The first is that there are theoretical avenues left to be discovered, but some contingent factor has barred our epistemic access to them. However, accepting this interpretation fails to provide scientists with any solutions, because the conceptual space under investigation remains the same size, offering too many possibilities and no hint how to find a more promising solution. The other interpretation is more optimistic and is the one Dawid prefers. It is to “conjecture a connection between the spectrum of theories scientists come up with and the spectrum of all possible scientific theories that fit the available data” (Dawid, 2013, 51). If scientists have problems finding
alternatives, it must be because there are few alternatives available to find. If a solution to the problem can be found at all, and only a small number of appropriate solutions can be constructed, we gain confidence in the solution already in hand, even without empirical support. In effect, the NAA raises the subjective degree of belief in a theory’s empirical adequacy.

Dawid concedes that the “step from an observation about the present human perspective to a conclusion regarding the overall spectrum of possible scientific thinking is by no means trivial” (51). The NAA may raise the subjective degree of belief for theories like ST, but it cannot do so without nagging doubts about unconceived alternatives. Further arguments are needed to establish the viability of the project of using constraints on scientific underdetermination for non-empirical theory assessment.

4.2.2 The Unexpected Explanatory Coherence Argument

The UEA constrains scientific underdetermination through examination of the structure of the theory itself as it is constructed. The argument comes into play when explanatory connections that were not purposefully searched for emerge during theory construction. As long as the theorist presupposes another brand of optimism (that there are empirically adequate scientific theories covering each phenomenon in a given domain and that there is at least one theory that covers all of them), a solu-

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8 Dawid’s apparent conflation of pragmatic considerations (that we should pursue theories for which we have no alternatives because the only other choice is abandoning any attempt to explain that research domain) and epistemic ones (his conjectured link between the available spectrum of theories with the actual spectrum fitting the data) can be understood as part of his larger project to confirm ST. However, as I explain in Section 4.3.1, it is unclear how this kind of argument works in general cases beyond ST, since Dawid’s arguments for a high degree of trust in ST don’t translate to other theories.

9 A slightly different approach to the NAA is available from Dawid, Hartmann, and Sprenger (2015). Using Bayesian epistemology, they argue that, within the conditions of scientific underdetermination, the NAA can raise the subjective probability of empirical adequacy for a hypothesis without any apparent alternatives, though this increase may be very small. It is in this paper that Dawid’s preference for the scientifically optimistic interpretation given above is defended on Bayesian grounds. For the purposes of my analysis, this formulation retains the same functionality in Dawid’s overall account. Naturally, anyone who objects to Bayesian confirmation (see, e.g., Mayo, 1996) will not find this defence of the NAA particularly convincing.
tition that emerges covering more than just the initial phenomenon in question looks more viable than rivals that do not. Scientists could construct a multitude of theories to account for any given phenomenon, but Dawid takes it that there is a smaller logical space for theories that account for diverse phenomena all at once.\footnote{It is not clear that the logical space for such theories actually is smaller than that of theories that explain a single phenomenon. One could argue that both logical spaces are in fact infinite, and therefore their sizes could not be differentiated except by cardinality, though Dawid et al. (2015) argue against an infinite number of alternatives under the constraints of scientific underdetermination. On the other hand, it is reasonable to question whether the logical space of possibilities even has a size.} That these connections in a theory were unexpected is relevant, says Dawid, by analogy with the distinction between novel data and data used in constructing a theory. Thus, unsought connections make the theory more attractive, both for being rarer, and for having more explanatory power than theories that cover only a single phenomenon.

These connections could be indicative of a more fundamental theory that underlies the one we are assessing however, so we cannot rely solely on the constraints to scientific underdetermination provided by the UEA. The argument can be used in conjunction with the NAA, however. A scientific optimist (or realist) would grant a theory with no actual, known alternatives a higher degree of belief. If that theory also covers more of the physical phenomena within its domain, our optimist would consider it even more likely to lead to empirical evidence, because a “true” theory would have no alternatives while also explaining phenomena it wasn’t originally designed to explain.

Yet, there could still be other explanations for these unexpected connections independent of a lack of alternatives. For example, there could be some theoretical interconnections underlying the diverse phenomena that have heretofore gone unnoticed, but are unrelated to the theory. So, it is necessary to “get a better grasp of the actual chances of empirical success of theories which show a strong pattern of unexpected explanatory success” (53), which is where Dawid’s final argument comes
4.2.3 The Meta-Inductive Argument

The MIA is an empirical argument, but one that employs its evidence for the strategy of non-empirical theory assessment, i.e. for the very notion of putting limitations on scientific underdetermination. If theories with certain features (in line with constraints on scientific underdetermination) tend to be empirically successful, then the MIA lends support to the strategy of limiting underdetermination. Any new theories that have no empirical evidence, but that benefit from the arguments that limit underdetermination, gain credence from the MIA. Thus, trust in currently unconfirmed theories can be enhanced by the empirical successes of established theories.

Dawid argues that the no alternatives and unexpected explanatory coherence arguments, which provide constraints on underdetermination, together provide a satisfactory constraint for predictive success of some theories. A scientist’s chances of developing a theory that both explains the available data and makes successful predictions for future observations seem miraculously slim, because the unconstrained conceptual space allows for countless theories that could explain the data while failing to make accurate predictions. Under the assumption of scientific optimism, the constraints of the NAA and UEA likely lead to empirically adequate theories (if they exist), so we have an explanation for predictive success: the fewer available theories, the greater the likelihood that scientists will select successful ones from what remains. Dawid dismisses other features, like simplicity or beauty, claiming they can’t explain the predictive success the way limiting underdetermination can.

Dawid still needs to explain how predictive success in existing theories justifies the assumption that sufficiently similar theories will ultimately be successful as well. He

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For Dawid, predictions help distinguish theories that otherwise have the same empirical content within the conceptual space: “If two theories make exactly the same predictions, then we consider them to be identical” (Dawid et al., 2015, 216).
has already linked the success of a theory to the NAA and UEA. If we regularly see predictive success in theories that went without alternatives and make unexpected theoretical connections, then currently unconfirmed theories that also have these features gain credibility as well. By using evidence of other theories’ success, Dawid makes the prediction that unconfirmed theories, like ST, eventually gain empirical support, not at the theoretical level, but at the “meta-level of the conceptualisation of predictive success” (36).

According to Dawid’s reading, ST has no alternatives and has made several unexpected connections throughout its development. These features put limits on the possibility space from which theories may emerge. In order to support ST with the MIA, all that’s left is to determine whether or not there is a sufficiently similar source of predictive success to count as meta-level evidence.

For this evidence, Dawid turns to the standard model of particle physics (SM). It was conceived in the 1960s and 70s as a way to solve problems with the available empirical data concerning nuclear interactions and eventually provided a host of new predictions. Some of these predictions went for some time without empirical confirmation. According to Dawid, the SM had no viable alternatives and made several unanticipated explanatory connections throughout much of its lifetime. In this sense, Dawid calls the SM the precursor to ST: the NAA and UEA apply to both. The SM has gone on to be extraordinarily successful experimentally, with the recent discovery of the Higgs boson being the most notable successful prediction (ATLAS Collaboration, 2012; CMS Collaboration, 2012).

Dawid’s account of the SM and its empirical successes leads him to construct the following meta-level hypothesis:

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12 For instance, the SM Higgs boson was first proposed in 1964 as an explanation for electroweak symmetry breaking (EWSB). A Higgs candidate was only detected in 2012 and confirmed as “a Higgs boson” in 2013 (see Chapter 2).

13 In addition, the SM is a low energy approximation of ST, in the same way that in certain reference frames Newtonian physics is an accurate approximation of relativity theory.
Scientific theories which are developed in the research programme of high energy physics in order to solve a substantial conceptual problem, which seem to be without conceptual alternative and which show a significant level of unexpected internal coherence tend to be empirically successful once they can be tested by experiment. This statement is argued for based on past empirical data (in our case, largely data from the standard model of particle physics and from some earlier instances of microphysics) and can be empirically tested by future data whenever any predictions which were extracted from theories in high energy physics along the lines defined above are up to empirical testing. (36)

So, according to Dawid, our epistemic assessment of ST is affected by empirical tests, even before any of its own predictions become testable. The NAA and UEA apply to ST, and it is a successor to the SM, so it gains empirical support through the MIA. Dawid’s argument is that ST should be considered a viable scientific theory because his account provides sufficient non-empirical evidence.

4.3 The Trouble with Non-Empirical Theory Assessment

As we have seen, Dawid has built a case for his account of theory assessment around three arguments he claims provide non-empirical evidence for theories that lack experimental support. If he is right, these three arguments, the no alternatives argument, the unexpected explanatory coherence argument, and the meta-inductive argument, all function in the framework of providing constraints upon scientific underdetermination, limiting the number of possible theories under consideration, so that only those with features salient to making successful predictions remain.

However, Dawid’s three arguments are not without their problems. I will focus my attention on the problems facing the MIA, because this is where I feel the most
interesting argument against Dawid lies. However, I would be remiss not to briefly mention some of the difficulties within his other arguments.

4.3.1 Problems for String Theory

Before raising my worries about Dawid’s non-empirical theory assessment, I’d like to emphasise some points to counter specifically some of Dawid’s defence of ST (for more on the basics of ST, see Section 3.2). First, the predictions of ST,\textsuperscript{14} are beyond the realm of testability for the foreseeable future. The Planck scale, which is where most of the unique properties of ST and other theories of quantum gravity are likely to be found, is far beyond the reach of even our most sophisticated tests.\textsuperscript{15}

Even when related consequences of ST are testable with currently obtainable energies, as is the case with some formulations of supersymmetry (SUSY), empirical evidence remains elusive. SUSY is an extension of the SM that posits a link between bosons and fermions through the broken symmetry between ordinary fermions and their superpartners with spins differing by a half integer. SUSY is a necessary ingredient in any version of ST that includes fermions, because it makes the theory consistent. Several versions of SUSY describe low energy superpartners that should be detectable with current particle accelerators. Before the Large Hadron Collider (LHC) became operational, calculations of some of these supersymmetric pairs had masses as low as 100–150 GeV (Buchmueller et al., 2009),\textsuperscript{16} which was within the range detectable by the initial run of the LHC. However, no superpartners have been

\textsuperscript{14}To the extent it makes predictions. Woit (2006) argues that ST does not actually make predictions, though string theorists I’ve spoken to reject this claim.

\textsuperscript{15}Kane (1997) argues that ST is currently testable, though this view is not widely shared and has not been born out over the last two decades.

\textsuperscript{16}Incidentally, they also determined that a Higgs mass greater than 120 GeV was detrimental to the constrained Minimal Supersymmetric SM (cMSSM), the version of SUSY that required the fewest modifications from the SM while still being consistent with particle physics phenomenology. The Higgs was discovered to have a mass of about 125 GeV, which can only be accounted for in cMSSM with modifications that many theorists believe are in tension with naturalness (see Draper et al., 2012).
discovered yet, which severely limits the parameter space of SUSY. Only by extending SUSY models with features that strike many physicists as “unnatural” can this particular avenue for testing ST continue (for more comprehensive look at the particulars of SUSY models and the tensions they have recently faced, see Section 2.3.4).

Continuously adding extensions after the fact to preserve a theory raises the spectre of unfalsifiability (Popper, 1959) or of becoming a degenerative research programme (Lakatos, 1978b). A charge of ST programme degeneracy is particularly strong in light of Lakatos’s claim that “the only relevant evidence is the evidence anticipated by a theory, and empiricalness (or scientific character) and theoretical progress are inseparably connected” (38).\(^{17}\) Dawid, though admirably attempting to describe the behaviour of physicists and provide an explanation for the predictive success of science, knowingly bumps against long established traditions of theory assessment and demarcation. ST has lasted more than four decades on the merits of its purported theoretical virtues alone, without even peripheral empirical evidence. Non-empirical theory assessment offers a novel way of avoiding the charge of “unfalsifiability” or “degenerative research programme”, but we should remain cautious in the face of the actual failures of ST to produce testable predictions. As I hope to show in the following arguments, we should also remain cautious of adopting Dawid’s solution to the problem.

The NAA, naturally, relies on acceptance that there are no alternative theories covering the relevant phenomena. Finding a workable combination of particle physics and relativity theory has been a problem in physics for decades, with ST seen as a

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\(^{17}\)Dawid does discuss a Lakatosian assessment of ST by Johansson and Matsubara (2011). They determine that a lack of novel empirical predictions in three decades indicates that ST is a degenerative research programme. Dawid counters that they didn’t pay attention to the perceptions of string theorists about their own programme, and therefore do not account for the unique features of ST (what Dawid calls its “meta-paradigmatic character”) that require a different account of progressiveness. I take a different approach to the Lakatosian framework, which is in agreement with neither Dawid nor Johansson and Matsubara, but further discussion is beyond the scope of this chapter (see Chapter 3 for more).
possible solution. It began from a simple foundation (that fundamental particles are extended, rather than point-like) and managed to create a credible scheme for unifying quantum and gravitational phenomena.\(^{18}\) Dawid claims that ST is the only currently available solution to the problem of quantum gravity, at least when comparing its scope to that of other quantum gravity research programmes.

This claim doesn’t match the perception of ST among many other physicists. Smolin devotes an entire chapter of *The Trouble with Physics* (2006, Ch. 15) to examining the myriad alternatives to ST under development.\(^{19}\) In his review of Dawid’s book, Smolin (2014) applies the three non-empirical arguments in favour of loop quantum gravity, the most popular iteration of canonical quantum gravity and the primary rival of ST for a theory of quantum gravity. Smolin’s point is that Dawid’s framework is overly, and perhaps fatally, broad if it can be applied to strengthen trust in another theory meant to account for some of the same phenomena. But the fact that Smolin can make a case for loop quantum gravity at all demonstrates some evidence for the possibility of alternatives. If there are in fact alternatives to ST, then the NAA fails to apply to ST.

For the MIA to work, Dawid’s case for ST also relies on the SM having no alternatives. Dawid claims that “none of the alternatives to the standard model that physicists could think of was satisfactory at a theoretical level” (35). This claim is surprising, as the field of alternatives (today called BSM physics for “beyond the SM”) has been around from the very beginning. SUSY, along with composite Higgs models and EWSB models introducing extra dimensions arose in the early 1970s, as

\(^{18}\) One of the consequences of the ST formalism is that the gravitational force carrier, the graviton, emerges naturally.

\(^{19}\) Dawid’s response to proposed alternatives is to claim that none of them have the universal scope of ST, and so are not true alternatives. Eventually, any less fundamental quantum gravity theories will either be subsumed by ST, or abandoned. Yet many physicists still consider some of these alternatives to be live avenues of research, though with many of the same problems with producing testable predictions as ST. The universality point may be moot, because a theory with universal applicability may not ultimately be worth pursuing (if it is achievable at all), though realists, including Dawid, are unlikely to agree with me on this point.
contemporaries of the SM. The same is true for ST itself. Dawid is wrong when he claims that “[t]he Higgs mechanism constituted the only known method of producing the observed mass of elementary particles in a gauge theoretical framework” (37), because alternative models explaining electroweak symmetry breaking, many of which have nothing like the SM Higgs, were common in the years leading up to the Higgs discovery. Even today, after physicists at the LHC have announced that they have found a Higgs boson, it is not clear that it is the Higgs boson described by the SM. The phase space still permits alternative models (though admittedly, it has shrunk as a consequence of continued testing) and it may yet be determined that the particle discovered in 2012 is a BSM Higgs (see Section 2.3 for more on the field of BSM alternatives of the EWSB mechanism and their relation to the evidence of a Higgs boson discovered in 2012). What would that conclusion show us about the lack of alternatives to the SM?

It is non-trivial to claim that the SM has had no alternatives. Dawid does not show that the history of the SM matches his claims. The strategy of non-empirical theory assessment may still be viable, but it is not helpful in providing warrant for ST research, at least not using the SM as inductive evidence. Still, the primary target of my argument is against the general strategy of Dawid’s non-empirical theory assessment, not its applicability to a particular theory. So, though I feel this criticism alone is enough to sink Dawid’s case for ST’s viability, I will move on with my arguments.

The UEA is essentially a non-empirical version of the no-miracles argument in the vein of Putnam (1975). From this perspective, the UEA claims that it would be miraculous if unexpected connections would appear without ST achieving empirical adequacy. Because this is just a variation of Putnam’s classic realist argument, Dawid must contend with challenges to the no-miracles argument if he wants to convince sympathetic anti-realists (see, e.g., van Fraassen, 1980, for a classic example of a
counterargument to no-miracles).

However, Dawid contends that the UEA is immune to most, if not all the standard attacks against no-miracles, at least in ST’s case. He argues that the underlying concept of ST is so simple that it would be impossible to reduce it to component parts. Its core is too simple to hide any fundamental pieces that would explain the unexpected theoretical connections.

A more fundamental theory could still provide an explanation for ST’s apparent theoretical connections. Dawid argues that this possibility is unlikely because of ST’s strong claim to being a final theory: it is a theory that “does not have any possible rivals that provide more concise or universal predictions of empirical data” (130). If ST is a final theory, then there cannot be a more fundamental theory that explains the unexpected explanatory connections, and so the UEA would apply.\footnote{Conceivably, there could be another theory as fundamental as ST. Dawid rejects this possibility by shifting the scope from local underdetermination to global. His account of global underdetermination is beyond the scope of this chapter, however, so I will not discuss this move further, though it is certainly a point anti-realists should probe further.}

According to Dawid, string theorists have two reasons for believing they have a final theory. First, ST is the only theory purporting to provide a description of all physical phenomena at the fundamental level. To string theorists, all other potential theories of quantum gravity lack this universal scope, and so aren’t true competitors and can’t be final theories.

Second, ST has duality relationships that suggest that, beyond a certain scale (one described by ST), any attempt at a more fundamental description of reality yields exactly the same physics as higher scales. For instance, duality provides theoretical reasons for believing in a lower bound for conceptually accessible distance scales. If the descriptions at a particular scale and a higher energy scale provide the same physics, it implies that “[d]uality […] translates all information below the string length into information above the string length, rendering the former fully redundant”
Similar dualities exist for other phenomena described by ST. Dawid uses dualities to argue that a theory with these relationships would be final, because we could never formulate a more fundamental theory that didn’t immediately become ST.

The obvious problem with this line of reasoning is that claims of finality of a theory can be only as secure as the theories that are making them. The viability of ST is the very issue in dispute. For ST to have a credible claim of being a final theory, there must be strong reasons to believe that it is a viable theory in the first place. The UEA is meant to provide such reasoning, yet it relies on there being no theory more fundamental than ST in order to avoid no-miracles counterarguments. Using the scientific merit of ST in order to bolster arguments for it’s scientific merit seems like begging the question.

Finally, Dawid focuses his discussion of the UEA on ST, whose proponents can at least point to fundamentality as a defence against counter-arguments to no-miracles arguments. But this defence doesn’t apply to general applications of the UEA, limiting its use beyond ST. Either the UEA applies, at best, only to theories that can claim to be final (and then, only with assurances that this move is not question begging), or else it needs to come packaged with arguments against standard objections to no-miracles arguments. In either case, Dawid’s account of it is insufficient.

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21 Dawid suggests other applications for non-empirical theory assessment beyond ST in (2013, Ch. 5) and palaeontology and anthropology are mentioned at the beginning of (Dawid et al., 2015).

22 Dawid has since made a defence of at least a certain form of the no-miracles argument in (Dawid and Hartmann, 2018). They defend the no-miracles argument from the charge that, when reconstructed in Bayesian terms, it is logically invalid because it commits the base-rate fallacy, as argued by Howson (2000) and Magnus and Callender (2003). Dawid and Hartmann argue that Howson only reconstructed a subset of the “frequency-based” no-miracles arguments, and only this subset is vulnerable to the base-rate fallacy. However, they acknowledge that there is ongoing debate against other no-miracles counterarguments from, for instance, van Fraassen (1980); Laudan (1981a); Fine (1984).
4.3.2 Against the Meta-Inductive Argument

Dawid uses his arguments as an explanation for the predictive success of theories, because the MIA ties the narrowing of conceptual space for theory building to theories that are already predictively successful. The observations that a theory has no alternatives, makes unexpected theoretical connections, and that theories that have these properties tend to be successful is explained by limiting underdetermination, in analogy to the way empirical data are explained by hypotheses. Dawid argues that non-empirical theory assessment explains belief in predictive success by an inference to the best explanation (IBE) on the meta-level of theory construction (see Section 3.4 of Dawid, 2013). The MIA is what transfers that IBE from extant theories to theories still lacking empirical evidence.

My argument against the MIA stems from the fact that it operates only on successor theories. I mean by ‘successor theory’ a theory that expands the scope of some existing theory, or offers a more fundamental account, yet accommodates all (or most) of the empirical content and explanatory domain of the existing theory. A successor theory acts somewhat like a member of the series of theories that make up Lakatosian research programmes, except it could potentially exist outside of a given programme (to an extent it could ignore the programme’s negative heuristic). For example, consider the general theory of relativity. It expands the scope of Newtonian gravitation (by unifying our concepts of space and time and by equating accelerations due to gravity with other uniform accelerations), provides a fundamental explanation of the phenomena of interest (rather than a mysterious attraction between masses, gravity is now explained via the curvature of spacetime), and accommodates the observed phenomena, (including observations like the anomalous precession of Mer-

\[\text{23By "accommodates" I do not necessarily mean that the successor theory accepts all the empirical content and the entirety of the explanatory domain of the theory it is meant to succeed. Rather, it is a combination of incorporating parts of the extant theory and explaining why some parts are not incorporated. This reading of "accommodates" will be important in what follows.}\]
cury, which did not align with Newton’s account). General relativity is a successor theory to Newtonian gravitation, though it doesn’t fit into the Newtonian research programme, since it doesn’t share that programme’s hard core.  

Similarly, ST is a successor theory of the SM. ST provides a more fundamental account than the SM, yet it retains SM physics, and consequently the empirical findings of SM experiments, as a lower energy limit. Theoretical features and empirical content from the SM are vitally incorporated into all versions of ST. This description goes beyond Dawid’s discussion of possible alternatives in particle physics. There, the only restriction on alternatives provided by the available data is that they have “the known theory as their low energy effective theory...at that theory’s energy scale” (63). Beyond that, Dawid says that “string theorists view their own endeavor as a natural continuation of the successful particle physics research program” because of the “entirely theoretical motives for its creation, the lack of satisfactory alternatives and the emergence of unexpected explanatory interactions” (35–36). My classification of ST as a successor theory is not incompatible with Dawid’s characterisation, though it goes further than his.

As a successor theory to the SM, any version of ST worth even cursory consideration has to accommodate the results of experiments done within the SM research programme. Any version of ST that does not suitably align with the empirical evidence of the SM would be rejected out of hand, just as general relativity wouldn’t have been declared viable had it not accommodated evidence generated by Newtonian physics. A regard for currently available evidence is already built into our assessment of all potential successor theories by Dawid’s scientific underdetermination. The crucial bit of my argument is that scientists must also accommodate the predictions of

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24Similarly, this notion of successor theory doesn’t quite align with Laudan’s notion of a research tradition, since, aside from not necessarily sharing in a research tradition’s ontology and methodology, a successor theory may also fail to share the necessary intellectual continuity to belong in a research tradition covering a specific domain of phenomena.
As I hope to show, accommodating predictions will leave the MIA idle.

To begin, a real world example may be helpful. Consider the prediction of the Higgs boson by the SM. Its prediction wasn’t made in a vacuum: there were well established theoretical reasons to think that something like the Higgs boson must exist. The Higgs boson is the quantum excitation of the Higgs field, a scalar field that explains EWSB, the extremely short range of the weak nuclear force, and the masses of various SM particles. This theoretical reasoning crucially tied into the formation of the SM, with the earliest papers describing a mechanism for EWSB (Englert and Brout, 1964; Guralnik, Hagen, and Kibble, 1964; Higgs, 1964a,b) used by some of the founding documents of the SM (Salam, 1968; Weinberg, 1967). Because of the existing theoretical reasons behind the Higgs prediction, a failure to find the Higgs boson would have shaken the foundation upon which the rest of the SM rests.

A failure to confirm the Higgs boson prediction would have negatively affected the non-empirical theory assessment of ST, as Dawid acknowledges, because that would have weakened the case for the MIA. I claim, however, that the confirmation of the Higgs boson didn’t actually help ST under the MIA. Assuming a somewhat monolithic view of the SM and Higgs field, as Dawid does, the prediction of the Higgs was so trusted and grounded in the rest of the SM that it was necessary for ST to also accommodate it. ST would not have been seen as a reasonable successor to the SM otherwise. Without accommodating the Higgs boson (and other SM predictions along the way), something critical would be missing in the formulation of ST. In that

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25 My account of successor theories aligns somewhat with views put forward by Laudan (1977, 1981b). As we saw in Chapter 3, Laudan’s sketched arguments for scientific progress as problem solving fit nicely with what I have said here, as his account of progressiveness includes solving both empirical and conceptual problems. He also argued that scientific progress does not have to be explanatorily or conceptually cumulative, a view that is compatible with my use of the term “accommodation” (see Laudan, 1976).

26 For recent technical looks at the background of the Higgs boson, see (Ellis et al., 2016; Ellis, 2017; Karaca, 2013; Wells, 2016). For a less formal discussion of the Higgs boson, see (Carroll, 2012).
case, we could rightly ask why this putative successor theory is lacking such important theoretical components that link it to its own lower-energy approximation. And this question is appropriate to ask even before the discovery of a Higgs boson through experiment. Because the SM is an approximation of ST, we would expect that ST must have had some way to explain EWSB, even prior to the Higgs boson’s discovery in 2012. The empirical confirmation of the Higgs boson must already have been an expectation built into ST.

Why is it important that the predictions of the SM were incorporated into the construction of ST? Because as a successor theory, ST relies on those predictions to pass the first round of constraints on the theoretical landscape described by scientific underdetermination. Without the predictions of the SM being somehow accommodated, ST wouldn’t be seen as viable: some theoretical consideration critical to our current understanding of physics would be missing from it without adequate explanation. In order to be seen as a successor theory, ST (and every other contender for BSM physics) must accommodate the predictions of our current theories within the same domain, just as it incorporates the available experimental data. In the Higgs boson example, the prediction is already accommodated by ST, and so it passed this initial hurdle placed on SM successor theories. Experimentally testing a prediction creates another restriction to the theory space, depending on the results of the test. Those successor theories that incorporated a successful prediction remain, while those that rejected the prediction are eliminated. The reverse occurs if the prediction fails.\textsuperscript{27} ST has already passed two different theory selection criteria before the MIA is even on the horizon: the accommodation of its predecessor’s predictions and the empirical

\textsuperscript{27}Of course, we must allow for adequate, rigorous testing before calling a prediction successful or not. The exact properties of the Higgs boson are still being explored, in hopes that some SM anomaly will be discovered. But because the theory space that Dawid describes holds every possible successor theory, including unconceived alternatives, I do not need to account for the sorts of adjustments made to the parameters of a theory to align it with experimental results. All possible iterations of a theory are under the same constraints.
tests of those predictions.

The specific circumstances of these accommodations provide criteria for the initial cut of available theories in constraining scientific underdetermination. Dawid argues by IBE that arguments that appeal to non-empirical criteria explain predictive success. But in cases where a successor theory accepts the prediction under test, I think there is another possible explanation. For a successor theory that already accommodates the empirical content (and often other parts of the theoretical framework) of a successful theory, it doesn’t seem mysterious that it could go on to become predictively successful itself by also incorporating the predictions of its predecessors. After all, the predecessor already has features that have led to successful empirical predictions, and these features are likely incorporated into many of the successors that share its empirical content. The machinery of parameter adjustments and phenomenological modelling that exists within theory construction helps produce predictions. Accepting the predecessor’s predictions just means a successor theory is including even more of the empirical content of a successful theory. For instance, the SM adopted much of the empirical content from the quantum and electromagnetic theories that preceded it, including some predictions that had not yet been tested. The SM went on to be extraordinarily successful, partly because it was not disconfirmed by betting on predictions from its predecessors that eventually failed. So, the mere fact that a successful theory makes successful predictions provides an explanation for why its successor theories that incorporate those predictions have a higher likelihood of empirical validation themselves.

It is important to note that this explanation operates on the same meta-level of theory construction that Dawid establishes for his arguments. Dawid is seeking an explanation for the success of theories and does so by examining what factors are relevant for predictive success. He concludes that his method of applying constraints to scientific underdetermination is the best explanation. However, my suggestion offers
another possible explanation for success: the empirical success of a predecessor hints at theoretical virtues that may be incorporated by its successors. Because it isn’t dependent on a particular prediction’s success, or its particular theoretical virtues, this explanation acts on the meta-level. My suggestion focuses on a theory having a certain relationship with its predecessors’ predictions, a non-empirical feature different from those specified by Dawid. Theories that accept and incorporate the predictions of their successful predecessors are more likely to survive underdetermination cutting.\(^{28}\)

So far, I’ve left out cases where a successor theory rejects its predecessor’s prediction by offering some alternative to it, or otherwise implicitly denying it. Many successor theories adopt the predictions of their predecessors to some extent,\(^ {29}\) but because rejection isn’t logically excluded and we are dealing with all possible successors in the conceptual space, we must consider such rejections. There are two possibilities at this point: either the rejected prediction is confirmed or it is not. If it is confirmed, the successor theory is eliminated from consideration for failing to align with experiment. If the prediction fails, then obviously that failure cannot be used by the MIA to bolster the successor theory. Because the MIA and my prediction-focused constraints use the same evidence and occur at the same level of theory assessment,

\(^{28}\)This sort of “natural selection” account of theories is not new. Van Fraassen (1980) introduced a version in his account of constructive empiricism: “For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which in fact latched on to actual regularities in nature” (40).

An evolutionary account of theory progression also has the benefit of being neutral in the realism/anti-realism debate, at least so far as the truth of theories is concerned. As we have seen, Dawid is committed to scientific realism, and his arguments (particularly the UEA) reflect that. However, my argument for the predictive success of theories and their successors, though borrowing from a famous anti-realist source, does not make a commitment either way. As a response to Dawid’s use of no-miracles arguments, I offer another rational explanation for the predictive success of a theory that is neither Dawid’s proposal nor miraculous. Because, ideally, the viability of theories like ST should be independent of one’s position on this debate, my account offers an attractive avenue for both camps. I would like to thank one of my anonymous reviewers for this helpful insight.

\(^{29}\)Of the alternatives to the SM Higgs boson, almost all of them accepted the SM prediction that there would be a particle produced by the mechanism of EWSB, for instance, though they differed on the properties of that particle.
there’s nothing for the meta-inductive argument to add in these cases.

One possible objection to my account is to suggest that the MIA could act on theories that do not have this successor relationship. Take a theory of palaeontology with no alternatives but with unexpected explanatory connections receiving non-empirical support from the success of the SM, for instance. A situation like this would not run afoul of my argument concerning successor theories. However, there are two problems with this move. First, Dawid himself quite carefully restricts his arguments to operate in one field of research with a single explanatory domain at a time. His meta-level hypothesis, quoted in Section 4.2.3 above, clearly restricts the source of meta-inductive support for ST to the research programme of high energy physics. The very strategy of putting limits on scientific underdetermination presupposes the use of the available data and the ampliative rules within a single field of study.

The second counter-objection is an extension of the first: for an inductive inference by analogy, like the MIA, to work, it is important that the source of inductive evidence is sufficiently similar to its conclusion. The SM, as a theory of high energy physics, is quite similar in many respects to ST (which is what enables ST to be one of its successor theories). It is not very similar to any theory of palaeontology (it lacks a shared set of data and ampliative rules, as well as methodologies and scope). Successor theories are similar enough, by their very nature, to be used in an inductive argument like the MIA. Other theories are not.\footnote{Another possible objection to Dawid’s MIA is to charge it with double-counting: the same empirical evidence from the SM is used to calibrate the MIA and to confirm ST. However, Steele and Werndl (2013) argue that double-counting should not automatically be considered a problem. I would like to thank one of the reviewers of this paper for pointing out this reference and thus improving my argumentation.}

4.4 Conclusion

If the meta-inductive argument doesn’t provide string theory with the additional evidence that Dawid claims it does, then non-empirical theory assessment is left in
a poor state. ST is in no better position than any other SM successor theory when it comes to the constraints placed on scientific underdetermination. Each of Dawid’s arguments cannot provide sufficient support for a theory by itself. The MIA is the linchpin of his account of non-empirical theory assessment: without it, ST fails to gain the confidence Dawid endeavours to provide to it.

The situation also looks grim for other theories one might want to support through non-empirical theory assessment. The scientific machinery of theory construction is much more complicated than Dawid credits. The MIA isn’t capable of providing empirical evidence for any theory for the exact reason it can’t help ST: any credence it might provide for successor theories is already accounted for by features of theory construction in the existing research programme. Any successor theory must have some relationship with the empirical predictions of its predecessors in order to survive the constraints on underdetermination. Depending on the specifics of that relationship, either there is a better explanation for predictive success, or there is a result that the MIA cannot use. In either case, the MIA remains idle.

Besides the structural problems of the MIA, I have also provided reasons to doubt Dawid’s NAA and UEA, at least in their application to ST. So, the case for the non-empirical theory assessment of ST looks grim, despite Dawid’s forceful arguments. This is not to say that I think the ST research programme should be abandoned, nor do I claim that string theorists are bad scientific actors. I admire Dawid’s attempt to explain the continuing work on ST, even though I believe the particulars fail. His work stands as an important call to understand scientific practices in situations where our ability to theorise vastly outruns our ability to experiment. But, though his account of scientific underdetermination hits on a sorely underrepresented facet of theory construction and choice, and though the NAA and UEA have interesting features, Dawid’s account ultimately doesn’t provide a positive assessment for ST, non-empirical or no. And therefore, his arguments cannot form the confirmatory basis
needed for other accounts of scientific progress, leaving my hybrid problem-solving account the only one capable of assessing ST (see Chapter 3).
I have articulated the need for a new understanding of scientific progress and non-empirical theory and model assessment. In order to create a new framework for understanding how to assess theories and models that lack prospects for empirical confirmation, I have borrowed ideas from the methodologist tradition of the philosophy of science, primarily Lakatos and Laudan. I showed that Lakatos’s (1978b) Methodology of Scientific Research Programmes (MSRP) accurately describes the dynamic situation of the model landscape of the electroweak symmetry breaking sector (EWSB). Several lines of models act as alternatives to the Higgs mechanism, the standard model (ST) account of EWSB. Predictive or explanatory models are formulated using a handful of core ideas that went beyond the SM (BSM), and each of these models is distinct from each other. When the discovery of a scalar boson, which looked very much like the SM expectations for the boson resulting from the SM Higgs mechanism, was announced in July of 2012, each of these groups of models faced significant unfavourable evidence. The Higgs discovery discounted large segments of the BSM parameter space that were either easy to test at the Large Hadron Collider (LHC), or were natural (and thus resolved the hierarchy problem). Describing these BSM model-groups as research programmes helps account for the way they persist as more and more data points towards the conclusion that physicists have discovered a SM Higgs boson. The central narrative framework of each group can be viewed as the hard core of a programme, which remains intact even in the face of empirical evidence that has been largely unfavourable, while the individual models taking
specific values of free parameters and making specific predictions can be tested and disconfirmed without affecting the core idea that went into their construction. However, Lakatos conceived of research programmes as composed of series of theories. By borrowing modern ideas of models in science, primarily the view that models can be autonomous from theory and target systems from Morgan and Morrison (1999), and the role developmental models play in theory construction from Hartmann (1995), I introduced the notion of a model-group, a new kind of research programme where the role of the series of theories is replaced by a cluster of models.

This isn’t the only change that adapting the MSRP for non-empirical theory assessment required. Lakatos emphasised the role of empirically confirming the predictions of a research programme in order to assess it as progressive or degenerative. Lakatos also left such assessments to after the fact, limiting the ability for his notion of rationality to be action guiding, beyond the imperative to follow the trail provided by “elite” scientists. In cases where the prospects of empirical confirmation are low and sharp disagreements exist about how to proceed, Lakatos’s account of rationality is of limited use. Laudan (1977) provides an alternative, problem-solving rationality, which I argued could be incorporated into Lakatosian research programmes without too much trouble. Problem-solving allows us to determine which programme in a specific domain we have good reasons to pursue, and which we have grounds for accepting. We can, in principle, add up the number of solutions that a programme has produced, weighted according to several pragmatic criteria, in order to determine which programmes are the most successful, and which programmes have had the greatest recent fruitfulness. I used the recent work of Johansson and Matsubara (2011) to highlight the flaws of Lakatos’s mechanism for assessing scientific progress, and to take the first steps towards showing how the subject of Johansson and Matsubara’s case study, string theory (ST), fared under such a problem-solving assessment.
Finally, I showed how another framework of non-empirical assessment, proposed by Dawid (2013), has flaws that undermine its applicability. Dawid makes three arguments establishing constraints on scientific underdetermination: the no alternatives argument (NAA), the unexpected explanatory coherence argument (UEA) and the meta-inductive argument from the success of other theories (MIA). Dawid uses these arguments to ground the continued pursuit of ST, after some 40 years without experimental confirmation and with little prospect for a change in that situation anytime soon. Although I take issue with Dawid’s application of the NAA and UEA to ST (there is an argument to be made that ST does in fact have alternatives, and the UEA is effectively a no miracles argument, which is open to standard counterarguments unless it is applied to a “final” theory like ST, at which point its application becomes question begging), the MIA is generally flawed. The MIA provides inductive evidence that theories that make unexpected explanatory connections and lack alternatives will eventually receive empirical confirmation, since earlier theories within the same domain with those features have since been empirically successful. However, I argued that the empirical success of those earlier theories was already accommodated in an important sense by the theories we are now assessing non-empirically. Because of this, the MIA stands idle, unable to import the evidence Dawid claims it does. Without the MIA, Dawid’s account of non-empirical theory assessment cannot provide significant confirmation to any theories, and thus cannot support other accounts of scientific progress which assess truthlikeness or veridicality. Therefore, in cases of non-empirical science, my account of progress can be applied and others cannot.

To summarise, I have suggested modifications to the Lakatosian notion of the research programme, creating a new hybrid framework for describing scientific progress in scenarios where we lack prospects for empirical guidance, and I have also shown

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1Recourse to the experimental confirmation of a theory’s predictions is a fundamental aspect of the classic notions of theory assessment that Dawid is attempting to supplement with his non-empirical approach.
that another prominent attempt is not successful at this task. However, there is still much work to be done.

First and foremost, the notion of problem-solving needs to be fleshed out. Laudan provides some preliminary descriptions of how to assess the conceptual problems that arise in a research tradition, how to determine their relative weights and importance, how to determine what counts as a solution, and so on. He remains somewhat vague and general, however, which is to be expected, since he is attempting to describe a framework that will account for progress across the entirety of science. I would like to reiterate that my hybrid framework is meant to provide rational underpinnings for instances of non-empirical theory and model developments, and also to provide some degree of normative guidance for those scientists working in such conditions. I fully expect some other account(s) of scientific change to be more relevant under different circumstances that are more favourable to the empirical verification characteristic of so much of science.

There are reasons that the methodologists lost so much of their direct influence in the 1980s and 90s, such that Fuller (1991) asks whether they are “withering on the vine.” New avenues for thinking about science emerged, including the sociology of science and the rise of Integrated History and Philosophy of Science (HPS) and the History of Philosophy of Science (HOPOS). These new approaches to understanding scientific progress rightfully criticised methodologists like Lakatos and Laudan for being overly reliant on case studies (which can be cherry picked to support predetermined conclusions (see, e.g., Hesse, 1980)), for utilising rational reconstructions (which often grossly mischaracterised the actual reasoning behind historical instances of scientific change (see, e.g., Arabatzis, 2017)), and for ignoring many of the non-epistemic factors that go into scientific decision making. Some philosophers, such as Longino (1990, 2002) criticised the methodologists for failing to see the importance of the epistemic values held by communities of scientists, and the interactions of
different perspectives and values within those communities. Sociologists of science, though often disagreeing with each other, argued that the methodologists “failed to take socio-political context into account and thus were still too much wedded to the old, abstract, acausal ideals of rationality, objectivity, and progress toward truth” (Nickles, 2017). By restricting my account to a specific domain of applicability, one that is currently developing, I hope to avoid the bulk of these criticisms.

Still, there is a need to further clarify what a problem is, how significant it is, and how to determine when a solution has been found. Since my framework is operating within a more restricted scope, it should be (in principle) easier to draw these answers directly from actual scientific practice, since there is a smaller domain of theories, models, and practitioners to examine. Even Laudan acknowledged that the relative weight of a problem is determined internally to a particular research tradition, and so an important part of this examination will require a greater understanding of how values and perspectives operate within scientific communities. Here, the works of feminist philosophers of science like Longino (1990, 2002) and Solomon (2001) will be helpful, since much of their work focuses on the epistemic norms of scientific communities. Examining the role of non-epistemic values in scientific goals (e.g., Elliott and McKaughan, 2014) and the role of values in the context of discovery and theory appraisal (e.g., Elliott and McKaughan, 2009) will also be helpful for understanding how the perspectives and values of scientists influence the way problems and prospective solutions are perceived. And, of course, a general understanding of the literature on values will be vital for this task. Beyond what has just been mentioned, a general survey of the literature on values and the social dimensions of scientific work would contain, for example, (Anderson, 2004; Douglas, 2009, 2013; Giere, 1991; Hacking, 2013).

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2 Determining the weight of problems is automatically more complicated under Laudan’s account than under Lakatos’s or Kuhn’s, since one must understand how best to compare empirical problems with conceptual ones. Laudan claims that conceptual problems are as important as empirical problems, but some empirical problems are more important than others, so his statement here can be understood only in a general fashion.
Another area of future research for this project is to extend the scope to other areas of science, a direction that may seem in conflict with my assertion that the limited scope of my framework helps avoid some of the charges levied against the methodologists arguments I'm adapting. However, I propose extending the application of my hybrid framework only to those scientific domains that would most benefit from an account of non-empirical scientific progress, namely the historical sciences. These are sciences engaged in historical research and that focus “on explaining existing natural phenomena in terms of long past causes” that cannot be tested under controlled experimental conditions (Cleland, 2002, 475). There are a variety of scientific fields that engage in this kind of non-experimental research: “palaeontology and archaeology provide the familiar examples,” but “historical hypotheses are also common in geology, biology, planetary science, astronomy, and astrophysics” (Cleland, 2002, 475) and even in economics (Simon, 1998). Cleland argues that there is a different mode of scientific reasoning being employed in historical sciences, in which scientists “look for present-day traces of [past events], and search for a smoking gun that unambiguously sets apart one hypothesis as the best among the currently available explanations for the traces thus far observed” (487). Currie (2014) argues that both simple and complex strategies for narrative explanations are employed in the historical sciences, and which strategy is used depends both on pragmatic considerations and also on the nature of the target to be explained.

Both Cleland and Currie argue that progress can be fruitfully made in the historical sciences, a conclusion I agree with wholeheartedly. I believe that a case-by-case examination of the historical sciences can reveal which field contains research pro-

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3 Turner (2004, 2007) argues further, that some of the features of historical science have implications for scientific realism.
grammes amenable to my hybrid framework. The key exemplar of such a case would be one in which there is a significant narrative component behind theories or models that makes hypotheses for which there are none of the traces Cleland discusses, and for which the prospects of finding such traces are slim. In situations like these, Laudan’s emphasis on non-empirical problem-solving (and to a lesser extent, on a kind of empirical problem-solving that isn’t directly reliant on confirming new predictions) can help us assess which research programmes it is rational to pursue and accept. Similarly to the cases I discuss earlier, however, the ultimate rational adjudication must involve empirical evidence, if and when we acquire it.

Finally, it would be advisable to take a closer look into the conditions under which models or theories are abandoned under the non-empirical circumstances I highlight. Lakatos allowed for scientists to continue pursuing research programmes long after they became degenerative, so long as the scientists felt the status may yet be reversed. Laudan provided more objective criteria for when it is no longer rational to accept or pursue a research tradition, though he doesn’t mark a definite threshold of problem-solving success below which it is no longer advisable to continue pursuing an unsuccessful tradition. I will use the resources available in modern particle physics (the large preprint archive used by physicists, the conference presentations that provide yearly summaries of scientific advancements, and the publications of groups like the Particle Data Group) to investigate what physicists actually believe about the circumstances required to say that it is no longer appropriate to pursue empirically unsuccessful models, if any. A good case study would be to look at the shifting importance of the naturalness criteria, which has been so central to many BSM models. As noted in the Introduction, the focus on naturalness has diminished somewhat in light of many of the best candidates for natural models being experimentally excluded following the Higgs discovery. Once the reasoning has been teased out of the physics resources, I will integrate it into my hybrid framework of theory assessment.
To conclude, I have argued that there is a valuable notion of scientific progress to be found by hybridising aspects of the philosophical frameworks of the methodologists of science. This hybrid account is useful for rationally assessing cases of scientific change and progress in which we lack prospects for the experimental confirmation of novel hypotheses, an otherwise defining feature of the scientific enterprise. Although there is much work to be done, I trust my framework will prove fruitful in describing the frontiers of physical research, as well as in offering some degree of normative guidance for scientists as they explore the far edges of our understanding of nature.


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APPENDIX A

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