

PHILOSOPHY AND PHYSICS: A RECONCILIATION

JESSICA ODDAN

Bachelor of Arts, University of Lethbridge, 2015

A Thesis

Submitted to the School of Graduate Studies
of the University of Lethbridge
in Partial Fulfilment of the
Requirements for the Degree

MASTER OF ARTS

Department of Philosophy
University of Lethbridge
LETHBRIDGE, ALBERTA, CANADA

© Jessica Oddan, 2017

PHILOSOPHY AND PHYSICS: A RECONCILIATION

JESSICA ODDAN

Date of Defence: June 15, 2017

Dr. K. Peacock Supervisor	Professor	Ph.D.
------------------------------	-----------	-------

Dr. B. Brown Thesis Examination Committee Member	Professor	Ph.D.
---	-----------	-------

Dr. S. Das Thesis Examination Committee Member	Professor	Ph.D.
---	-----------	-------

Dr. M. Stingl Chair, Thesis Examination Committee	Professor	Ph.D.
--	-----------	-------

ABSTRACT

Historically, philosophy and physics have been highly integrated disciplines. However, after the Quantum Revolution, physics distanced itself from the philosophical method, leading to a dramatic change in methodology and philosophy of science. As scientific style has shifted and philosophy fallen out of favour, the distancing of philosophy and physics has made its way into the public arena, with highly regarded physicists denigrating the discipline of philosophy and philosophers arguing for the value of their discipline. This thesis discusses different conceptions in philosophy of science, the role of scientism in the public discussion of the integration of philosophy and physics, and how string theory provides a unique and fitting example of how science can be affected by unrecognized changes in underlying philosophies of science. I determine that as evidenced by my discussion of string theory, philosophical intervention is necessary for solving some of the most fundamental problems in physics and that collaboration between the two disciplines must continue to increase.

ACKNOWLEDGEMENTS

First and foremost, I would like to thank my supervisor, Dr. Kent Peacock, for his continued support. Thank you for our endless conversations, for continuing to inspire me, and for pushing me to always do my best. I truly could not thank you enough. I would also like to thank Dr. Bryson Brown and Dr. Saurya Das for serving on my committee and for your wonderful feedback. Thank you to Dr. Michael Stingl for serving as the Chair for my defence. Thank you to my family and friends, particularly my mother, who has always encouraged me. And finally I extend my thanks to the University of Lethbridge, to the School of Graduate Studies, and to the Government of Alberta for providing funding and support throughout this endeavour.

Contents

1	Introduction	1
2	Philosophy of Science: Karl Popper	4
3	Philosophy of Science: Thomas Kuhn	12
3.1	Phase change in science	13
3.2	Tradition and innovation	16
3.3	Paradigm use	18
3.4	Incommensurability thesis	19
3.5	Objections	22
3.6	Conclusions	30
4	Scientism	37
4.1	The honourific use of “science” and its cognates	38
4.2	Inappropriately adopting the trappings of science	40
4.3	Preoccupation with “the problem of demarcation”	41
4.4	The quest for the “scientific method”	44
4.5	Looking to the sciences for answers to questions beyond their scope	46
4.6	Denigrating the Non-scientific	48
4.7	Deference, Respect, and Anti-Science	49
5	History of String Theory	51
5.1	The First Revolution	56
5.2	The Second Revolution	61
5.3	A Theory of Anything	65
6	Philosophy of Science: String Theory	72
6.1	The Current Status of String Theory	73
6.2	Canonical Understanding of Scientific Process	76
6.3	Paradigm shift	78
6.4	Three Contextual Arguments	80
6.5	Scientific underdetermination	83
6.6	Summary	86
7	Concluding Remarks	91

Chapter 1

Introduction

It has often been said, and certainly not without justification, that the man of science is a poor philosopher. Why then should it not be the right thing for the physicist to let the philosopher do the philosophizing? Such might indeed be the right thing at a time when the physicist believes he has at his disposal a rigid system of fundamental concepts and fundamental laws which are so well established that waves of doubt can not reach them; but it can not be right at a time when the very foundations of physics itself have become problematic as they are now. At a time like the present, when experience forces us to seek a newer and more solid foundation, the physicist cannot simply surrender to the philosopher the critical contemplation of the theoretical foundations; for, he himself knows best, and feels more surely where the shoe pinches. In looking for a new foundation, he must try to make clear in his own mind just how far the concepts which he uses are justified, and are necessities. (Einstein) [16]

The above quotation comes from one of the most profound thinkers of the 20th century. Albert Einstein approached questions in physics with philosophy in mind, as did many pioneers of quantum theory such Heisenberg and Niels Bohr. These people who revolutionized quantum theory, physics, and knowledge as a whole practiced physics with a different ‘style’ than is predominant today. Of late, it appears as though physics and philosophy are at odds. Many of the world’s most celebrated physicists (such as Stephen Hawking, Richard Feynman, and Steven Weinberg) have publicly denounced philosophy as useless, fickle, and obsolete. The quotation below is a poignant example of what I argue is a widespread contemporary attitude toward the integration of physics and philosophy:

How can we understand the world in which we find ourselves? How does the universe behave? What is the nature of reality? Traditionally these are questions for philosophy, but **philosophy is dead**. Philosophy has not kept up with modern developments in science, particularly physics. Scientists have become the bearers of the torch of discovery in our quest for knowledge. Stephen Hawking and Leonard Mlodinow [My emphasis] [67]

Lawrence Krauss, upon receiving criticism of his book *A Universe from Nothing* from philosopher and physicist David Albert, called Albert a “moronic philosopher” [2]. Steven Weinberg, a theoretical physicist and Nobel laureate wrote that he finds philosophy “murky and inconsequential” [24]. These examples are an indication of the gradual distancing between two robust disciplines, physics and philosophy. But physics and philosophy have always been closely intertwined, physics being born out of philosophy. So the disintegration of the connection between these two disciplines is surely a matter of interest, the disintegration being, I argue, a dangerous occurrence for the advancement of knowledge. The weakening link between physics and philosophy is the antithesis of what I am arguing, that a continued and open dialogue and collaboration between physicists and philosophers is necessary for the betterment of both academic fields and knowledge as a whole. I shall begin with Karl Popper’s falsifiability criterion as it is important for my discussion of the status of string theory. I shall then discuss Thomas Kuhn’s philosophy of science which I shall use for the notion of phase change. The above quotations by Hawking, Feynman, etc., are examples of scientism, a notion discussed by Susan Haack. I shall identify scientistic attitudes as an obstacle to my overall thesis. I shall then go through the historical development of string theory as I have emphasized that keeping science in a historical context is important for overall understanding. A discussion of Richard Dawid’s *String Theory and the Scientific Method* will follow where I shall argue that the current disagreements regarding the status of string theory stem from a fundamentally philosophical disagreement. Finally, I shall conclude that in order to properly discuss the status of

string theory there must be an increased integration of physics and philosophy. I use string theory as a real-world example of how physics can go awry when philosophy and physics are not integrated. This integration of the philosophical method into physics is what I shall consider a necessary paradigm shift in physics.

Chapter 2

Philosophy of Science: Karl Popper

The principle of falsification in science was described by Karl Popper. Popper's work in the philosophy of science focused on the demarcation between science and pseudo-science and the search for "a criterion for the scientific character of status of a theory" [54, p. 3]. Popper believed the demarcation between science and pseudo-science to be the foremost problem in the philosophy of science. He was moved to work on the problem of demarcation because he observed a tendency for scientific inquiry to fail, and for pseudo-science to nonetheless be perceived as being successful. According to Popper, the general consensus within philosophy of science is that the demarcation between science and pseudo-science occurs in virtue of science's empirical method that is based upon observation and experimentation [54, p. 4]. This empirical method relies on the process of induction, a philosophical notion with a complicated history.

A general definition of induction is the process of predicting future events on the basis of previous experiences. However, this definition is limited; induction is difficult to define because its use is so broad. But induction is essential to scientific reasoning. The problem of induction was discussed by Hume in the *Treatise of*

Human Nature, where he argued that inductive inferences are unjustified [65]. However, unlike many philosophers who have aimed to circumvent or solve the problem of induction, Popper accepted “the validity of the Humean critique of induction, and indeed, goes beyond it in arguing that induction is never actually used in science” [63].

But Popper’s acceptance of the problem of induction as an obstacle did not lead him to the particular brand of Humean skepticism shared by Hume—Popper maintains that observation in science is “selective and theory-laden—there are no pure or theory-free observations” [63]. Popper shares this notion with Thomas Kuhn, who similarly asserted the theory-ladenness of observations in science [8]. The belief that observations are necessarily theory-laden undermines the notion that science is differentiated from non-science by its inductive methodology. If observations are theory-laden, then scientific observations do not hold a special epistemic status as being objective reports of phenomena. The notion of theory-ladenness taken to its logical conclusion leads Popper to the determination that there is in fact no methodology that is unique to science, and as such science “consists largely of problem-solving” [63].

As Popper abandons the use of induction in scientific reasoning, he replaces it with his notion of falsifiability. To elucidate this new conception, Popper contrasts the overwhelming success of Einstein’s discovery of the theory of relativity with the ‘contrived success’ of psycho-analysis and individual psychology. It is neither the *truth* of the conception nor the “exactness or measurability” [54, p. 5] that struck Popper as odd. Rather, his inclination was that there was a difference between Einstein’s discovery and the latter examples, a significant difference that would show that both psycho-analysis and individual psychology had the appearance of sciences but were in fact pseudo-sciences [54, p. 5]. Pseudo-sciences, according to Popper, had many of the features of a successful science: they contained significant

explanatory power, they appeared to be true, and verifications of the theory were manifest upon “revelation” [54, p. 5]. It may however be more modest to say that what Popper called ‘significant explanatory power’ is better described as ideas that may have explanations that are more easily accepted. As such, instead of Popper claiming that ‘pseudo-sciences’ have a significant ability to explain phenomena, it might better be said that ‘pseudo-sciences’ have explanations that can easily be accepted. This difficulty with the role of ‘explanatory power’ resulted in Popper rejecting it as a virtue of a scientific theory [63]. Popper uses the term ‘revelation’ to poke fun at what he saw as a sort of religious indoctrination. The study of pseudo-science “seemed to have the effect of an intellectual conversion of revelation, opening your eyes to a new truth hidden from those not yet initiated” [54, p. 5]. After this ‘revelation’, one would see confirming instances everywhere. And Popper noticed in particular that pseudo-science could in most if not all instances be confirmed by a myriad of dissimilar cases, “which in the eyes of their admirers constituted the strongest argument in favour of these theories” [54, p. 6]. That is to say, pseudo-science does not have the property of being falsifiable as it can accommodate nearly any new fact or observation. Let us consider Einstein’s theory of gravitation which asserts that light must be attracted to heavy bodies which would cause a shift of the light in the direction of the heavy body. This concept could be tested as stars close to the sun “would look as if they had moved a little away from the sun, and from one another” [54, p. 6]. Einstein’s prediction of the amount of shift was tested by Eddington during an eclipse and found to be correct. Popper argued that Einstein’s theory had the property of being falsifiable, unlike Freud’s psycho-analysis and Adler’s individual psychology. Popper claimed that both psycho-analysis and individual psychology are untestable and thus irrefutable [54, p. 8]. However, it is a matter of debate whether or not Popper’s use of Freud’s psycho-analysis as an example of a pseudo-science was an accurate depiction.

Philosopher Adolf Grünbaum in *The Foundations of Psychoanalysis* argued that in fact Freud's psycho-analysis does have the ability to make some testable predictions, concluding that indeed the theory had been falsified [19]. Popper still maintained that the "clinical observations which analysts naively believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice" [54, p. 8], and thus neither theory can be classified as 'science.'

Thus, Popper offered a list of properties that a scientific theory should have in order to be falsifiable. To begin, Popper concluded that the only *confirmations* in science that should be credible are those confirmations that are the result of risky predictions [54, p. 7]. Only those confirmations that were expected to be incompatible with the theory and that could conceivably be false could count as supporting a theory [63]. A 'good' scientific theory must be one that is almost exclusively a theory of prohibition, i.e. "The more a theory forbids, the better it is" [54, p. 7]. It is logically impossible to confirm a universal proposition ('All swans are white') via experience. However, a single-counter example to a universal claim falsifies the universality ('There exists a swan that is all black') [8]. This is partly how Popper's notion of falsification works. Most important is the assertion that a theory that cannot be refuted by any conceivable event is necessarily non-scientific. This assertion is central to Popper's overall requirement of falsifiability, as a theory must be able to be shown wrong in order to count as being scientific. Another assertion is that "testability is falsifiability" [54, p. 7] and thus 'genuine' tests of a theory must be attempts to falsify it. Evidence that confirms a theory should only count as a result of the aforementioned genuine testing. Finally, Popper warns against theories that are upheld by admirers, despite the theory having been found to be false. This has the effect of lowering the theory's "scientific status" [54, p. 7]. Popper is also careful to distinguish between the logic of falsifiability and its applied methodology [8]. Logically, one must find only a single instance in which a case does

not fit the theory, and that theory is therefore falsified. As such, theories in science under a Popperian model are conclusively falsifiable but not conclusively verifiable [8]. However, this simplicity is lost when it is applied to methodology. It may be the case that ‘falsified’ results have come from an error, and thus we must question whether or not the claim of falsification has been confirmed. From this we must conclude that Popper’s model in practice is far more complicated than the model in theory. Nonetheless, Popper concludes that the problem of demarcation is solved by the criterion of falsifiability.

Popper and his notion of falsification is arguably one of the most popularized philosophical concepts in science, alongside Kuhn and his ‘paradigm shift.’ But as with many theories put forth in the philosophy of science, Popper entertained many objections to his proposal and his ideas on falsification inevitably evolved [63]. Imre Lakatos, a student of Popper’s, took issue with Popper’s theory of falsifiability. Lakatos accused Popper’s theory of being an oversimplification, one that could not be utilized in science [41]. Lakatos noted that in particular, Popper’s theory of demarcation is heavily reliant on ‘critical tests’ that have the ability to either falsify a theory or “give it a strong measure of corroboration” [63]. These critical tests bear a resemblance to Kuhn’s ‘exemplars,’ the important, novel, puzzle-solutions that help define a disciplinary matrix (an idea I shall discuss in the next section). For Popper, critical tests play an integral role in the determination of falsification for theories. However, Lakatos flatly denies the existence of such tests in science [63]. Lakatos believes that the disjunction between falsification and corroboration is “far too logically neat” [63]. Non-corroboration, according to Lakatos, does not necessarily lead us to falsification [63]. Lakatos also asserted that the falsification of a theory came about rarely, and not often due to isolated observations, as theories tend to be “highly resistant to falsification” [63].

Perhaps the most damning objection to Popper’s falsification criterion is its

reliance on convention. Basic statements are empirical claims that can be used both to determine whether a theory is falsifiable and to corroborate falsifying hypotheses. They take the form of a single (single entity, ‘a person y ’), existential (‘there exists item x ’) claim that can be confirmed through observation by an appropriately located observer (‘person y should be able to see star x at so-and-so a time and date’). Basic statements for Popper are not mere objective reports of phenomena. Rather, they are descriptions of phenomena set against a theoretical background that (often subconsciously) influence the descriptions—they are theory laden. As such, according to Popper basic statements are “open-ended hypotheses” [63] that cannot be confirmed via experience. Herein lies the difficulty. In order for a theory to be testable, we need to be able to determine whether or not basic statements are in fact true or false. For a theory to be falsifiable, it must be incompatible with an observation statement. In order for a theory to be falsified, it must be incompatible with some possible accepted basic statement. However, basic statements are verified via experience. But Popper has already asserted that experience is not by itself a legitimate basis so the question then becomes how we are able to accept basic statements with the reliance on experience playing a much smaller role. To this Popper replies that “basic statements are not justifiable by our immediate experiences, but are accepted by an act, a free decision” [53, p. 109]. This ‘acceptance by an act’ is only a veiled form of conventionalism [63]. A unique feature of conventionalism is that there are alternative conventions that could be accepted as good alternatives. As such, what we choose is “undetermined by the nature of things, by general rational considerations” [56]. This leads us to the conclusion that our choice of which particular convention we give uptake to is arbitrary and hardly a solid foundation for knowledge claims. So Popper’s conception implies that basic statements are accepted on convention within the scientific community and as such the ability of a basic statement to act as a falsifier

in a given theory is an arbitrary matter. Taken to its logical conclusion, we can determine that the falsification of an entire theory depends on an arbitrary, free act [63]. If the falsification of theories is partially an act of convention, then the scientific community's determination that a theory has been falsified need not (necessarily) reflect reality. This is contra to a realist conception that science actually 'reaches out and touches reality,' and as such I would venture to say would not be palatable to much of the scientific (and philosophical) community. Clearly, this objection leads to an unsatisfactory view of science.

And in light of these objections to his theory, Popper softened his ideas with several caveats. He conceded that demarcation between science and non-science is not done via falsifiability of scientific statements *by themselves*. Rather, scientific theories must be viewed holistically, much like Wilfrid Sellars' notion that science is an enterprise "which can put any claim in jeopardy, though not all at once" [58, SPR: p.170]. As such, scientific theories must be taken alongside auxiliary hypotheses if they are to be able to make predictions and be prohibitive [63]. So Marxist scientific socialism, which previously qualified as being unscientific (as it only became falsifiable with the addition of *ad hoc* hypotheses to match the facts), later became scientific. Popper concluded that Marxism indeed was falsifiable because "it was falsified by the events of the Russian revolution" [20, p. 11]. He also changed his mind on the theory of evolution, which at first he determined was a purely metaphysical (and thereby nonscientific) enterprise [20, p. 11]. The lack of internal consistency with Popper's seemingly simple criterion of demarcation became an important problem. Haack asserts that "Popper's criterion of demarcation proved so attractive to so many in part because it was amorphous—or rather, polymorphous—enough to seem to serve a whole variety of agendas" [20, p. 84]. But the apparent lack of a single form means that Popper's theory is not consistently and easily defined, and as such not easily used. Most important, the

polymorphous nature of Popper's criterion precludes its reliability as a logically neat philosophy of science.

But although Popper's notion of falsifiability is indeed a problematic one, it is falsifiability that is often used by scientists as an ill-defined philosophy of science [72]. The inability for a theory to be falsified is one of the main objections raised regarding the status of string theory. But more often than not, when pressed on the philosophical baggage that comes along with adhering to the notion of falsifiability, scientists lack the necessary information to discuss whether or not falsifiability is a good philosophy of science to adopt. This is ultimately due to a 'lack of philosophical awareness [and] belief that the "rules of science" are clear, god-given and do not need discussion' [42]. So scientists often adopt bastardized notions from philosophy of science without recognizing the (potentially large) limitations that may stem from these notions. And as I shall show in a later section, the adoption of poorly defined or incorrect interpretations of philosophy of science can lead to serious consequences for physics (and philosophy) as a whole.

Chapter 3

Philosophy of Science: Thomas Kuhn

Thomas Kuhn, one of the most influential philosophers of science of the 20th century, is noted for his integration of the history of science with his philosophy of science. Kuhn's work was a pointed movement away from the positivist tradition and into a more fluid account of the nature of scientific revolution. In his book *The Structure of Scientific Revolutions* [30], Kuhn posited that science is an enterprise that fluctuates between quiet phases of puzzle-solving and tumultuous phases of revolution. Although I argue that Kuhn's project in its entirety is an oversimplification, I aim to show that we can use pieces of his theory to improve the discipline of physics and science in general. I shall begin with a discussion of Kuhn's main ideas including his 'phase' account of change in science, his ideas surrounding tradition and innovation in science, his postulation of 'paradigm use', and his incommensurability thesis. I will then consider some objections to Kuhn's ideas and offer some insights of my own. Finally, I shall pull from Kuhn's work several ideas that can be of use in modern physics and philosophy, and tie these notions into my overall thesis that a continued and open dialogue between physicists and philosophers is necessary for the betterment of both academic fields. Philosophers of

science have worked extensively on optimal methodology in science, how to treat competing theories, and on the foundations of science. Kuhn made a novel move in his analysis of how science changes over long periods of time.

Kuhn is arguing against what he sees as the general nature of change in science. The general view, according to Kuhn, is that science progresses piece by piece, by considering new evidence set against the backdrop of the prevailing theory, and either accepting or disregarding that piece of evidence based on its compatibility with the theory. When new pieces of evidence are accepted, they are added to the framework to describe a more complete scientific landscape. As such, our science reaches farther toward a complete and true scientific picture. Implicit in this conception is the notion that “progress itself is guaranteed by the scientific method” [7], and that change in science is a smooth, linear progression, moving closer to objective truths. The above notion of scientific change is however, highly simplistic, and Kuhn may be criticized along these lines. Scientific change does not necessarily follow such simple lines; for example, it is not always the case that new evidence is discarded if it is not compatible with the prevailing theory. Rather, we may revise or replace our theory to accommodate the new piece of evidence (particularly if the evidence is empirically well-established). So Kuhn may have chosen an oversimplified version of change in science to raise objections to.

But Kuhn proposed something quite different to this (simplified) version of scientific change. Instead of a clean linear progression, Kuhn, who was by training a theoretical physicist [7], articulated an alternative account to the status quo. He proposed that there are alternating ‘revolutionary’ and ‘normal’ phases.

3.1 Phase change in science

‘Normal’ phases are described as “puzzle-solving” [30, p. 36]. During a normal phase, the scientist works within a familiar framework, slowly solving problems

using accepted methods. This is a slow, careful process. Basic scientific principles are not questioned, and work is done ('puzzles' are solved) within the framework in order obtain as much internal consistency as possible. Puzzle-solving in normal science follows rules of method determined by the reigning paradigm and there exists a 'range' of possible solutions to the puzzle. This 'range' of acceptable solutions should be consistent with the base assumptions of the reigning paradigm. Puzzle-solving can involve both the prediction of new phenomena and the explanation of given phenomena, but it is important during periods of normal science that both predictions and explanations fall into the paradigmatic understanding.

The progress of science during normal periods is motivated by loyalty to a 'paradigm' [7]. Among the relevant scientific community, there must be a consensus on and commitment to "shared theoretical beliefs, values, instruments and techniques, and even metaphysics" [7]. The paradigm or disciplinary matrix is composed of an array of shared beliefs, values, and techniques, disciplinary matrix being best described as the professional practice (accepted methods, ideas about theory assessment, etc.) of the scientific community [30, p. 182]. According to Kuhn, loyalty to the disciplinary matrix is achieved through the indoctrination of scientists during their scientific training [7]. Since loyalty to basic scientific principles has been established, relevant work inside the accepted framework can be done, and "scientists neither test nor seek to confirm the guiding theories of their disciplinary matrix" [7]. Just as during normal periods the scientist is unlikely to question the tenets of their disciplinary matrix, Kuhn posits that she is also unlikely to grant great bearing to results that do not fit within her framework. Instances of anomaly are ignored or rationalized away. 'Revolutionary' phases, according to Kuhn, are marked by heightened periods of progress. The reforming and revision of basic scientific principles is the mark of a revolutionary phase. Kuhn postulated

that during these periods, a phenomenon known as ‘Kuhn-loss’ may occur. A period of revolution may see old scientific principles left by the wayside as new principles are sought after [7]. As such, events in science that were previously accounted for in the old theory (developed and articulated during a normal period) may in the new theory have no explanation. Some of the details in our scientific landscape may be abandoned in the movement toward a different paradigm.

However, Kuhn requires that even if there is some ‘Kuhn-loss’, the replacement framework must retain most of its *quantitative*, not qualitative, problem-solving power. The loss of qualitative problem-solving power means that a new scientific framework may lose some of its explanatory power and may leave previously resolved questions *unresolved* [29, p. 20]. Kuhn uses phlogiston (a proposed element that was contained in all combustible material and ‘given off’ by combustion) as an example of ‘Kuhn-loss’ [30, p. 107]. Phlogiston served as a kind of elementary principle in chemistry that was primarily a qualitative theory where, for example, substances were identified based on their properties (i.e. qualities) [66, p. 74]. Phlogiston was later abandoned in lieu of Lavoisier’s quantitative theory that excluded phlogiston and rather integrated oxygen as serving the primary role in combustion, Lavoisier’s theory being quantitative as it focused on quantitative measures of matter (utilizing the principle of conservation) [66, p. 74]. However, the accuracy of this example as representing ‘Kuhn-loss’ is thought by some to fail [66, p. 74]. It is thought to fail because we can consider both the qualitative nature of phlogiston and the quantitative nature of Lavoisier’s chemistry not as incommensurable images, but as commensurable images. Lavoisier chemistry can be considered “a continuation of the chemical qualitative investigations, formed in the framework of the phlogiston theory, with more accurate and sensitive physical means” [66, p. 77]. Further and most important, it may be the case that there are in fact no concrete examples of ‘Kuhn-loss’ and as such this supposed ‘phenomenon’

may not be an accurate description.

3.2 Tradition and innovation

Ongoing during a normal period is the tension between the desire for innovative ideas and the commitment to adhering to the agreed-upon tenets in one's scientific community. Kuhn describes this in his work *The Essential Tension* [31] wherein he contrasts divergent and convergent thinking. Divergent thinking is the scientist's ability to entertain new ideas and inquire into new directions, while often, though not necessarily, rejecting old solutions [31, p. 226]. This mode of thinking is commonplace during periods of revolution. Convergent thinking is prevalent during periods of normal science, and describes the adherence of the scientist to traditional methods, beliefs, and solutions [31, p. 226]. This mode of thinking is endemic during periods of puzzle-solving. The essential tension that Kuhn refers to is between these two modes of thinking, and a successful balance of this tension is one of "the prime requisites for the very best sort of scientific research" [31, p. 226], and thus a successful scientist must "simultaneously display the characteristics of the traditionalist and of the iconoclast" [31, p. 227]. Normal science rewards convergent thinking, as convergent thinking is necessary for normal science to solve puzzles within the paradigm [31, p. 229]. During periods of normal science, since it is the prevailing background state (versus revolutionary science), scientists receive "rigorous training in convergent thought" which is "intrinsic to the sciences almost from their origin" [31, p. 228]. But not only does normal science reward convergent thinking, it tends to heavily discourage divergent thinking [31, p. 228]. We need only to look toward instances in science when people have proposed scientific notions contra to mainstream notions (for example, initial proposals of string theory [59, p. 104], heliocentric models of the solar system [59, p. 120]) to see that ideas that challenge the reigning notion of science are often met with fierce protest.

Thus, periods of revolution are reached only after periods of great tension, when the presence of particularly troublesome anomalies becomes too obvious to ignore or explain away (conditions that disrupt the practice of normal science). A widespread loss of commitment to the scientific framework is what Kuhn calls a ‘crisis’ [30, p. 66], and a revision to the disciplinary matrix will likely occur thereafter. Kuhn stipulates that influences outside of science, such as a scientist’s nationality, may determine the outcome of a revolutionary phase [30, p. 153]. This is not a particularly strong assertion, as he does not provide further argumentation for his statement. However, this comment likely spurred on the development of the Sociology of Scientific Knowledge [5], which asserts that sociopolitical factors are the primary influence on the trajectory of science. Kuhn showed “limited sympathy for such developments” [7] and asserted that the factors that determine the outcome of a revolutionary phase would be those inside of science [7].

Kuhn articulates a notion of progress in science that is different from what he considers to be the standard view. According to Kuhn, the standard view of science holds that progress is made in a linear fashion in order to get closer to objective scientific truths. This is done when we add new pieces of information (evidence) to our pre-existing theories. However, for Kuhn this is not the case. Science does indeed progress in his view, but it is not about getting ‘closer to truth’ by adding new pieces to a grand cumulative puzzle. Rather, science evolves [30, p. 170]. Kuhn asserts that much as evolution does not steer in any particular direction, science does not move toward an “ideal true theory” [7]. Theories shift and change depending on the particular environment that they are in. This is in contrast to the standard view that seems to imply the movement toward an ideal picture of our world. Einstein’s search for a Unified Field Theory is an example of the latter conception [28, p. 336]. For Kuhn, the progression of a science happens in stages. Sciences that lack consensus on their disciplinary matrix are ‘immature sciences.’ In

this phase, science is marked by different methodologies, theories, or philosophical foundations (for example, positivism or scientific realism). Progress in such a state, if it occurs at all, is achieved only in a small area, but does not occur collectively. This is because most of the “intellectual energy is put into arguing over the fundamentals with other schools instead of developing a research tradition” [7]. An example of an immature science would be string theory, as it has yet to reach a consensus on its methodologies, philosophical basis, and techniques.

3.3 Paradigm use

A central notion of Kuhn’s is that the quality of a theory is determined by comparing it to a paradigmatic example, in contrast to the standard view that the quality of a theory is determined through rules of scientific rationality, the rules of which create the foundation of the scientific method [39]. Mature, normal sciences progress on “the basis of perceived similarity to exemplars” [7]. Exemplars are novel ‘puzzle-solutions’ that are regarded as ideal exemplifications of the disciplinary matrix. New hypotheses are ‘held up’ against these exemplars in order to determine their compatibility within the science. As such, exemplars are central theories that are used in a comparative way. An example of an exemplar is the mathematization of the electromagnetic field by James Maxwell [29, p. 23]. This became a central theory within its domain, and new theories were compared heavily against it in order to determine if they would fit into the overarching science.

In the standard view, new hypotheses require rational justification in order to determine “whether a new hypothesis should, in the light of the evidence, be added to the stock of accepted theories” [7]. Kuhn’s account of justification in science diverges significantly from the standard view. In Kuhn’s view, scientists depend heavily on similarity to exemplars in order to evaluate new information. However, the perception of similarity is a difficult notion to understand and utilize [7] and his

use of perceptions of similarity as grounds for making such decisions is a cloudy process. Kuhn posited that the perception of similarity could not be a rule-oriented rational process. Perceptions of similarity, according to Kuhn, are theory-dependent (as is necessary for a comparison) and impermanent (as paradigms will shift upon cycles of revolution) [7]. Due to this impermanence, we cannot maintain one particular method of evaluation as such methods are subject to change. This very important point leads Kuhn to what he calls ‘incommensurability’, one of the central notions of his project.

3.4 Incommensurability thesis

A key implication of incommensurability is the notion that because revolutionary phases require the reworking of fundamental scientific standards, a shift in a paradigm precludes science from being a cumulative process simply adding to objective knowledge. Alexander Bird asserted that there are three different ‘forms’ of incommensurability in his discussion of Kuhn’s work [7]. Methodological incommensurability is the example mentioned above, whereby “puzzle-solutions from different eras of normal science are evaluated by reference to different paradigms” [7]. Thus according to Kuhn it is *very difficult* to compare old theories to newer theories because there is no standard means of comparison as fundamental scientific standards have changed. Another example of methodological incommensurability is that the types of problems that should be solved may change. This is because, as our disciplinary matrix changes, a shift in methodology, theory, etc., may no longer encourage us to ask one ‘sort’ of question, but may under the new standards encourage us to ask a different ‘sort.’ Since our decision-making is not rule-dependent, even those scientists working within the same disciplinary matrix need not agree on others’ evaluations of their theory [7]. However, for the most part Kuhn believes that scientists still manage to create some kind of

consensus. This consensus is based on five characteristics that provide a shared basis for determining how to choose between competing theories. These characteristics are accuracy, consistency, scope, simplicity, and fruitfulness [32, p. 95], although this list is not exhaustive and serves only as a loose guideline (a good example of a theory that has these characteristics would be the general theory of relativity). Kuhn asserts that accuracy is the agreement between the results of already-accepted experiments with results from the new theory [32, p. 95]. This agreement happens within a particular scientific domain [32, p. 95]. A theory must be consistent, not only internally, but also with the other theories that have been accepted. A theory must have scope in the sense that the consequences of a theory should reach farther out than what observations, ‘subtheories’, etc., sought to explain [32, p. 95]. Kuhn upholds the concept of simplicity as a virtue in science, and posits that theories must clearly organize confusing phenomena. Finally, a theory must be fruitful in the sense that it enables and encourages further development and research through the generation of novel ideas and predictions. However, Kuhn was careful to maintain that these characteristics were guidelines rather than hard rules [32, p. 95]. The aforementioned characteristics are flexible, imprecise, and do not determine scientific choice.

Another type of incommensurability is observational incommensurability. This is largely a discussion about the nature of perception and how it affects our science. Kuhn developed the notion of theory-dependent observation, a denial of the standard view that observations are theory-independent in the sense that they act as “the neutral arbiter between competing theories” [7]. Kuhn promoted the idea that our observations in science depend heavily on our preconceived notions and experiences. Thus, according to Kuhn, we are not impartial observers collecting data; we are collections of biases that inadvertently affect our science. From this we get observational incommensurability, as different observers will perceive

observational data differently.

The last form of incommensurability is semantic incommensurability. Semantic incommensurability is the notion that the semantics (meaning) used to describe scientific theories shift between phase-changes. The semantics are incommensurable because they represent two distinct images (two different 'sets' of meanings) that cannot be collapsed into one another. So, a word j may mean y in one normal phase but may mean x in the next phase. Kuhn uses the word 'mass' as an example. He asserts that Newton and Einstein used 'mass' very differently. According to Kuhn, "Newtonian mass is conserved; Einsteinian is convertible with energy" [30, p. 102]. He concluded that the term 'mass' could not be conceived of in the same way by Newtonians and Einsteinians, showing how terms change meaning between revolutionary phases. This example of semantic commensurability is highly contested, and thought by many to fail [68], as "Newtonian mass is a limiting case of relativistic mass" [47]. But Kuhn insisted that semantic incommensurability is an indication of a wider reality that scientific knowledge does not lead us closer and closer to objective truth. The standard view asserts that although Einstein's relativistic model was supposed to replace Newton's model, it appeared that Newton's model was just a special case of Einstein's [7]. As such, in the standard view, Einstein's notions lead us closer to truth, closer than Newton's could have. However, due to Kuhn's concept of semantic incommensurability, it would be impossible and incoherent to make such a comparison. The translation of terminology from one phase of normal science to the next phase is not possible, and as such, so too is the notion that science moves closer to objective truth. So according to Kuhn we cannot compare different phases of science against one another, because our very language with which to talk about them has fundamentally changed during the phase-change.

However, there remains a continuity in both mathematical and experimental

practice that may work against Kuhn’s notion of incommensurability. For example, we have access to the mathematics and experiments that Newton was able to perform, but our interpretation of the mathematics and experiments may be different. As such, “one might say that it is the interpretations that are incommensurable, not the practice” [47]. So it may be the case that Kuhn was mistaken on what incommensurability would qualify as.

The development of this idea of incommensurability has its basis in an intuition that Kuhn had as he looked back to the history of science. It is partly to account for how our involvement in current science distorts our view of old science. If, for example, we think back on the discovery that the earth rotates around the sun (and not the other way around) it seems absurd to have thought differently. What Kuhn is aiming to show is that science done within its period is *reasonable* science. It is easy to look back on theories and see them as being strange based on our current understanding. This, according to Kuhn, is not because ‘old science’ is bad science’ but rather because the two images are incommensurable. This implies a sort of relativism that supports Kuhn’s notion that normal phases cannot be compared to one another. However, Kuhn still maintains that there are external constraints that enable us to more easily compare periods in normal science. Thus, it is possible for a new paradigm to display “a quantitative precision strikingly better than its older competitor” [30, p. 153].

3.5 Objections

Although Kuhn’s work in *The Structure of Scientific Revolutions* was highly influential in science and philosophy, it has been criticized along several lines. Both Kuhn’s account of the development of science and his notion of incommensurability have been met with harsh disagreement from philosophers, historians, and scientists alike.

Throughout his book, Kuhn maintains a clear demarcation between normal and revolutionary phases in science. During revolutionary phases, paradigms are questioned, revised, or replaced. During normal phases, these paradigms are maintained and fall into the background of the scientific landscape. Revolutionary phases are “particularly significant and reasonably rare episodes in the history of science” [7], and normal phases are common periods of maintaining the status quo. Stephen Toulmin took issue with this particular distinction. Toulmin argued that the process of scientific change is far less dramatic and occurs much more often [7]. Toulmin also maintained that paradigm changes occur not just in Kuhn’s revolutionary phases, but in normal phases as well. As such, according to Toulmin, Kuhn’s distinction between revolutionary and normal phases requires further justification and explanation. Bird asserted that to this criticism, Kuhn could reply that “such revisions are not revisions to the paradigm but to the non-paradigm puzzle-solutions provided by normal science” [7]. This however requires Kuhn to develop a method for distinguishing in science elements that are paradigmatic and non-paradigmatic, which Kuhn has not provided.

This is a legitimate challenge to Kuhn’s project. I would agree with Toulmin in that Kuhn’s clean distinction between ‘phases’ is an idealization. Kuhn wants discrete phase-changes that lead to a paradigm shift with no overlapping of theory, semantics, and methods. Kuhn likened a paradigm shift to a Gestalt-switch that occurs when the same image appears to be either a rabbit or a duck, depending on someone’s interpretation of his or her experience [30, p. 120]. But in practice it is very difficult to determine which phase one would currently be in. And even if it were possible to delineate between phase changes, we would need to determine on whose authority such a delineation is decided. A possibility is that phase changes are determined by the consensus of a scientific community, which is arguably normative. However, as Kuhn rightfully observed, scientists working within a period

of normal science tend to reject movements toward revolutionary periods of science. As such, scientists during a normal period would be disinclined to recognize a phase change into a revolutionary period. This same line of reasoning would apply to the inverse. If it is the case that ‘fringe’ revolutionary scientists who reside in a time of normal science are suppressed, then it seems highly unlikely that a consensus would be reached recognizing a phase change. And as such, maybe what Kuhn stipulates about how revolutions occur (as being heavily influenced by non-scientific factors) gains a more solid footing. But even then, what counts as being ‘revolutionary’ versus what counts as being ‘normal’ likely falls along a gradient rather than being distinctly dichotomous. We can conclude then that clean-cut phase changes are an idealization.

Against Kuhn, it may also be the case that periods of ‘revolution’ (not in the Kuhnian sense) in science need not be instigated by anomalies, as he has suggested. Bird uses the discovery of DNA as an example. The double-helical structure of DNA was not discovered as a result of the presence of anomalies but rather was discovered during the course of ‘normal’ science. The double-helical structure immediately became a jumping board from which many new areas of science were developed. As such, Bird claimed that it is not necessarily the case that revolutions are always instigated by anomalies, but just occur in the process of normal scientific inquiry [7]. However, against Bird, it may not be the case that this example qualifies as a ‘revolution.’ This example may better be referred to more modestly, not as a revolution but as an important discovery that enabled a great deal of new work to be done in the field.

My evaluation of Kuhn’s project is that it was an oversimplification of a highly complicated process (changes in science) in a field with a long history. Kuhn attempted to obtain a complete description of scientific change, likely fueled by what I believe to be a philosophical desire to “understand how things in the

broadest possible sense of the term hang together in the broadest possible sense of the term” [58, SPR: [p. 1]. But these types of ventures wherein a consistent philosophical picture is aimed at are very difficult (maybe impossible) to carry through to a definitive success. We need only look at the Logical Positivists and their attempt to rid philosophy of metaphysics to see that such ambitious endeavours in philosophy will never be completely successful, although these attempts are endemic to philosophy. Such is the nature of philosophy, to constantly analyze and critique, in which case ‘knockdown’ arguments (arguments that establish their conclusion definitively) in philosophy are few and far between (Hume’s problem of induction may be considered to be one of the only ‘knockdown’ arguments [65]). And ‘knockdown’ arguments become even less likely to be successful when they broaden their reach to attempt to create a consistent and accurate scientific and philosophical picture. That is not to say that Kuhn’s project was neither admirable nor useful; on the contrary, I believe that it was indeed admirable and that pieces of his theory can be utilized very nicely. So although it is the case that we cannot use Kuhn’s theory in its entirety, I argue that there are several, very important notions that we can draw from it.

One of Kuhn’s faults was that he tended to use certain words and phrases with several different meanings. In philosophy, the ambiguous usage of terms is a subtle but potentially damning error. Throughout his work, Kuhn seems to use the term ‘paradigm’ both as a set of concepts that has been agreed upon by a scientific community, and also as a *particular* theory within science that can be used as a comparison against other theories (he also would use the term ‘exemplar’ for this). He does not, however, qualify the difference in usage and as such it leaves his audience to fill in the gaps of his writing (which is a problem if we are aiming for univocality). Although this was a mistake on Kuhn’s part, I believe his second usage of the term (‘exemplars’) is a useful idea in philosophy of science. I would argue

that we *do* have such theories in science that function as exemplars. The tenets of Einstein's relativistic mechanics come to mind. In fact, Einstein's relativity is believed by many scientists to be a cornerstone of modern physics. So what Kuhn may have done is bring to light that comparison against exemplars occurs in science, and from there we can determine whether this method is a good tool to use in the philosophy of science.

While I agree with Toulmin that Kuhn's incommensurability is problematic, I do believe that it highlights several important concepts that should be noted in science. Regarding methodological incommensurability, it is clear that methods in science have changed over time. The Hippocratic school strongly favoured the role of observation [9]. Thales of Miletus is hailed as the founder of natural philosophy, the phrase 'natural philosophy' later to be replaced with 'science' [46]. Plato emphasized the role of "reasoning as a method, downplaying the importance of observation" [1]. Aristotle advocated for the importance of method, observation, experimentation, and supported empiricism [1], arguably a return to the Hippocratic school. I argue that Kuhn's notion of methodological incommensurability is in fact too dramatic an account. Although specific methods in science change, the general scientific mode of inquiry wherein scientists deal with empirical subject matter remains constant. If for every revolutionary phase a completely novel methodology was created, we would see in the history of science *vastly* different conceptions of what it means for something to qualify as 'science'. But that is not the case. If we understand science to be a general mode of inquiry that deals with empirical subject matter, we can see that such a theme is common to the history of science, before and after what Kuhn may consider to be 'revolutionary' periods. Kuhn's example of the Copernican revolution can elucidate my point. Copernicus' heliocentric model supplanted the Ptolemaic geocentric model after it became apparent that the latter theory was rife with anomalies [30, p. 69]. The Ptolemaic method of using cycles and epicycles

required more and more complex explanations to explain away anomalies. Kuhn cites several ‘ingredients’ that caused the paradigm shift such as “social pressure for calendar reform” [30, p. 69] but stipulated that the “technical breakdown would still remain the core of the crisis” [30, p. 69]. It was not until Ptolemaic method was abandoned by both Galileo and Kepler that the Copernican model was accepted, although only after much resistance [30, p. 71]. So we can certainly see a change in the tools, the method that were used by scientists within these different paradigms. But the general method of inquiry remained consistent wherein, as Susan Haack has posited, we “make an informed guess at the possible explanation of the event, figure out the consequences of that guess, see how well those consequences stand up to the evidence, and then use his [the scientist’s] judgment whether to stick with the initial guess, modify it, drop it and start again” [20, p. 14]. So although specific scientific tools may change, like mathematical models or particular instruments, the general mode of inquiry remains constant throughout phase changes. This conclusion is what we can pull from Kuhn’s notion of methodological incommensurability.

Regarding semantic incommensurability, Kuhn has made a good point in that the meanings of the words that we use in science can change without the recognition of a change. Although I disagree that semantic incommensurability leads us to the conclusion that science necessarily cannot be a cumulative picture, it reminds us of the importance of analyzing what we mean when we use certain words.

Interpretations of quantum mechanics, for example, are often plagued with ambiguous and ill-defined phrases. Part of what philosophy can contribute to quantum mechanics is cleaning up ambiguous usages of terms. Thus, Kuhn has picked out an important notion, although I do not think he is justified in his particular conclusion.

Observational incommensurability is of particular importance as it highlights the fact that science is not a purely objective endeavour. Current research in

cognitive bias shows us that our brains are very adept at subconsciously seeking out the results we wish to obtain. This poses a particular problem for scientific research [44]. In physics, cognitive bias takes many forms: viewing data that confirms your hypothesis more favourably and viewing data that disconfirms your hypothesis more skeptically [45], collecting data that confirms your hypothesis while ignoring data to the contrary [45], and question-begging arguments in physical theories ([40], [48], [69]). It is the case that our observations are tinted by our experiences and recognition of this fact will enable us to do better science by guarding ourselves against it. If we fall into the belief that we are completely ‘neutral arbiters’ of observations, we lose the ability to be fully aware of the biases that play a role in our observations. This is not to say that science has not recognized the significant problem of cognitive bias and taken steps to mitigate it. But what is important is that we remind ourselves of the influence that cognitive bias has in our perceptions, as this recognition enables us to minimize bias in our observations.

There is also something good to be said about Kuhn’s use of history in his development of his philosophy of science. Kuhn relied heavily on the history in order to show how rules of method, underlying philosophical assumptions, and techniques have changed. Throughout its history, the philosophy of science has taken many forms: mechanism, positivism, realism, and instrumentalism, amongst others. The philosophy of science lays the foundation for scientific methodology (empirical data, hypothetico-deductive models), the determination of competing theories (by using criterion of, for example, elegance and simplicity) and what it means for something to be considered science (falsifiability, verificationism). As such, different scientists at different periods of time are integrated into different philosophies, and their science (methods, philosophical assumptions, etc.) developed, sometimes unknowingly, according to these ideas. The integration of history is a positive concept that we can take from Kuhn’s ideas. Being

knowledgeable of the history behind science encourages scientists to identify and analyze the ‘rules of method’ that they employ. Inquiry into the history of science can enable us to draw connections between past conceptual, methodological errors, so that we are able to identify them in current theories. This specific style of analysis and inquiry is central to the philosophical method, and as such philosophers generally are particularly adept at this. This enables scientists to take part in the history and philosophy of science, thereby encouraging them to examine the (likely unexamined) philosophical assumptions that create the foundation for their theories. Knowledge of the history of science may also help scientists to put their own place in the scientific landscape into perspective. Science as an endeavour has a rich and complicated history, and is filled with people who got things very wrong [14]. Historical knowledge can inform our philosophy of science and can show us that science as an endeavour is a fluid process, one that has changed drastically throughout its history and one that *will continue to change*.

I believe it is of particular importance that we can use Kuhn’s ideas to encourage scientists to look more charitably toward proposed theories that challenge their preconceived notions of what fits into their scientific landscape. We should aim to be more charitable because when we fail to do, we do a disservice to science and scientists. For example, Hans Kramers, a doctoral student of Niels Bohr, developed the theory of what is now called the Compton effect [13, p. 40], [36]. It was Hans Kramers who first developed the idea that the scattering of a photon causes an increase in wavelength in that photon [11, p. 16]. However, Kramers’ work was contrary to several preconceived notions that Bohr held about the discrete nature of light particles, and Bohr convinced Kramers that his concept was nonsense. The Compton effect was later credited to Arthur Compton in 1923, who had developed the same theory independently [13, p. 40], [36]. But due to Bohr’s unwillingness to challenge his own firmly held beliefs, an important scientific idea was developed

much later than it could have been, stunting the progress of science, and a young scientist was discouraged and lost an important opportunity.

3.6 Conclusions

In relation to my overall project, we can pull several ideas from Kuhn’s work. Scientism, as defined by Susan Haack, is “a kind of over-enthusiastic and uncritically deferential attitude towards science, an inability to see or an unwillingness to acknowledge its fallibility, its limitations, and its potential dangers” [20, p. 2]. I must take a moment here to discuss a common immediate response to the definition of scientism that Haack has proposed. There is often a conflation between skeptical attitudes toward science, its methodology, and its epistemological status, and an anti-science attitude. But this conflation is a mistaken one, as it is absolutely not the case that a skeptical attitude toward science and its claims is one and the same with an anti-science attitude. But I will cover the discussion of the line between skepticism and anti-science attitudes in a later section.

I argue that scientism exacerbates the isolation of non-mainstream ideas, non-mainstream ideas being the product of what Kuhn calls ‘divergent thinking’. When we treat science with undue deference, denigrate ‘non-scientific’ disciplines, and value scientific methods of inquiry over all other methods, we discourage open discussion between different disciplines (e.g., philosophers and physicists). When we discourage open interdisciplinary discussion and criticism, we stifle the creation of new ideas, thus discouraging divergent thinking. Such interdisciplinary work has the ability to lead to tangible discoveries in science. For example, Ivette Fuentes, a physicist at the University of Nottingham has taken philosophical insights regarding the nature of time and has created new experiments based on those insights. Her discussions with philosophers Jonathan Tallant and Stephen Mumford has led her to question how a clock’s capacity to measure time is influenced by quantum laws [43].

This question led to the development of “a model for a quantum clock moving in space-time and Per Delsing’s group at Chalmers University is implementing the clock in their laboratory” [43]. This experiment established a new result, showing that the creation of particles (a quantum effect) slowed the clock down [43], [35]. So collaboration between disciplines (and I am advocating specifically for the collaboration between physics and philosophy) can lead to new scientific results. Creative thinking is necessary for scientific innovations. But often, a scientific community can be very hostile to creative ideas that they perceive as being *too creative*, too outside of the mainstream belief set. This is the ongoing struggle between traditional scientific values and the desire for innovation. Changes in physics (such as the quantum revolution and the Copernican revolution) are important examples of the struggle between prevailing accepted theories and new theories that seem to challenge the status quo. This struggle is important because it determines the trajectory of science, and it is a concept I shall explore further.

Lee Smolin writes extensively about the struggle between traditional ideals and innovative ideas. Lee Smolin is a theoretical physicist at the Perimeter Institute for Theoretical Physics in Waterloo, Canada, who has made contributions both to physics (as a contributor to quantum gravity) and philosophy of science (with contributions to conceptions of time and work on foundations in quantum mechanics). In his book *The Trouble with Physics* [59] Smolin seems to agree with Kuhn’s notions about ‘normal’ and ‘revolutionary’ periods in science [59, p. 310]. *The Trouble with Physics* is largely a discussion of how progress in physics has slowed severely due to its fixation on string theory, a theory that Smolin determines to be an unsuccessful theory [59, p. 198]. Another theoretical physicist, Peter Woit, has criticized string theory along the same lines in his book *Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law* [71]. Although Smolin concedes that science does not necessarily proceed in such a fashion (of

discrete alternating phases), Smolin does hold that “there are certainly normal and revolutionary periods, and science is done differently during them” [59, p. 310]. And according to Smolin, not only is science done differently during each phase, the type of scientist required for each period differs dramatically. But Smolin asks if the current ‘sociological’ features of academic physics provide an environment that allows each type of scientist to thrive; indeed, he claims that the current academic environment is not at all conducive for the flourishing of a particular kind of scientist, one that he names ‘the seers’ [59, p. 309].

Seers, according to Smolin, are individuals who have the ability to ask “genuinely novel but relevant questions and look at the state of a technical field and see a hidden assumption or a new avenue of research” [59, p. 309]. Seers are technically proficient and capable of engaging with the relevant literature in their field. However, seers tend to be visionary ‘big picture’ thinkers who may be less interested in mathematical cleverness. Smolin’s example is Albert Einstein, who although proficient at the mathematics involved in his theorizing, was certainly not as strong mathematically as say David Hilbert or John von Neumann [59, p. 309]. But Einstein undeniably made some of the most revolutionary discoveries about the physical world [28, p. 280]. Smolin also mentions Niels Bohr, whose research notebooks consisted almost exclusively of verbal argumentation and pictures rather than equations [59, p. 309]. Seers are one of the main sources of periods of revolution [59, p. 310].

Normal periods of science require and attract craftspeople. Craftspeople tend to be technically gifted puzzle-solvers. Craftspeople are the talented technicians who work out mathematical intricacies. They tend to be more concerned with the formalism rather than broad philosophical questions. In logic, I would compare Smolin’s categorization along similar lines. You could qualify those individuals in logic who can churn out, quickly and elegantly, proof after proof, as craftspeople.

The seers could be those who tend to focus on the philosophy behind logic, such as logic's connection with natural language. I would argue that the distinction that Smolin is making between seers and craftspeople, articulated further, is the difference between scientists who employ the philosophical method and those who do not. Although Smolin recognizes, of course that scientists can be a mixture of both 'seer' and 'craftsperson', he maintains that generally "the majority of theoretical physicists I know fall into one or the other group" [59, p. 311]. In fact, I might go so far as to replace 'seer' with 'philosopher'. Einstein, in a letter to Robert Thornton, a philosopher of science, advocated for the integration of history and philosophy into science:

I fully agree with you about the significance and educational value of methodology as well as history and philosophy of science. So many people today, and even professional scientists, seem to me like somebody who has seen thousands of trees but has never seen a forest. A knowledge of the historic and philosophical background gives that kind of independence from prejudices of his generation from which most scientists are suffering. This independence created by philosophical insight is, in my opinion, the mark of distinction between a mere artisan or specialist and a real seeker after truth. [27]

This quotation expresses a large part of what I am advocating for, that philosophy and the philosophical method gives us the ability to 'see the forest'. Philosophy is in the game of asking big questions. It is primarily concerned with the analysis of concepts, argument structure, and use of language. Philosophers develop highly specialized tools with which we can analyze arguments, and such a methodology can be easily applied to physical theories. Thus, through the use of the philosophical method scientists can be wary of prejudice (aided by critical analysis) and can engender intellectual independence. The individuals who utilize the philosophical method are what Smolin calls 'seers', and it is during times prior-to-and-during revolutions that seers are necessary for the progression of science.

And ‘seers’ are exactly what Smolin asserts we need more of. Smolin posits that, regarding Kuhn’s alternating phases in science, “we are indeed in a revolutionary period, but we are trying to get out of it using the inadequate tools and organization of normal science” [59, p. 311]. He categorizes physics as being stuck in a revolutionary period that began at the beginning of the twentieth century, during the quantum revolution. This period, fuelled by the visionary seers such as Einstein, Bohr, Heisenberg, and Schrödinger (among others) prompted but failed to complete the revolutionary period. But as these revolutionaries began to die out, physics required difficult technical work in order to catch up to the theories that they (the revolutionaries) proposed. Thus, physics moved into a phase of normal science “and was dominated by master craftspeople” [59, p. 311]. And, according to Smolin, as physics became dominated by Americans during the 1940’s, there was a definitive triumph of craftspeople over seers. This led to a diminishing of the more reflective and philosophical style that was prevalent during the quantum revolution and the ascendancy of what Smolin classifies as “the pragmatic, aggressive mode that gave us the standard model” [59, p. 312]. Foundational work in quantum mechanics took a backseat, and “the spirit was pragmatic; ‘Shut up and calculate’ was the mantra” [59, p. 312]. An example of the ‘pragmatic’ style is the concerted effort to get a vast amount of experimental results from the predictions of the Standard Model. Technical prowess became the most useful attribute that a physicist could have. But the revolution that began with Einstein and his philosophically-inclined colleagues could not be completed, even after the success of the development of the standard model. Smolin asserts that normal science has taken us as far as it can, but can take us no further, and that seers are necessary for us to move into a new revolutionary period beyond string theory [59, p. 312]. The difficulty, however, is that the period of normal science that has just occurred has made for an environment that is hostile to seers, “science having been done so long

in a way that rarely recognized and barely tolerated them” [59, p. 312]. This ‘dying out’ of seers, according to Smolin, is a result of the professionalization of the academy and the dominance of the methods of normal science [59, p. 313]. The professionalization of the academy meant that “the practice of normal science has been enshrined as the single model of good science” [59, p. 313]. As such, science became entrenched in the methods, language, and tools of normal science that have subsequently dominated the scientific landscape. Issues such as the foundations of quantum mechanics and the nature of space and time have fallen into the background [59, p. 313]. But there are still a few physicists and philosophers who are concerned with tackling these conceptually difficult philosophical problems, even if the academy of physics provides a less-than-hospitable environment for it.

It does appear to me that Smolin’s assessment of the period in which we currently reside is somewhat confused. That the quantum revolution was just that, a revolutionary period, is a reasonable assertion, and one that Kuhn agreed with [7]. But Smolin’s classification of physics being *currently* in a revolutionary period is mistaken [59, p. 311], and in fact he seems to claim both that physics is in a state of normal science *and* revolutionary science [59, p. 311]. But this surely is reflective of the difficulty I mentioned earlier, wherein the delineation between phases is practically difficult and strong delineation is an idealization. Bearing that in mind, I argue that we are currently shifting from a period of normal science (which began after the revolution of quantum mechanics) into a period of extraordinary science. The transition from normal science to extraordinary science “includes the community’s recognition that the reigning paradigm is unable to account for accumulating anomalies” [38]. These accumulating anomalies, which I will discuss later, predominantly include troubles in string theory, foundational problems in quantum mechanics, and conceptions of space and time. But regardless of Smolin’s slight mischaracterization, the takeaway is the same, that “it is a fantasy to imagine

that foundational problems can be solved by technical problem solving within existing theories” [59, p. 314].

So it seems that Kuhn’s theories can still be helpful in the current predicament that Smolin is writing about. Kuhn’s *Structure of Scientific Revolutions* can serve as a reminder that science is fundamentally a human enterprise. Even if we take it to be the case that science is describing reality in some way, it is fallible humans who practice it. My aim was to show that we could still use Kuhn’s theory to encourage scientists to be aware of their history, examine the philosophical underpinnings of their disciplines, and be open and charitable to ideas that may be contrary to their own. Knowledge of the history of science encourages us to not repeat the same mistakes, and also encourages us to recognize that science is an imperfect process. Examining one’s philosophical foundation allows for greater consistency and less likelihood of a methodological, conceptual, or semantic error. Being less convinced that your particular paradigm is ‘correct’ encourages the consideration and creation of new and innovative ideas. This humbled uncertainty also discourages dogmatic beliefs in science. But what is most important for my overall project is to pull from Kuhn his notions of phase change as they are particularly helpful when talking about the current state of physics as requiring a paradigm shift, an idea I shall elucidate in a later section. But we have to leave aside Kuhn’s incommensurability in the robust sense, outside of using it to remind ourselves to check our language usage, to be aware of shifting methodology, and to check cognitive biases.

Chapter 4

Scientism

I consider the comments that I have mentioned previously by Hawking, Krauss, and Weinberg, to be manifestations of a scientistic attitude. Susan Haack defines scientism best as “a kind of over-enthusiastic and uncritically deferential attitude towards science, an inability to see or an unwillingness to acknowledge its fallibility, its limitations, and its potential dangers” [20, p. 2]. In her book *Defending Science—Within Reason* Haack discusses the recent uprising in scientistic attitudes and the effect that it has on our conceptions of knowledge.

Haack begins by discussing the etymology of the word ‘scientism.’ She notes that although currently ‘scientism’ is predominantly used in a pejorative sense, this is a relatively recent change [20, p. 2]. During the nineteenth century, the word ‘science’ generally referred to “any systematized body of knowledge” [20, p. 2]. As such, the word ‘science’ had a very broad usage, and included subjects like physics, chemistry, philosophy, and history. Our usage of the word greatly narrowed in its scope as to what fell under the umbrella of ‘science.’ The modern usage is now reserved to refer to disciplines like physics, chemistry, and biology, while casting out subjects like philosophy and sociology. The modern usage of ‘scientism’ began to adopt a pejorative tone early in the twentieth century, the word being used as a designation for an overzealous attitude wherein the methods of the highly-successful

natural sciences could (and *should*) be applied to all areas of human knowledge [20, p. 3]. Currently, ‘scientism’ is used to refer to a general attitude of undue deference to science and a general attitude that science is uniquely and solely epistemically authoritative.

There are several important questions that must be addressed. We must determine what does and does not qualify as being an instance of scientism, and when it is and is not appropriate to defer to the sciences. Most important however is the determination of *why* a scientific attitude ought to be avoided. Haack outlines ‘six signs of scientism’ in the hopes of answering some of the aforementioned questions.

4.1 The honourific use of “science” and its cognates

Gradually, words that signify the scientific endeavour have taken on a new role. Terms closely related to science like ‘scientifically,’ ‘evidence,’ and ‘scientific study’ have now arguably become correlated with ‘epistemically good.’ Science as a discipline has been gaining more and more prestige, while at the same time meeting more and more resistance from those who are ‘anti-science.’ As science has grown in prestige, those wishing to take advantage of the discipline’s rise in prestige have claimed scientific terminology for their own. For example, the phrase ‘scientifically proven to work’ has become commonplace in advertising products. ‘Unscientific’ became a word to describe something that is not epistemically trustworthy. And the effect of science being considered as epistemically good is not only relegated to the confines of advertising. As Haack has studied intensively, the notion of science as being epistemically good has reached, for example, the American justice system, wherein expert testimony in court proceedings in the U.S. supreme court require

that ‘such testimony must be “scientific knowledge,” arrived at by the “scientific method” ’ [20, p. 5] and according to Haack, “the honorific usage is ubiquitous” [20, p. 5]. As is to be expected, it is not only advertisers who have adopted scientific terminology as a means to garner prestige or trustworthiness. Haack posits that disciplines that no longer fell under the umbrella of ‘science’ began to use phrases like ‘scientific’ to lend perceived credibility to their ‘non-scientific’ approaches. For example, Albert Hobbs notes the increased usage of ‘scientific’ and ‘scientific conclusions’ in sociological studies, sociology being a discipline that no longer qualified as being strictly scientific [20, p. 20]. So when ‘science’ becomes an honourific word, nearly everything wants to acquire the label, whether or not the label is accurate or deserved.

But as Haack points out, the honourific usage of ‘science’ creates a lot of problems [20, p. 6]. She asserts that when we correlate ‘science’ with ‘epistemically good’ we cloud over several key realities: that not all science is good science, not all scientists are good scientists, and that this correlation has the tendency to seduce “us into the false assumption that whatever is *not* science is no good, or at any rate inferior” [20, p. 6]. Undoubtedly, scientific achievements are among some of the best achievements of human cognition; but it would be a mistake to say that scientific achievements make up the entirety of the achievements of human cognition. Haack’s final point is that the honourific use of ‘science’ also “encourages uncritical credulity about whatever new scientific idea comes down the pike” [20, p. 6]. This assertion is perhaps more hasty than I would accept. Generally I would say that new scientific ideas within an academic setting are treated with the appropriate amount of skepticism. Particularly with scientific claims that may have resounding influence, scientists often are very careful to satisfactorily confirm their assertions (usually via experiment) before making a claim. A wonderful example of this is the meticulously cautious manner in which the discovery of the Higgs boson was announced [37]. But

while I do think that Haack was somewhat uncharitable in this instance, she is very correct in saying that many explanations in science tend to be “at first, highly speculative, and most are eventually found to be untenable, and abandoned” [20, p. 6]. It is easy, when looking at the successes of science, to forget about the overwhelming amount of failure in science [73].

4.2 Inappropriately adopting the trappings of science

Unsurprisingly, when ‘science’ and its cognates become honourific terms, disciplines outside of science begin to “borrow the manners, the trappings, of these fields, in hopes of looking ‘scientific’ as if technical terminology, numbers, graphs, tables, fancy instruments, etc., were enough by themselves to guarantee success” [20, p. 7]. As such, we begin to see disciplines and practices outside of science adopt the methods and terminology of science, whether or not these adopted practices actually improve their disciplines. But as we conflate ‘science’ as being ‘epistemically good’ and inadvertently (or intentionally) devalue anything that falls outside of the umbrella of science, we push disciplines outside of science to grasp for perceived epistemic legitimacy.

Implicit in scientism is the notion of the general and complete applicability of the scientific method to all areas of human knowledge (and beyond). There is no denying that scientific procedure (whatever exactly it is, which is another matter entirely) is a highly successful technique for solving *some kinds of problems*. It is not the case, however, that we can necessarily extend this to *all* kinds of problems. Such an assertion would be a logical misstep. Different disciplines have radically different subject matters and so a ‘one size fits all’ methodology will inevitably fail. What is more is that the adoption of scientific method and terminology often serves “not as

useful transferable tools, but as a smoke-screen hiding shallow thinking or half-baked research” [20, p. 7]. Haack asserts that the misuse of scientific tools is particularly endemic in the social sciences, with the sudden appearance of lengthy sections on methodology (for example, in sociology texts) that often serve as a superficial and unimportant digression [20, p. 7]. Another example is the increased desire for the mathematization of phenomena that would usually fall outside of the scope of mathematics. I suspect that the use of Bayesian Confirmation Theory in the philosophy of science is an example of this [62]. This is not to say that the mathematization of principles or the adoption of scientific methodology is *never* an advantageous acquisition that improves a non-scientific discipline. For example, Haack describes the use of a cyclotron by historians in order to determine the type of ink used on several early prints of the bible, as well as the use of medical imaging to discover writing on lead ‘postcards’ used by Roman soldiers [20, p. 10]. Such adoptions of scientific techniques are undeniably useful and serve a real purpose. Rather, the problem is when these things are borne out of the desperate search by non-scientific disciplines for perceived credibility. Oftentimes this has the effect of non-scientific disciplines borrowing scientific methodology, terminology, and techniques “for display rather than serious use” [20, p. 10]. Likely, when the search for credibility is the impetus for such changes in non-scientific disciplines, we may be inclined to accept assertions based on their scientific trappings even though the claims may be inaccurate.

4.3 Preoccupation with “the problem of demarcation”

According to Haack, an inevitable consequence of the honourific use of ‘science’ and the adoption of scientific trappings by non-scientific disciplines, is that the problem

of demarcating between science and non-science becomes very important, a problem that Haack believes “will loom much larger than it should” [20, p. 10]. The Logical Positivists, who developed verificationist approaches to the problem of demarcation, were particularly concerned with distinguishing between metaphysical statements and scientific statements, scientific statements being at least in theory empirically verifiable [21]. Much of the philosophy of science deals directly with the problem of demarcation. But Haack concludes that the problem of demarcation need not take such a central role in philosophy of science and that “the best we might hope for is a list of ‘signs of scientificity’ none of which would be shared by all science, but each of which would be found in some sciences” [20, p. 13]. Haack does not delve into what these ‘signs of scientificity’ might be as these ideas are approximations. The term ‘science,’ in her view, just does not have any clear boundaries; what ‘science’ refers to is not in itself clearly demarcated [20, p. 13]. But Haack does not mean to say that we cannot distinguish between science and other forms of inquiry. Rather, we can loosely define what we mean by science but only, and this is important, “in a rough and ready way” [20, p. 13]. Thus the purpose for Haack is not to create a single overarching notion of ‘science,’ but rather to point out general signs of science that can be potentially shifted dependent on context. Haack asserts that science is a specific kind of inquiry rather than a body of knowledge, an inquiry that generally deals with empirical subject matter (as such, logic and mathematics fall outside of the range of ‘science’) [20, p. 13]. Normative disciplines such as jurisprudence and epistemology, according to Haack, also fall outside of the umbrella [20, p. 13].

Adding to her observation that ‘science’ does not have clear boundaries, she also stipulates that what we pick out when we say ‘science’ is not univocal and rather that ‘the sciences’ are “best thought of as forming a loose federation of interrelated kinds of inquiry” [20, p. 13]. Haack claims that generally, a ‘scientific’ kind of inquiry would be more systematic, refined, and persistent than other types of inquiry

[20, p. 14]. This could be contrasted with what Haack calls ‘common sense’ inquiry, the type of inquiry we use in our everyday lives (questioning for example, whether or not to eat a food item that may or may not be spoiled) that does not have the same type of systematic methods (like the guidelines for a proper experiment, for example) that a ‘scientific’ kind of inquiry might.

Rather than a specific delineation between scientific inquiry and other types of inquiry (philosophical inquiry, for example), Haack asserts that theories should be evaluated based on their ability to be “genuinely explanatory” [20, p. 15]. Haack wants to say that if we can soften the importance of the problem of demarcation, we can instead look toward general notions of what it means to be a seeker of knowledge. The consideration of contrary evidence in light of inquiry should be considered “a mark not, as Popper supposes, of the scientist specifically, but of the honest inquirer, in whatever field” [20, p. 15]. Haack believes that if we “suppress the demarcationist impulse” [20, p. 14] then we will better be able to determine not between science and non-science (and pseudo-science in particular) but rather between proper kinds of inquiry and improper kinds of inquiry.

I am not so sure that Haack is doing anything different than many accounts of philosophy of science. In her view, science is intimately tied in with empirical subject matter, and she seems to determine boundaries cleanly enough to, for example, leave logic and mathematics out from under the scientific umbrella. I suppose the key feature for Haack is that our definition of what it is to be science must remain fluid enough to change if needed, although I wonder in this context who we defer to in order to determine when a change *is* needed. I can certainly see the appeal of a notion of ‘science’ that is fluid, but I can see a problem with such a context-based notion of defining what ‘science’ qualifies as being. It seems to me that this context-based notion of defining science is a veiled conventional account (similar, ironically enough, to the difficulties that Popper had with his veiled

conventional account). We are led to the conclusion that since changes in our conceptions of science need to be determined by convention, we fall again into a normative notion of science. But Haack has already stipulated that normative disciplines fall *outside* of science’s umbrella [20, p. 14] so this undoubtedly seems nonsensical.

I am also not convinced that the problem of demarcation should be so easily dismissed. Now more than ever it has become important for us to be clear on what science is, to whatever extent that is actually possible. The difficulty lies in not making the mistake of automatically conflating ‘scientific’ with ‘epistemically good.’ But there is some urgency on this matter when we see, for example, children dying of treatable infections because parents shun conventional medicine [3]. In such cases, the demarcation becomes far more crucial than just a philosophical abstraction. Admittedly, such cases are far more complex than *just* the demarcation between science and non-science, and tend to stray into the territory of an ‘anti-science’ attitude. And there is no denying that an anti-science attitude is very, very dangerous in so many contexts [34].

4.4 The quest for the “scientific method”

Our preoccupation with the problem of demarcation, Haack asserts, has the effect of instilling the notion that scientific inquiry holds a special epistemic value over other kinds of inquiry due to its particular methodology, the “scientific method” [20, p. 16]. But as can undeniably be seen throughout the history of philosophy of science, to define ‘scientific method’ as a one-size-fits-all entity that can be applied across *all* disciplines in science is to fight a losing battle. Haack cites several forms of inductivism (which posits that scientific knowledge is determined via induction from observation), deductivism (the inference from theories to observations), as well as current usages of Bayesian confirmation theory (a probabilistic approach) in order

to show how varying philosophies of science really are [20, p. 16]. What Haack wants us to pull from this is that not only are these notions of the scientific method very different, but they are mostly incompatible. As such, it may be the case that the quest for a uniform scientific method to define the procedure of all of the disciplines under the umbrella of science is misguided. Paul Feyerabend advocated for a radical, anarchistic form of the idea that there is no scientific method, in that the only method in science that should be used is “anything goes” [55]. Feyerabend was unsurprisingly widely ostracized by philosophers of science after this particular idea became known [55], and is generally relegated to the realm of ‘fringe philosophy of science.’ The relegation of Feyerabend to ‘fringe philosophy’ however may have been an unfair reading of him. A more fair reading of Feyerabend’s radicalism is that his intention with his ‘anarchistic model’ was to show how the logical empiricist program leads us to some very unsatisfactory conclusions, and that such a program would be unwise to follow. A weaker and more plausible version of this is that there is no *constant* methodology in science, or that there is no single, overarching method but rather many different methods that are used by different scientific disciplines [20, p. 16]. These two versions combined, that there is no constant and single overarching methodology in science, is what Haack is advocating for.

What Haack thinks that scientists *as well as any other empirical inquirers* do is make an informed guess at a potential explanation for some phenomenon, determine the consequences from that guess, and see how their guess stands up in light of any evidence. After this the inquirers can modify their hypothesis, accept it, or throw it by the wayside. This is what Haack calls the underlying procedure of thoughtful and honest inquiry [20, p. 17]. According to Haack there is no single underlying method to all science, but what is unique to each science is the specific set of tools and techniques, or “scientific helps” [20, p. 17]. These tools and techniques enable science to “refine, amplify, and extend unaided human cognitive powers” [20, p. 18]

and as such enable science to progress. So what differentiates the natural sciences from the social sciences, for example, is the set of tools unique to many of the disciplines in the natural sciences that the social sciences would not contain.

At face value I can see the appeal of the notion that methodology in science is neither constant nor uniform across all scientific disciplines. It grants scientific inquiry a certain fluidity. But the maintenance of some form of demarcation between science and non-science is hindered by the lack of an overarching scientific methodology. If an overarching methodology is a key element in the delineation between science and non-science, then the lack of a ‘universal’ methodology hinders the ability to delineate between science and non-science. As such, an overarching scientific methodology may serve as a heuristic in the demarcation problem.

4.5 Looking to the sciences for answers to questions beyond their scope

The issue of the ‘encroaching borders of science’ on disciplines not within the scientific umbrella (we might call this the ‘imperialist expansion of the natural sciences’) stems largely from the notion that science is pushing into areas of inquiry that are beyond the scope of science. The idea that science and the scientific method can answer all possible questions within the realm of human knowledge has become more and more popular, which is a result, I argue, of the rapid insurgence of scientific attitudes being popularized by ‘celebrity-scientists’ (such as Bill Nye and Neil deGrasse Tyson, both of whom have very publicly denigrated philosophy). It is becoming more commonplace to find books on store shelves claiming, for example, that science can determine human values [22]. Such questions have generally been kept in the realm of philosophy. But as scientism becomes more of a force in our understanding of what is epistemically good, legitimate questions that fall outside of

scientific inquiry are seen as being less ‘rigorous’ and there is a belief that in order to give them credibility they must be tackled scientifically.

This is not to say that boundary-shifting between science and other areas of inquiry is *necessarily* scientific. Oftentimes questions in disciplines outside of science have found answers in the scientific endeavour, and as such have added to the landscape of human knowledge. Take for example the traditionally metaphysical question of “why is there something rather than nothing” that was adopted and worked on by cosmologists [20, p. 19]. Although it is debatable whether or not the inclusion of cosmologists has solved this particular conundrum, it is undeniable that the use of scientific inquiry in such a case has been a positive contribution. But it is a mistake to say that scientific inquiry alone can solve all types of problems. For example, Haack discusses how medical science has the ability to present to us facts about when a human fetus becomes viable [20, p. 19]. Science alone, however, does not have the ability to determine whether or not abortion is morally permissible, a problem that is predominantly tackled by philosophers and ethicists. There is a danger here too when scientists allow “their ethical or political convictions to affect their judgment of the evidence, or when they present those ethical or political convictions as if they were scientific results” [20, p. 20]. Just how often this occurs would be difficult to determine. But it is a mistake to believe that science is wholly factual, completely objective, and value-free, as Kuhn rightly asserted (although the branching off of the Sociology of Scientific Knowledge is a poor interpretation of this). Science and scientific method are capable of supplying us with information about “the relation of means to ends, but cannot by themselves tell us what ends are desirable” [20, p. 20].

4.6 Denigrating the Non-scientific

The denigration of disciplines that are understood to be ‘non-scientific’ is increasingly pervasive. It is a sign of scientism, asserts Haack, to assume that “advances in the sciences will eventually displace the need for any other kind of inquiry” [20, p. 23]. This scientistic attitude can be seen clearly in many remarks by some of the most well respected physicists, as I have mentioned previously. Steven Weinberg has an entire chapter in his book *Dreams of a Final Theory* titled “Against Philosophy” that is largely a dismissal of philosophy as a discipline. There is some irony however in that his entire book is an exercise in that which he denigrates often—he is in fact doing philosophy. Weinberg takes issue in particular with the philosophy of science, this being ironic again as *Dreams of a Final Theory* necessarily required a philosophical framework for Weinberg to make his assertions regarding the nature of science [68]. Weinberg places himself firmly in the camp of the realists, a philosophical position with many advantages and limitations [60]. Richard Feynman, who is famously credited with the quote that “philosophy of science is about as useful to scientists as ornithology is to birds” [64], is another vehement critic of philosophy and in particular, philosophy of science. Massimo Pigliucci, a notable philosopher of science, called Feynman’s remark as “encapsulating both scientistic arrogance toward a non-scientific discipline and a pernicious misunderstanding that many scientists have about philosophy” [50]. These types of comments (by Hawking, Weinberg, etc.) are damaging because when highly respected physicists offer generally ill-informed judgments about philosophy, scientistic attitudes entrench themselves further into the community of physics. Well-respected physicists such as Feynman are undoubtedly individuals who carry a certain amount of scientific authority and as such their evaluations are highly regarded and subsequently spread throughout the scientific community. But these evaluations are not constrained to the realm of physics; indeed, such evaluation by a

member of a community with scientific authority has resounding effects in many other areas of science.

4.7 Deference, Respect, and Anti-Science

The line between undue deference toward science and scientific method and appropriate respect is certainly a fine one. I want to be painstakingly clear in that remaining skeptical about science, scientific method, and its epistemic value is very different from having an anti-science attitude. Often skepticism regarding science and the label of ‘anti-science’ are incorrectly conflated. What is important here is to guard against an anti-science attitude while maintaining a healthy skepticism regarding science and its claims. It is *not* scientific to integrate well-conducted empirical studies into discussions about, for example, governmental policy. It *is* scientific, however, to automatically value information with the label of ‘scientific’ as being more epistemically credible. Unfortunately even discussing the limitations of the scientific approach can be viewed as taking an anti-scientific stance. Haack’s “Six Signs of Scientism” runs the risk of being misconstrued to serve the ‘anti-science’ camp, something I am sure Haack would disapprove of as her book is clearly in defence of science. There is robust discussion surrounding science and anti-science [21] that I shall not delve into here, as it deserves a thorough treatment that I will not provide. But science is best done when it is open to rational and relevant criticism, and the objection to scientific attitudes is both a rational and relevant criticism. And for the purposes of my overall argument that philosophers and physicists should be collaborating, I discuss scientism partially because philosophers can be defenders of science. Many philosophers see philosophy of science as serving this purpose; Massimo Pigliucci, a philosopher of science with doctorates in philosophy, genetics, and biology, claims that philosophy can serve “as a crucial simultaneous watchdog and defender of science in the public arena” [50].

But primarily, I discuss scientism because scientific attitudes block other lines of inquiry that may be fruitful, and narrows the narrative to a single position. Specifically, scientific attitudes discourage philosophical inquiry into areas of science where a philosophical approach is required (a position that I will ‘cash out’ later). And we are only able to mitigate scientific attitudes if we can identify them and we have a vested interest in mitigating these attitudes because they block philosophers and physicists from listening to one another. And as I shall argue, philosophers and physicists need to be collaborating because when they do not it can negatively affect the trajectory of science.

Chapter 5

History of String Theory

The first steps in string theory were taken in the 1960s, alongside the monumental discoveries being made in the standard model of particle physics. In 1968 an Italian physicist named Gabriele Veneziano developed a new formula based on data collected during strongly interacting particle collisions [59, p. 103]. By 1970, physicists in the United States and Europe had fleshed out the formula giving it a physical framework that represented particles not as points, but as ‘stringlike’ rubber bands that existed in a single dimension. These ‘strings’ stretched and contracted relative to energy gained and lost respectively, and vibrated. It was postulated that the various states of vibration corresponded to different kinds of particles. The theory was first called *rubber band theory* but was later changed to *string theory* as the former lacked a certain elegance [59, p. 103]. The ‘stringlike’ interpretation was independently developed by several physicists such as Yoichiro Nambu, Holger Nielsen, and Leonard Susskind, but received a cold reception from the general physics community [59, p. 103]. But as string theory remained a theory of the strongly interacting particles, work in the standard model appeared to supplant string theory as strongly interacting particles were very successfully described by a gauge field [59, p. 103]. A gauge theory is a type of field theory with inherent symmetries that allow for the dynamics of a physical system to stay the

same under certain transformations. In the standard model, the forces (electromagnetism, strong nuclear force, and weak nuclear force) are explained in terms of gauge theories, the forces being conveyed through particles called gauge bosons. The gauge bosons for three of the four fundamental forces have been observed. The gauge bosons for electromagnetism, the strong nuclear force, and the weak nuclear force are the photon, the gluon, and Z , W^+ , and W^- bosons respectively. The postulated boson for the force of gravity is the graviton, although it has yet to be observed. String theory, however, allows for gravity to be explained as a gauge theory, and the existence of the graviton is implied by the theory. This is one of string theory's unique advantages [59, p. 103].

The notion of symmetry is of particular importance here, and is defined in terms of invariance “under a specified group of transformations” [10], specified (mathematical) operations being, for example, reflections, rotations, and translations. Spontaneous symmetry breaking is the spontaneous movement of a physical system in a symmetrical state (where we have a symmetry of the equations of motion) into an asymmetrical state. Symmetry is spontaneously broken when some parameter reaches a critical value so that the “lowest energy solution respecting the symmetry of the theory ceases to be stable under small perturbations and new asymmetric (but stable) lowest energy solutions appear” [10]. Spontaneous symmetry breaking accounts for the differences between the four fundamental forces and for the lack of infinite ranges of some forces (the weak nuclear force in particular has a finite range) [59, p. 60].

Although the standard model appeared to make string theory obsolete in that it described the interactions of strongly interacting particles, the concept of strings banding between quarks could still be applied in some cases [59, p. 104]. The difficulty at this point in the development of the theory was combining string theory both with general relativity and quantum theory and this early attempt at

unification could only be successful if we allowed string theory some strange qualities. String theory required twenty-five dimensions of space, a faster-than-light particle called a tachyon, and massless particles, “particles that could not be brought to rest” [59, p. 105]. Smolin wondered “why it is that the theory was not just abandoned then and there is one of the greatest mysteries of science” [59, p. 105]. The postulated existence of twenty-five dimensions provided a unique difficulty in the comprehension, plausibility, and testability of dimensions outside of the four dimensions we experience. Tachyons had never been observed and appeared to be inconsistent with Einstein’s determination that the speed of light is a universal speed limit, and strongly interacting massless particles were not known to exist. The final problem was that this preliminary version of string theory did not contain fermions and bosons, which was a major problem for a theory of strong interactions [59, p. 105]. In order to solve three of the four aforementioned problems, theorist Pierre Ramond, in 1970, postulated a way to alter the equations in the theory to include fermions, and integrated a new symmetry that “would mix the old particles with the new ones— that is, it would mix bosons with fermions” [59, p. 105]. This new symmetry opened the door to a supersymmetric string theory that eliminated the need for tachyons and reduced the number of required dimensions from twenty-five to just nine [59, p. 105]. The postulation of nine dimensions still presents very difficult interpretational issues, but it is certainly more simple than twenty-five. When time was integrated, superstring theory grew to ten dimensions. Another version of string theory that contained fermions was developed around the same time by Andrei Neveu and John Schwarz, that was also free from tachyons and postulated nine dimensions. Neveu and Schwarz also developed superstring interactions that “were consistent with the principles of quantum mechanics and special relativity” [59, p. 106].

But aside from the successes, superstring theory still had to contend with

strongly interacting massless particles. This was solved by Neveu and Joel Scherk (a French physicist) in 1972 when they found that the supersymmetric theory had vibrational states that corresponded to gauge bosons [59, p. 106]. In 1974, Scherk and Schwarz found that a number of the predicted massless particles in the supersymmetric theory could in fact be gravitons [59, p. 106]. Thus, it appeared that a superstring theory could have both gauge bosons and gravitons. The significance of this is that, as Scherk and Schwarz postulated, instead of superstring theory being a theory only of strongly interacting particles, it could instead be a fundamental theory, an attempt to unify gravity with the other forces [59, p. 106].

Strings as a fundamental unit of matter could be both open and closed, a closed string forming a loop and an open string forming a line. Photons could come from vibrations of either open or closed strings and gravitons could come only from vibrations of closed strings [59, p. 106]. If we assert that open strings represent charged particles, then one end of the string would represent a positively charged particle, and the other end would represent a negatively charged particle. The photon is then described in terms of the massless vibration of the string between either end of an open string, the photon carrying the electrical forces between the particle and antiparticle [59, p. 107]. This is a description of not only particles but also of forces and if “designed cleverly enough, it [the theory] can produce all the forces and all the particles of the standard model” [59, p. 107]. The inclusion of closed strings enabled us to postulate the existence of gravitons, thus integrating the gravitational force into our unification. Not only are we able to postulate the inclusion of gravitons, but within the parameters of superstring theory, the inclusion of gravitons is indeed *necessary* [59, p. 107]. This is due to the collisions of particles and antiparticles. Such collisions create photons through their annihilation (the act of annihilation being described by the closing of the two ends of an open string to form a closed string). This closed string gives us the graviton. Thus, superstring

theory could be made to be consistent with relativity, and out of necessity integrates the force of gravity [59, p. 107]. Herein lies some of the elegance of the proposed theory.

However, this apparent success is contingent on the postulated behaviour of strings. It must be the case that there is some law that determines whether or not strings will join or break. In string theory, laws of motion determine laws of forces [59, p. 107]. Usually the motion of particles and the fundamental forces are considered separately, although that is not the case in string theory. This is because “all forces in string theory have the same simple origin— they come from the breaking and joining of strings” [59, p. 107]. As such, forces can be described in terms of the joining and breaking of strings, a simple explanation that unifies both force and motion. That forces can be described in this way enables string theory to drastically decrease the amount of constants required to describe the system. This is not the case when we conceive of particles as points, which leads to a high number of constants [59, p. 108]. In string theory, we need only two constants to properly describe the system, one being *string tension* and the other being the *string coupling constant*. String tension is the description of the amount of energy that is contained within one unit-length of string [59, p. 108]. The string coupling constant is “a number denoting the probability of a string breaking into two strings, thus giving rise to a force” [59, p. 108]. The string coupling constant is not however a free constant and is rather dependent on the solution of the theory, meaning that the probability that a string will break and join is dependent on the particular environment [59, p. 109]. The *duality of strings and fields* was described by physicist Holger Nielsen and further developed by physicist Kenneth Wilson [59, p. 110]. The duality of strings and fields proposed that strings are quantized lines of electric flux, these lines later being referred to as ‘Wilson lines.’ The development of the duality of strings and fields suggested a duality of descriptions where “one can

think of the field lines as the primary object and the basic laws as describing how they stretch and move, or one can think of the field as primary and the field lines just as a convenient way to describe the field” [59, p. 111].

At face value, string theory seemed to be the best current proposal for a physical unified theory-of-everything. Not only did string theory describe all of the elementary particles, but it also unified the forces (all stemming from the vibrations of strings). It also incorporated gauge fields, which could be described by the vibrations of open strings, as well as integrating gravity through the necessity of gravitons (which come from the vibrations of closed strings). Thus gravity, which had been the most ‘resistant’ of the forces to be unified into one consistent physical picture, could be integrated. Finally, the introduction of the notion of supersymmetry (SUSY) allowed for the unification of bosons and fermions, “thus unifying all the forces with all the particles” [59, p. 112]. It is not difficult to see why superstring theory looked so appealing from the seemingly simple postulation of vibrating strings. But at this juncture in string theory’s history, the mathematical formalism had not quite caught up with the theorization. String theory had received a cold reception from the physics community, and it wasn’t until the ‘first superstring revolution’ that string theory came into the limelight.

5.1 The First Revolution

The 1980s saw an explosion of interest in superstring theory. The impetus for the first revolution came from a mathematical discovery made by Schwarz that seemed to provide “strong evidence that string theory was a finite and consistent theory” [59, p. 114]. Specifically, Schwarz and Green showed that a particular kind of anomaly was absent in a supersymmetric string theory with at least ten dimensions [59, p. 114]. From this point on in superstring theory’s development, interest expanded rapidly. String theory quickly dominated the market share in physics.

And as Smolin describes, there was “an air of triumphant celebration” [59, p. 116] as many physicists began to believe that string theory was the final answer to unification in physics. But Smolin also describes how it appeared as though the issue of experimental confirmation and testability for string theory was seldom discussed and often ignored and that “*mathematics* [my emphasis] now sufficed to explore the laws of nature. We had entered the period of postmodern physics” [59, p. 116]. What Smolin refers to as ‘postmodern physics’ is not strictly defined by him. What I can surmise however is that Smolin is referring to a movement in physics wherein certain scientific theories could not be strictly evaluated in terms of observation reports and empirical confirmation, but rather relied more heavily (and sometimes nearly exclusively) on mathematical formalism. There seems to be some irony in Smolin’s comment, likely referring to Postmodernism (a movement described as being marked by skepticism and a high degree of relativism—for example, moral and epistemological relativism). The subtle movement away from reliance on empirical testability and observation and into reliance on mathematical formalism dissuaded some physicists from working on string theory, but was inconsequential to others.

But string theory was not free of theoretical difficulties. A crucial obstacle that plagued string theory but was only now coming to light was the apparent lack of uniqueness. Working within ten dimensions, it was discovered that there were five consistent superstring theories [59, p. 117]. Thus, it appeared as though string theory was not a single consistent theory, but rather a collection of many consistent theories. The addition of six extra dimensions to our recognizable four dimensions (with a total of ten) led us to the problem of nonuniqueness because there are several ways to wrap up the extra dimensions [59, p. 119]. Since there are so many ways to wrap up the extra dimensions, we are left with many different formulations of string theory. And each different formulation had different physical characteristics

as well, and thus different physical predictions [59, p. 119]. The complicated nature of a six-dimensional sub-space required many different constants “which were freely specifiable” [59, p. 119], leaving us with a multiplicity of constants. But the six extra dimensions enabled us to account for the diversity of particles and forces. The complicated space of six dimensions allow strings to move in a variety of different ways and each of these different ways can describe a different particle (with different vibrations) [59, p. 121]. Thus, “we get a natural explanation for the apparent differences among the particles, something a good unified theory must do” [59, p. 121]. But this comes at the cost of nonuniqueness. We end up ‘trading’ constants in the standard model that represent particle mass and force strength for constants that describe the geometry of the six dimensions [59, p. 121]. Freely varied constants in the standard model were ‘translated’ into freely varied constants in string theory, which drastically increased the number of possible constants [59, p. 121].

At this juncture, there remained an air of optimism that physics could be unified with so simple a theory. But string theory still had many problems. If string theory was to be a unified theory for all of physics, it would be necessary for the theory to explain all of the twenty constants that appeared in the standard model. String theory required only two constants, string tension and the string coupling constant. So all twenty constants in the standard model would have to be explained in terms of string tension and the string coupling constant [59, p. 118]. It also appeared as though string theory came along with some ‘extra baggage.’ In order to make string theory mathematically and theoretically consistent, several features were necessary. Supersymmetry is one of those necessary features. Although there exist formulations of string theory without supersymmetry, the inclusion of supersymmetry eliminated the necessity of the tachyon. But supersymmetry required the existence of nine spatial dimensions, far outside of our experience with only four dimensions. So in order for a physical theory to postulate the many extra

dimensions, physicists had to find a way to ‘hide’ these extra dimensions to coincide with our experience of the world, as only four dimensions are macroscopically measurable.

The ability to ‘hide’ the extra dimensions came from a 1985 paper written by Philip Candelas, Gary Horowitz, Andrew Strominger, and Edward Witten, which seemed to show that “the conditions needed for string theory to reproduce a version of the supersymmetric standard model were the same as the conditions that defined a Calabi-Yau space” [59, p. 123]. A Calabi-Yau space is a particular six-dimensional geometry that is used in string theory to preserve the ‘handedness’ of particles that is seen in nature (as nature is not perfectly symmetrical). Candelas, Horowitz, Strominger, and Witten, proposed that the extra six dimensions could be described in terms of a Calabi-Yau space, this being done by asserting that the extra dimensions are ‘wrapped up’ in this particular kind of geometric space. Thus, we now had a way to account for the six extra dimensions through compactification [15, p. 12]. This proposal provided more structure to a physical theory that was ballooning into a theoretical monster and at the very least somewhat lessened the problem of nonuniqueness. So the landscape of possibilities of theories that could be considered a description of string theory became very so slightly smaller—but not significantly. Although string theory could now be narrowed down through the use of Calabi-Yau spaces, the number of possible Calabi-Yau spaces was enormous. In fact, there was a multiplicity of Calabi-Yau spaces, Shing-tung Yau himself stating that “there were at least a hundred thousand” [59, p. 123]. Each of these spaces provided us with different versions of particle physics, different lists of free constants, provided no new predictions, and also required new forces, many of them with infinite ranges [59, p. 123]. Thus it seems that again, string theory was running into the problem of a severe form of nonuniqueness. This problem worsened when Strominger “discovered a way to construct a vast number of additional

supersymmetric string theories” [59, p. 124], Strominger claiming that indeed “all predictive power seems to have been lost” [59, p. 124].

As Smolin has suggested, “theorists came down strongly on either side” [59, p. 124]. Superstring theory contained a fair amount of explanatory power as an attempt at unifying physics, but an equal if not larger amount of serious problems. Many physicists were dismayed that much of string theory had no foundation in experiment [59, p. 125]. The theory’s apparent lack of uniqueness posed a problem for our canonical understanding of science wherein a theory should be unique and make unique predictions. The problem of uniqueness led many theorists to the conclusion that rather than string theory *being* a fundamental theory, it was instead *leading us toward* a fundamental theory. So maybe instead of studying the fundamental theory itself we were studying the solutions to a deeper and unknown theoretical structure.

At this point, the many constructions of string theory were background-dependent theories. Einstein’s general theory of relativity showed us that “there is no fixed-background geometry for space and time” [59, p. 54]. Background-independent theories follow this insight by not assuming a set background. Background-dependent theories however follow a more Newtonian conception with is a fixed background of space and time. According to Smolin, the development of string theory worked predominantly within the context of background-dependence, an avenue, thought Smolin, that was inherently flawed [59, p. 54]. And what fell out of the supposition that instead of string theory being a fundamental theory, there existed a deeper underlying theory, is that “since the various approximate string theories live on different spacetime backgrounds [are background-dependent], the theory that unifies them *must not live on any spacetime background*” [59, p. 126]. Thus the unification of the myriad of background-dependent theories had to be a single *background-independent*

theory.

By the 1990s, progress in string theory was slowing. String theorists had succeeded in formulating “hundreds of thousands of distinct theories, each with many free constants” [59, p. 128], but had not formulated one unique string theory. The myriad of versions could not easily be connected with reality, so ultimately there was no way of knowing whether or not string theory was accurate. But the most daunting problem was that “there was not a single prediction made that might be confirmed or falsified by a doable experiment” [59, p. 128]. String theory needed something more. And that ‘something more’ came during ‘the second superstring revolution.’

5.2 The Second Revolution

By the early 1990s string theory was in a state of disarray, with five superstring theories in ten dimensions, and a multiplicity of variations with compactified dimensions. The search for a single unified theory that lay in the background of the many, many formulations of string theory found success in 1995, Smolin calling it the “second superstring revolution” [59, p. 129]. ‘Duality symmetries’ between the many different formulations of string theory became apparent. Dual theories “are exactly equivalent concerning their observational signatures, though they are constructed quite differently and may involve different types of elementary objects and different topological scenarios” [15, p. 14]. Some physicists believed that the duality symmetries between the different formulations of string theory pointed to a single underlying formulation. A simple example of a duality relation is T-duality, ‘T’ for ‘topological’ (having to do with the topology of space). Recall that string theory calls for the compactification of extra dimensions. This is represented by closed strings that are wrapped around the compactified dimensions like a rubber band around a cylinder [15, p. 14]. In accordance with the basic principles of

quantum mechanics, momenta can only be represented as discrete quantized eigenvalues and as such the state of a closed string can be described by two discrete numbers [15, p. 14]. These two numbers are the winding number, the number of times a string is wrapped around a compactified dimension, and the eigenvalue of its momentum state [15, p. 14]. It was discovered that the momentum eigenvalue and winding number were two descriptions that would “give identical physics” [15, p. 14]. There are other types of dualities, dualities becoming a unique and interesting new feature of string theory.

Strong-weak duality is another kind of duality. Strong-weak duality (S-duality) relies on the coupling constant g , which describes the interaction strength of a string using the probability that the string will break apart or join another string. S-duality postulates that theories with strong coupling constants are theoretically identical to theories with weak coupling constants [59, p. 131]. Although the two theories have different coupling constants (g versus $\frac{1}{g}$), it is asserted that they can give identical physics if both theories have additional strings. These additional strings are emergent strings, and only exist as an emergent property of complex physical systems. These chaotic systems require us to look at the properties of large collections of strings, rather than looking at particular strings, which in these large complex physical systems becomes very difficult. New strings are supposed to come out of the chaotic framework, “the behaviour of these emergent strings the exact opposite of that of ordinary strings” [59, p. 132]. Ordinary and emergent strings act inversely to one another so that increased interaction of ordinary strings results in decreased interaction of emergent strings [59, p. 132]. An odd yet convenient feature of strong-weak duality is that differentiation between ordinary and emergent strings is in most cases impossible, and in fact we can uniformly substitute ordinary strings with emergent strings (and the inverse) to get the same physical picture [59, p. 133].

T-duality and S-duality expressed relations between particular pairs of the five superstring theories [59, p. 133]. The relations however could only be derived from specially constrained states (using certain symmetries) of the pair theories [59, p. 133]. More ‘optimistic’ theorists began positing that these constrained symmetries could be extended to describe all five superstring theories, rather than describing only the specially constrained cases. As such, the five superstring theories, instead of being distinct theories, could collapse into one theory with different descriptions, creating a unification. However, if the ‘optimists’ were wrong, then string theory still remained to be a massive collection of theories with no unification, no fundamental theory, and no uniqueness.

These new-found dualities led Edward Witten to the supposition that an extra dimension was required. Witten posited a supersymmetric eleven-dimensional world with supergravity [59, p. 135]. This was inspired by a previous theory called the eleven-dimensional supermembrane theory that is described by “two-dimensional surfaces moving in an eleven-dimensional spacetime” [59, p. 135]. This supermembrane theory, which was introduced in the 1980s, was disregarded because at the time of its conception it could not be made consistent with quantum theory [59, p. 135]. But Witten (alongside other string theorists) noticed several features of supermembrane theory that could potentially help with current problems in string theory. Within the context of supermembrane theory, if one of the eleven dimensions is a circle, it enables us to wrap one of the dimensions around that circle, which leaves “the other dimension of the membrane free to move in the remaining nine dimensions of space” [59, p. 135]. Witten discovered that by wrapping up one of the dimensions in different ways he could derive all five superstring theories *and no others*. We also find dualities within the theory, but these dualities are exact ones, not approximate as they were in previous formulations. The strong-weak dualities that were previously postulated (and were only proven within special cases) could

now be confirmed to come from transformations within this new supermembrane theory [59, p. 135]. Witten labelled the new formulation M-theory.

The invention of M-theory led physicist Joseph Polchinski to posit that string theory may not only be a theory of strings, but also of other types of things. Polchinski postulated that alongside strings, string theory included two-dimensional dynamical surfaces that moved in the background space called *D-branes*. Branes, due to their electric and magnetic charges, could function as places where open strings could end. The addition of branes in string theory granted a certain robustness that greatly increased “the number of background geometries where a string could live” [59, p. 137]. This allowed us to wrap branes around compactified dimensions as well as around loops and surfaces in the geometric space. This enabled us to construct “an infinite number of possible backgrounds” [59, p. 137]. The addition of branes also enriched the symmetries in the theory, allowing new symmetries to appear as we increase the number of branes. Polchinski also discovered three-dimensional branes that, when stacked, constructed a three-dimensional world with various symmetries [59, p. 138]. This led several physicists to the notion that our three-dimensional universe is actually a brane world, the brane being a subspace on which our three-dimensional world is constrained in the context of a much larger space (with the additional dimensions). Branes also described certain constrained black holes within string theory, discovered by Andrew Strominger and Cumrun Vafa in 1996 [59, p. 138]. And Juan Maldacena, in the fall of 1977, published a paper on a new duality that postulated that a gauge theory could provide a dual description to string theory [59, p. 141].

The second string theory revolution, also referred to as the Duality Revolution, was largely marked by different proposals of string theory’s dual descriptions. This revolution pointed out many different paths that string theory might follow, but

gave no clear indication of which pathway would lead to a true description of our universe. As Smolin asserted, “we were either making fast progress toward the theory of everything, or we were off on a wild-goose chase, unwisely overinterpreting results, always taking the most optimistic reading from the calculations we were able to do” [59, p. 146]. The key problem that arose out of this revolution, according to Smolin, was how to make M-theory consistent with both background-independence and quantum theory [59, p. 146]. But most progress in making M-theory consistent with background-independence and quantum theory was hindered because most models were background-dependent [59, p. 147]. Regarding M-theory, Smolin stated that “in the absence of a real formulation, it is not really a theory—it is a conjecture about a theory we would love to believe in” [59, p. 147].

5.3 A Theory of Anything

At the outset of string theory, the search was on for a unique theory that made unique predictions. But the first and second string theory revolutions, rather than narrowing the focus to a single theory, presented us with a vast expanse of theories without a strong indication of which theory would represent reality. And in both revolutions, “observation played almost no role” [59, p. 149], pushing string theory farther away from a canonical understanding of science. But the discovery of dark energy in 1998 had quite a dramatic effect on string theory. Although it had been previously established that the universe is expanding (by Edwin Hubble in 1929), the discovery of dark energy appeared to imply that not only was the universe expanding, it was accelerating [59, p. 150]. The discovery of dark energy was a problem for string theory because string theory did not predict dark energy and worse, the detected value of dark energy was “very hard for string theory to accommodate” [59, p. 150]. Dark energy was very difficult for string theory to accommodate because string theory predicted that the value must be zero or a small

negative number [59, p. 153]. Dark energy is postulated to be a *cosmological constant*, a universal-value energy that is a feature of the entire universe. A positive value would result in an accelerating expansion of the universe. But quantum theory predicted a massive cosmological constant based on the uncertainty principle and ground-state energies. The uncertainty principle disallows simultaneous definite values for both position and momentum, meaning that particles at a temperature of zero maintain movement (this is referred to as a ground-state energy). Quantum field theories predict a massive vacuum (ground-state) energy and thus a massive cosmological constant [59, p. 152]. But this prediction is clearly not the case “because it implies that the universe would have expanded so fast that no structure at all could have formed” [59, p. 152]. This prediction has been hailed as possibly the worst prediction in physics [23, p. 187] and Smolin states that “a reasonable person could take the view that a radically new idea is needed and that no progress can be made in the unification of gravity and quantum theory until this discrepancy [between the quantum and observed cosmological constant] is explained” [59, p. 153]. And until it was discovered that the universe was accelerating, it was widely believed that the cosmological constant was zero (or a minuscule number), string theory seeming to show that the cosmological constant could only be zero or possibly a negative number [59, p. 153]. As such, the discovery of a positive cosmological constant created “a genuine crisis, because there appeared to be a clear disagreement between observation and a prediction of string theory” [59, p. 154].

But it seemed that string theory could still be saved in the face of this cosmological crisis. The resolution came from a solution to a different but yet unsolved problem in string theory, how to make the higher dimensions stable. In order for a string theory to theoretically represent our world, the higher dimensions must be stabilized, as without stabilization our universe collapses into either a

singularity, or a rapid expansion that makes the compactified dimensions as large as the observable dimensions [59, p. 155]. This problem is referred to as the *problem of moduli stabilization*. The solution came from the use of discrete branes in order to stabilize the higher dimensions. Because branes carry both magnetic and electric charges, they give “rise to discrete units of electric and magnetic flux” [59, p. 155]. A breakthrough was made in 2003 by a group of physicists at Stanford that stabilized the higher dimensions but also made string theory consistent with the observations of dark energy. Starting with a four-dimensional spacetime and one of the six-dimensional Calabi-Yau spaces, we can wrap “large numbers of electric and magnetic fluxes around the six-dimensional spaces over each point” [59, p. 156]. Since the electric and magnetic flux only come in discrete units this can ‘freeze’ the apparent instabilities and with the combination of certain quantum effects (that are understood in the context of a supersymmetric gauge theory), further stabilize the geometry in which all moduli are stable [59, p. 156]. The Stanford group then introduced ‘antibranes’ that wrapped around the geometry resulting in moduli stabilization and a positive cosmological constant. The introduction of ‘antibranes’ in order to stabilize the higher dimensions seemed to circumvent the problem of a positive cosmological constant. But string theory still had to be made consistent with quantum theory and the methods available to us were inadequate to overcome such a challenge. The best that was available, asserts Smolin, was to “apply tests, which give us *necessary but insufficient* conditions for good string theories to exist” [59, p. 157] and with these tests we could determine which versions of string theory pass our criteria. The answer to this depends on the value of the cosmological constant we wish to see, with a negative or zero cosmological constant eliciting an infinite number of distinct theories and a positive cosmological constant eliciting 10^{500} distinct theories (each of these theories making different predictions regarding the elementary particles and values of the parameters of the standard model).

Thus we are left with a multiplicity of versions of string theory. This discovery introduced the concept of a string theory landscape which posited that instead of a single theory, string theory may instead be a landscape of many possible theories. The idea of a landscape of string theories had been proposed in the late 1980s and early 1990s but experienced a cold reception [59, p. 158]. But this revitalized version received a much warmer reception based on its ability to solve the problem of moduli stabilization as well as being consistent with the observations of a positive cosmological constant. The problem now, asserts Smolin, “was no longer how to find a unique theory but how to do physics with such a huge collection of theories” [59, p. 158]. There is a question as to whether or not it is even possible to do physics with a collection of theories. It is also a question whether or not the multiplicity of theories that arise in the landscape model “exist mathematically, let alone physically” [59, p. 159].

A proposed solution to the landscape of string theories is the anthropic solution. If we take it to be the case that there is a landscape of string theories, a multiverse, then there exist all possible values of the cosmological constant. The anthropic principle is born out of the observation that it appears as though the physical conditions of the universe were finely tuned to allow for the existence of beings like us. Physical constants (such as gravitation and the speed of light) and conditions (such as the presence of oxygen and water) appear in very narrow ranges that have allowed beings like us to develop. But how did such a miraculous thing happen? The strong anthropic principle claims that the universe was finely-tuned in order to provide an environment for life to flourish. The strong anthropic principle is decidedly theistic. The weak anthropic principle takes the existence of things like us as a matter of luck. If we look to the variety of planets with very different parameters, we can infer that it just so happened that the particular conditions of our planets allowed for us to evolve. Thus, “given that we evolved, we would of

course *find ourselves* on a world that was hospitable to us” [47]. The anthropic solution merely states that the multiverse allows for the existence of a small cosmological constant (required to coincide with observation) because there is a “high probability [the landscape] will populate one or more regions of space with an anthropically favorable vacuum” [61]. The anthropic principle and its use is a philosophical argument, and is an extremely controversial assertion, tending to harshly divide physicists and philosophers between two sides. Further discussion on this is available in *The Trouble with Physics*. But the nonuniqueness of string theory remains to be solved and persists as one of string theory’s largest problems. And it may be the case that the reason string theory is nonunique, lacks internal consistency, does not make predictions, and cannot be experimentally verified is because string theory is just wrong. It is possible that string theory is formed with “some basic misconception built into it” [47]. This is an obvious possibility that many physicists and philosophers seem to have a difficult time acknowledging.

I shall end this section of the history of string theory with a discussion of some of the features that string theory possesses.

1. String theory does not have a complete and unique formulation. It is well-established that there is in fact a multiplicity of string theories rather than a single formulation. It is also the case that string theory remains to be theoretically complete (theoretical completeness referring to a consistent internal framework).
2. The most robust and consistent versions of string theory are those that are background-dependent. However, many of them appear contrary to our universe with features like unbroken symmetries, unobserved fermion and boson superpartners of equal mass, as well as unobserved “infinite-range forces in addition to gravity and electromagnetism” [59, p. 180].
3. String theory does appear to unify particles and forces. Within certain

formulations of the theory, the different oscillations of strings appear to correspond to all known kinds of forces and matter. Gravity is integrated through the oscillation mode of loops (closed strings) that give rise to the graviton. The photon, which carries the electromagnetic force, also arises out of string vibrations. String theory also predicts *similar* gauge fields to that of the strong and weak nuclear forces, although the theory does not predict the exact combination of forces we see in nature [59, p. 183]. And “on the level of the bosons” [59, p. 183] all four fundamental forces are represented by oscillation modes of the string, including, with the addition of supersymmetry, quarks, electrons, and neutrinos. As such, within constrained models, string theory can describe both particles and forces, both coming from one fundamental object, particles arises from string vibrations and forces arising from the breaking and joining of strings.

4. String theory is based on a simple law that strings must follow, that strings propagate through spacetime taking up the least amount of area [59, p. 183].
5. The problem of quantum gravity remains to be solved. The oscillation modes in string theory do appear to describe a graviton. However, “string theory is not currently formulated as a background-independent theory. This is its chief weakness as a candidate for a quantum theory of gravity” [59, p. 184]. Thus Smolin stipulates that string theory has not properly been integrated with Einstein’s well-established notion of dynamical space and time.
6. It is not currently possible to know whether or not string theory is a finite theory [59, p. 188].
7. String theory has a tendency to “get marvelous results for very special cases, and we are unable to decide whether the results extend to the whole theory or are true only of the special cases where we can do the calculations” [59, p. 190]. Descriptions of black holes, but only very particularly constrained black

holes, are key examples of this tendency. But the reliance on constrained cases in string theory is ubiquitous.

There are many other positive and negative features of string theory. As I have stated earlier, it is important to put string theory in the context of its historical development. String theory has experienced, like many scientific theories do during the period of their development, a wide variety of successes and failures. It is important to be knowledgeable of the historical development of string theory as it gives us an informed perspective regarding some of the discussions to follow. So rather than dwell on particulars of the theory, I am primarily concerned with how string theory withstands theory assessment. As I shall discuss thoroughly in the next section, string theory provides a unique perspective on the status of theory assessment in theoretical physics.

Chapter 6

Philosophy of Science: String Theory

Physics is in very much a different mood after the success of the standard model of particle physics. The development of the standard model saw intricate theoretical structures that seemed to closely resemble reality, and not soon after the development of the theories, empirical results came flooding in [15, p. 5]. These empirical results confirmed what our theories had predicted, and from it came the successful standard model.

The development of theory continued in theoretical physics. We saw the creation of grand unified theories, supersymmetry, supergravity, and string theory. Grand unified theories aim to unify various fundamental forces (weak, strong, electromagnetic). Supersymmetry posits a fundamental symmetry in particle physics that pairs each particle of the standard model with a partner particle. Supergravity utilizes the same fundamental symmetry and reconciles it with general relativity. And string theory is an attempt at unifying all interactions. But there is something fundamentally different about all of the aforementioned theories, and that is the fact that “none of them has found empirical confirmation up to now” [15, p. 6]. This is due in part to the higher and higher energies that are required to test

such theories, and outside of supersymmetry, all of the other theories “have characteristic energy levels which must be expected to lie far beyond the range of feasible collider experiments” [15, p. 6]. This results in a gradual distancing of theoretical models from empirical testing. And this has many physicists and philosophers concerned about what many perceive to be a crisis in physics, with the fear that if the connection between theory and empirical confirmation becomes too loose, “scientific progress may be suspected to slow down significantly or even come to a halt” [15, p. 6].

But Richard Dawid, a theoretical physicist and philosopher, asserts that although theoretical physics in general has moved farther and farther away from empirical testing, that “scientists often have a high degree of trust in their theories despite the lack of empirical confirmation. String theory and cosmic inflation are the prime examples in this respect” [15, p. 7]. String theory remains as one of the foremost areas of research in physics, with a large number of physicists dedicated to the theory, even though it is “arguably more detached from empirical testing than any other current theory in fundamental physics” [15, p. 7]. Dawid claims that although there remains a dearth of empirical data, there is something at work that maintains physicist’s trust in the theory that they nonetheless pursue [15, p. 7].

6.1 The Current Status of String Theory

According to Dawid, string theory remains, despite approximately four decades of intense work by a myriad of physicists and mathematicians, an incomplete theory [15, p. 17]. String theory is a complex web of interconnected descriptions “consisting of elements of rigorous mathematical analysis, of general conjectures which are based on reasoning in certain limiting cases, of modeling that is done within specified frameworks and of some approximate quantitative assessments” [15,

p. 17]. A great deal of work within the theory has provided fruitful connections (dualities) and has provided the theory with a good amount of internal structure. But connection with empirical data remains elusive and at this juncture “quantitative calculations of observables from the fundamental principles of string theory” remain out of reach [15, p. 17]. String theory remains a well-developed formalism with little connection to empirical data and the basic ‘unit,’ the string, is only empirically testable 13 orders of magnitude beyond the energy scales that the LHC is capable of reaching [15, p. 17]. As such, “there is no hope to reach those scales within the framework of collider physics” [15, p. 17]. The extra dimensions necessary for the consistency of string theory provide a similar difficulty as they are also speculated to be approximately 13 orders of magnitude beyond the capabilities of the LHC [15, p. 18]. These two core predictions of string theory (the existence of strings and extra dimensions) thus remain outside of the realm of empirical testability. Likely, “as of today, it is not possible to derive any quantitative predictions from basic principles of string physics” [15, p. 19].

It is thus well-established that string theory can only be considered as an “unconfirmed speculative hypothesis” [15, p. 19] according to the canonical understanding of theory assessment. But the actual status of string theory as an unconfirmed speculative hypothesis does not seem to mirror the status in physics that string theory enjoys. Both Dawid and Smolin discuss the unique treatment of string theory in physics as a theory that has maintained a central role in the discipline, receiving large sums of funding and having “produced the majority of the field’s top-cited papers” [15, p. 19]. But most imperative for Dawid is that string theory is a highly trusted theory in physics, regardless of the obstacle of the theory’s incompleteness and very loose connection with empirical confirmation. Dawid recognizes that although there is widespread recognition that the aforementioned obstacles need to be addressed, he claims that most physicists believe that “the

theoretical quality of string theory in itself justifies the claim that the theory constitutes an important step towards a deeper understanding of nature” [15, p. 19]. This mismatch between the recognition of the theory’s apparent flaws and the high level of trust in the theory has led to intense disputes within physics, leading to what some claim to be a deep crisis in the discipline [72].

There has been much in-fighting in physics over the current status of string theory. Dawid believes, and I wholeheartedly agree, that this in-fighting is an indicator that “something philosophically interesting is happening in physics today that is capable of creating serious divides within the physics community at a deep conceptual level” [15, p. 20]. And these divides do not stay constrained to the physics community, but spill out into philosophy, affecting the general understanding of what constitutes a theory (properly so-called). This dispute can serve as an indication that “theoretical physicists currently face a situation where philosophical considerations on the conceptual foundations of their ways of reasoning can be of interest to them” [15, p. 20]. And not only can philosophical considerations be of interest to physicists, but I shall argue that these philosophical considerations lie at the heart of the crisis in physics. Dawid has placed physicists on either of two sides of the dispute surrounding string theory and its rightful status in the physics community: on one side are those physicists who are highly influenced by string theory who seem convinced that string theory is a step toward a correct description of the world, and on the other side are physicists, often from a different theoretical field or from experimental physics, and most philosophers of physics who “consider string theory a vastly overrated speculation” [15, p. 21]. Dawid places Lee Smolin and Roger Penrose appear in the latter category, with Brian Greene and Michael Greene in the former [15, p. 21]. It is not that followers of string theory ignore the problems that lie within the theory or that critics cannot see how the theory is appealing and potentially fruitful; but rather there lies a powerful

difference between either side’s philosophical loyalties. String theory critics, claims Dawid, are “defenders of the classical empirical paradigm of theory assessment” [15, p. 22].

6.2 Canonical Understanding of Scientific Process

The canonical understanding of scientific process relies heavily on continuous empirical confirmation in order for a theory to be considered a good scientific representation of reality. During the evolution of a theoretical research program, several (if not many) theoretical alternatives may present themselves, and according to the canonical understanding it is empirical confirmation that indicates to us which of these theoretical alternatives will lead us to the ‘correct’ one, and the extended absence of empirical guidance will increase the likelihood that the theory will be a poor representation of reality. Dawid describes a general process that theoretical research programs will undergo, beginning with the development of a complete (or near-complete) theoretical picture [15, p. 22]. After a complete-to-near-complete picture is established the theory is evaluated on its internal consistency and its ability to provide quantitative predictions, the final step of the process being the execution of empirical testing (within a reasonable timeframe) in order to confirm or disconfirm the theory [15, p. 23]. The execution of empirical testing is the main indicator of which theoretical alternative will be the most accurate representation of reality, and many philosophers of science and physicists maintain that “empirical data is the only way a scientific theory can acquire the status of an acknowledged and well-established scientific theory” [15, p. 40]. Thus, what Dawid calls the ‘classical scientific paradigm’ requires both an internally coherent theoretical structure in conjunction with quantitative predictions

and empirical confirmation [15, p. 42].

But string theory, asserts Dawid, does not follow the aforementioned general process [15, p. 23], nor does it fall within the classical scientific paradigm. Proponents of string theory speculate that it may be possible that empirical confirmation of string theory will take longer than expected but may still be achieved once more of the theoretical framework is developed [15, p. 23]. But critics of string theory assert that this must relegate string theory to the status of scientific speculation rather than a successful bona fide scientific theory, and “given the large number of physicists who have worked on string theory with high intensity over the last 30 years, the theory may actually be called remarkably unsuccessful by those classical scientific standards” [15, p. 23]. So despite string theory’s appealing and interesting theoretical properties, many claim it to be “unfit to play the role of a pivotal, let alone dominating, conceptual focal point of an entire scientific discipline” [15, p. 23].

Thus it is largely the deviation from standard scientific theory evaluation that the critics of string theory are contesting. According to Dawid, Smolin goes so far to say that string theorists have set up their own set of criteria with which to evaluate the theory, with the result that string theorists overestimate their theory’s performance [15, p. 23]. An example of this is treating mathematical progress as physical progress or treating structural beauty as a sort of theoretical virtue [15, p. 23]. And the result of the creation of these “arbitrary mirages of genuine scientific success” [15, p. 23] is that objective evaluation of the theory is clouded by these ‘soft’ criteria. This results in resources being given to string theory rather than being allocated to other avenues of research that may be more fruitful. But Dawid asserts that string theorists have adopted an altered understanding of the viability of a theory that holds little connection to empirical confirmation, claiming that string theorists have strengthened the role of non-empirical theory assessment [15, p.

24].

6.3 Paradigm shift

Although slightly different from the sense that Kuhn uses it, Dawid claims that the disagreement between string theorists and critics of string theory is a paradigmatic shift between the two parties [15, p. 27]. At the base of either side are different fundamental assumptions, indeed different philosophical assumptions. Dawid posits that the paradigmatic shift between the disputants is different than the Kuhnian sense because the disagreement stems from “the meta-level of defining the notion of viable scientific argumentation” [15, p. 27] rather than stemming “directly from conceptual differences between specific scientific theories” [15, p. 27]. Simply put, critics of string theory tend to dispute methods of theory assessment rather than specific claims in the theory, although this undoubtedly occurs as well. But primarily critics of string theory are disputing issues in the philosophy of science, specifically concerning the status of scientific theories that fall outside of the canonical understanding of theory assessment. Dawid postulates that the paradigmatic shift also deviates from the Kuhnian conception in that the paradigm shift was not the result of a revolutionary period, but rather the meta-level shift in the understanding of theory assessment evolved gradually as scientists worked through and developed the theory. The gradual progression of the theory caused a subtle, unacknowledged shift “between their [the scientists] understanding and the canonical paradigm of theory assessment prevalent in other parts of physics” [15, p. 27].

Thus, Dawid is claiming that there has been in the context of string theory a philosophical shift that tends to leave the physical and philosophical community primarily on one of two sides, and that there is a lack of recognition that such a philosophical shift has occurred. This lack of recognition is at the root of the

disagreement surrounding string theory and serves to further the divide between two well-meaning parties. Without recognition of the meta-paradigmatic bases for their disagreement, critics and proponents of string theory “lead the discussion based on incompatible sets of hidden preconceptions and therefore miss each other’s point” [15, p. 28]. These are philosophical issues that lay at the foundation of the conversation, and such philosophical issues must be addressed before an evaluation of string theory can take place.

Dawid is claiming that string theorists, in the process of the development of the theory, have been led toward a different conception of theory assessment, with the result that “the scientific process itself thus has led beyond the canonical limits of scientific reasoning” [15, p. 29]. And Dawid maintains that this evolution in conceptions about theory assessment was a reasonable one and was not in fact instigated by the lack of empirical evidence present in string theory [15, p. 29]. Rather, Dawid posits that string theory itself and the context in which it has developed provide substantial support for the deviation from a canonical understanding of theory assessment [15, p. 29]. Dawid also maintains that the move by string theorists from a canonical understanding of theory assessment to a newly evolved form is a legitimate one [15, p. 29]. He claims that it is to be expected that “novel scientific input thus must be expected to alter the scientific paradigm in the future” [15, p. 29], if we take the evolution of paradigms in science to be the case. So Dawid asserts that it is to be expected that our philosophies of science, specifically our criteria for assessing scientific theories, will evolve as our theories evolve and this is done in reference to the successes and failures of scientific reasoning observed in the past [15, p. 29].

There are reasonable arguments on both sides of the paradigmatic rift. Critics of string theory see a significant deviation from traditional theory assessment, traditional theory assessment being something that historically has enabled science

to be highly successful. But the critic claims that changing our conceptions of theory assessment in light of a new scientific theory is an indication of a problem with the theory and not with the scientific framework. So it is the theory that should be discarded, revised, or should fall outside of the scientific umbrella, rather than the framework in which the theory resides. On the other side, proponents of string theory might see the continual, rigid adoption of old paradigms (old conceptions of theory assessment) to be of a disservice to the progression of science “by sticking to a misguided chimera of the static nature of scientific principles” [15, p. 30]. And Dawid is furthering the line of argument that it is the traditional paradigm of theory assessment which has led to the current crisis that fundamental physics finds itself in [15, p. 30].

6.4 Three Contextual Arguments

Dawid puts forth three contextual arguments for the viability of string theory and an explanation of why there is so much trust in a theory that lacks empirical confirmation.

The first contextual argument is the **no alternatives argument (NAA)**. String theory is a proposal of a unified theory of elementary particles and gravity. This attempt at unification is one of the more unique characteristics of string theory and is part of why the theory has been of such interest in the physical and philosophical community. However, string theory is not the only attempt at unifying general relativity and the principles of quantum mechanics; rather, string theory is a postulated solution to the question of quantum gravity. As such it is important to place string theory in the context of the larger question of quantum gravity.

Quantum gravity seeks to unify gravity with quantum mechanics, a unification that has been long sought-after but highly elusive. Gravity falls within the classical framework of Einstein’s general theory of relativity, while quantum mechanics is

demonstrably non-classical. Quantum gravity would “be able to provide a satisfactory description of the microstructure of spacetime at the so-called Planck scale at which all fundamental constants of the ingredient theories, c (the velocity of light in vacuo), \hbar (the reduced Planck’s constant), and G (Newton’s constant), come together to form units of mass, length, and time” [70]. The unification of the two often-contradictory domains is desired if we are to have a consistent scientific picture. There are several responses to the question of quantum gravity, including string theory, loop quantum gravity, and twistor theory. But Dawid maintains that string theory is unique compared to these other possible explanations to quantum gravity because the goal of string theory is to “provide a truly unified description of all natural forces” [15, p. 31] wherein “the viable results of canonical quantum gravity are put into the context of contemporary particle physics [which will] blend into the string theory research program” [15, p. 31]. So Dawid is claiming that string theory is the only viable possible solution to a grand unification, whereas other possible solutions to quantum gravity have a smaller scope. The uniqueness of string theory as the only attempt at unification, asserts Dawid, is bolstered by arguments by Polchinski [51], [52]. But the uniqueness claim is a contestable one, so Dawid finds it necessary to take all three contextual arguments in conjunction as one argument alone is not sufficient.

The second contextual argument is the **argument of unexpected explanatory coherence (UEA)**. The argument of unexpected explanatory coherence maintains that a theory that explains unexpected phenomena is more likely to be correct. Dawid asserts that string theory appears to provide many unexpected “deeper explanations of seemingly unconnected facts or theoretical concepts” [15, p. 33]. Dawid claims that the theoretical postulation of “the extendedness of elementary particles” [15, p. 33] gives rise to several surprising results. Alongside providing a possible framework for quantum gravity, string theory

also implies the existence of gravity (stemming from a particular oscillation mode), extensively develops the concept of supersymmetry (which previously was primarily a mathematical abstraction), and explains certain aspects of black hole entropy (in specially constrained string theoretic models) [15, p. 34]. These interconnections, according to Dawid, fit into a coherent whole that could not have possibly been predicted at the outset of string theory and “it would look like a miracle if all these instances of delicate coherence arose in the context of a principle that was entirely misguided” [15, p. 34]. But it is possible, admits Dawid, that the reason for these unexpected explanations could stem from a deeper and more fundamental theory than string theory while disregarding string theory entirely, although he finds it unlikely that the unexpected explanations do not hinge on string theory itself [15, p. 35]. As such, Dawid concludes that the unforeseen explanations that string theory provides allows us to further trust in the theory’s viability.

The third contextual argument is the **meta-inductive argument from the success of other theories in the research program (MIA)**. The meta-inductive argument maintains that if a proposed theory is based on previous well-developed and empirically well-established theories, then trust is higher in the proposed theory. So the success of a sufficiently similar research program lends likelihood of the success of the theory in question. Dawid uses the development of the standard model as a sufficiently similar theory that is tremendously successful [15, p. 35]. String theory and the standard model, he claims, are similar in that the standard model, although it made a myriad of interesting particle predictions, initially lacked empirical confirmation. But just as is the case in string theory, “it turned out that none of the alternatives to the standard model that physicists could think of was as satisfactory at a theoretical level” [15, p. 35], and the standard model presented new surprising interconnections [15, p. 35]. Dawid considers the standard model a ‘direct precursor’ to string theory and string theory as a natural

progression of the particle physics research program [15, p. 36]. From the success of the standard model, Dawid draws this conclusion:

Scientific theories which are developed in the research program 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. [15, p. 36]

Dawid puts forth the idea that MIA allows us, based on the methodology and success of the standard model, to conclude that a new theory that satisfies the constraints of methodology in the standard model will lead to reliable predictions in the new theory (string theory) [15, p. 36]. But the question here is whether or not string theory *really* falls within these limitations.

6.5 Scientific underdetermination

It is possible however that after the construction of a theory that makes particular predictions and fits available data there exist “other, so far unknown scientific theories which fit the present data equally well but predict different new phenomena” [15, p. 44]. As such, scientific theories run the risk of being *underdetermined* by current data, Dawid calling this notion the principle of scientific underdetermination [15, p. 45]. Underdeterminism can be categorized as a more generalized notion that considers all logical possibilities of underdetermination under “some general assumptions which are taken to be constitutive of all viable scientific research” [15, p. 45]. These general assumptions are specific to scientific fields that determine what is considered to be a proper scientific statement. There is robust philosophical literature on underdetermination by Hume, Quine, and Laudan, that I shall not discuss here. But Dawid endorses a version of the view put forth by Laudan [33] that the classical paradigm of theory assessment severely undervalues the theoretical side of theory assessment [15, p. 43]. Dawid deviates

slightly from Laudan in that he (Dawid) is focusing on assessments of *limitations* to scientific underdeterminism [15, p. 44], as unconstrained scientific underdetermination would not enable us to make correct predictions (because there would be an infinite number of viable theories to ‘pick’ from). Ultimately, scientific underdetermination posits that there might be an alternate theory that better matches the empirical data of a theory with different predictions. Because the theory is not empirically confirmed a purely theoretical research program (without empirical confirmation) cannot be trusted as a scientific theory as we could not know which ‘version’ would be the ‘correct’ one.

It might appear strange then that scientific underdetermination is applied to string theory, a theory that remains empirically unconfirmed. But Dawid maintains that although string theory itself remains to be empirically confirmed, two theories on which string theory conceptually relies on, general relativity and gauge field theory, do have empirical confirmation [15, p. 50]. Although empirical confirmation of general relativity and gauge field theory does not constitute empirical confirmation for string theory, it aids in limiting scientific underdetermination in string theory as it draws a connection between available empirical data and theory creation in string theory [15, p. 50]. This connection is a result of Dawid’s meta-inductive argument, in that theory creation in general relativity and gauge field theory is similar to that of string theory. As such, success in relativity and gauge field theory increases the likelihood that string theory will find success as well, and that string theory will *eventually* receive empirical confirmation. So if we limit scientific underdetermination, we gain the ability to fortify “the inferential connection between the empirical data that has motivated the theory’s construction and the theory itself” [15, p. 51]. But Dawid is careful to assert that non-empirical theory assessment could not conceivably *replace* empirical confirmation, nor can non-empirical theory assessment be granted the same ‘status’ as empirical

confirmation [15, p. 58]. He maintains however that the epistemic status between the two is “less fundamental than one might assume at first glance” [15, p. 58].

The crux of Dawid’s theory is that fundamental physics has entered a stage that calls for a revision of our philosophies of science. Dawid discusses several different areas of physics, like loop quantum gravity, supergravity, and cosmic inflation, that represent the necessity of this revision. Using these theories (including string theory) as examples, Dawid concludes that we have entered a stage where “empirical evidence for new theoretical conceptions is scarce or remains entirely absent for many decades” [15, p. 94]. It may be the case that empirical confirmation is not in fact possible in many circumstances. And Dawid believes that theories that have remained unconfirmed for extended periods of time, rather than being considered merely speculative, require theory assessment in their empirically unconfirmed states [15, p. 95]. This is justified to a certain extent because even in the absence of empirical confirmation, high energy physics still “is characterized by a considerable directedness of theory building” [15, p. 96], often developing intricate and highly predictive theories. String theory specifically, Dawid claims, although it is arguably the most distant from empirical confirmation of most-if-not-all theories in theoretical physics, is overwhelmingly trusted by scientists [15, p. 96]. As such, he asserts that the development of non-empirical theory assessment remains a necessary and urgent philosophical endeavour [15, p. 96]. And high energy physics provides a particular kind of environment that requires an evolved philosophy of science. Dawid maintains that high energy physics must exploit all possible kinds of theory assessment (theoretical plus empirical when possible, theoretical plus non-empirical when required) in order to progress.

6.6 Summary

Thus, what Dawid is positing is that the canonical scientific paradigm is far too narrow and does not account for the type of non-empirical theory assessment that he believes scientists already practice (UEA, MIA, NAA). Non-empirical theory confirmation is an extension of empirical theory assessment, but assessment occurs at the meta-level. Non-empirical theory assessment is a means to determine whether or not a theory might be viable and should be passed on to receive empirical testing; as such, non-empirical theory assessment is probabilistic in nature. Dawid is asserting that we already integrate non-empirical theory assessment during the actual *doing* of science and it is nonsensical to ignore that type of reasoning, particularly in scientific areas that deal with increasingly inaccessible phenomena (high energy physics). These non-empirical forms of theory assessment enable us to limit scientific underdetermination by pointing us in the direction, based on UEA, MIA, and NAA, of the theory that is more likely to be true. Scientists rely on non-empirical theory assessment to temporarily bridge the gap between theorization and empirical confirmation, empirical confirmation still remaining as the gold standard of theory confirmation. But these non-empirical standards increase the trust in a yet-to-be-empirically-confirmed theory, trust being a belief that the theory will reveal itself (upon empirical testing) to be an accurate representation of reality.

String theory is philosophically interesting because of its movement away from a canonical understanding of theory assessment. It is well established at this juncture to make the assertion that string theory certainly falls outside of the realm of canonical theory assessment. String theory possesses within it many features that confirm this assertion, such as its very loose connection with empirical data and confirmation, its dearth of prediction-making, and the overabundance of different versions of the theory (which means that we cannot intelligibly speak of one single

‘string theory’). As such it is either the case that we must disregard string theory as mere speculation rather than a legitimate scientific theory or we should ‘revolutionize’ our philosophy of science to, as Dawid claims, include non-empirical theory assessments.

Dawid posits that even though under the canonical understanding of theory assessment string theory can only be considered speculation, string theorists treat their theory as a legitimate scientific theory. It is this belief on behalf of string theorists that is an indication to Dawid that string theory is *not* mere speculation. It appears to me that Dawid is claiming that the *belief* that string theory is a real scientific theory is an indication that it *is* in fact a real scientific theory. I infer that the justification for this is based on the success of the sufficiently similar research program, in this case the standard model. Dawid wants to claim that the standard model underwent a similar process where trust in the theory was strong during the theorization period and the standard model was *eventually* empirically confirmed. As such, Dawid asserts that a similar process will occur regarding string theory. This line of argument however relies on the assumption that the development of string theory and the development of the standard model are sufficiently similar. I take issue with this claim for several reasons. First, the standard model after its theorization period had reached a high degree of internal consistency. The standard model became a near-complete theoretical picture. String theory however is admittedly (by Dawid, Smolin, and others) an incomplete theoretical picture, to a much larger degree than the standard model near its completion. Neither does string theory enjoy the same internal consistency that the standard model does. Second, the standard model offered a consistent and, most important, unique formulation. As I have discussed previously, string theory contains a myriad of different versions of the theory, severely lacking uniqueness. Third, the standard model made exact predictions, with string theory not only *not* making exact predictions but in some

cases not making predictions at all. But regardless of whether or not the standard model and string theory are sufficiently similar, it is philosophically dubious to predict that string theory will eventually receive empirical confirmation and base our current evaluation of the theory on a future-contingent proposition.

There is also the matter of testability, a notion that is often at the forefront of the conversation regarding the status of string theory. As I have discussed earlier, the basic building blocks of string theory such as the existence of other dimensions and the existence of strings, are far beyond our ability to test. The Large Hadron Collider (LHC) at CERN is the only means that we currently have to study matter at its smallest scale. However, the existence of the string cannot conceivably be verified by the LHC, now or in the foreseeable future, and the existence of extra dimensions would be incredibly difficult to verify experimentally. It *is* conceivable, however, to verify supersymmetry, which is one of string theory's most important features, as the proposed antiparticles that make up supersymmetry could be viewed. But supersymmetric partner particles have yet to reveal themselves, and the status of supersymmetry is in a dire state. But hypothetically, even if supersymmetry were to be verified at CERN, string theory would not automatically be verified as well. A verification of SUSY would not be sufficient to verify string theory, as string theory's central features still remain untestable. Dawid would likely say that this would help string theory's case, as at the very least it would empirically verify a piece of string theory's research program. Again, string theory may be given slightly more credibility and the likelihood of it being correct may increased slightly. But the reality of string theory, even beside the verification of SUSY, is that string theory is fundamentally untestable, now and perhaps always. And if string theory is untestable it is surely unfalsifiable. Both of these conclusions are problems under the canonical understanding of physics. So how should we treat a theory that can neither be verified nor falsified?

Thus we are left with several questions that as philosophers and physicists we must collaborate to work toward an answer. We must determine whether the generation of a new paradigm of theory assessment to meet the needs of string theory (and other high energy physics research programs) is a justifiable action to take. Or we must determine whether or not Dawid's conception is an *ad hoc* hypothesis of a theory that is mere speculation and should only be considered as speculation.

My purpose throughout this section was not to perform an evaluation of string theory myself, as this would require a much more in-depth explanation than is possible. Dawid's theory is of interest to me because it shows us how string theory has indeed undergone a change in its philosophies of science, whether or not such a change was a justified one. So the crux of the issue of the assessment of string theory, even if I shall not perform an evaluation of string theory myself, is that this issue *stems* from philosophical disagreement. Critics and proponents of the theory are on either sides of a philosophical rift, and this is why increased collaboration of physicists and philosophers is important—because when the disciplines are divorced, it has real effects on the trajectory of physics (and of philosophy). These are important issues and they determine the trajectory of science and we need philosophers and physicists to be working together to analyze them.

String theory leaves us in an interesting predicament. Dawid claims that it is imperative that we revolutionize our philosophy of science in order to fit string theory into our understanding of 'good' science. But an important alternative is that string theory is just wrong. It is either the case that string theory is incorrect, or our scientific methodologies are wrong or at the very least, insufficient. The more conservative approach is to conclude that string theory is wrong, although this is one of the lesser explored conclusions made regarding string theory. That this is one of the lesser explored conclusions may be for a myriad of reasons. It is difficult, as

Kuhn and Smolin discussed, to be a divergent thinker in an environment inhabited predominantly by convergent thinkers, to go against mainstream ideas. It is difficult to lay to the wayside an idea that thousands of highly intelligent people have devoted their lives to. It is also difficult to let go of a theory that aimed to be a unification of physics.

What this ‘interesting predicament’ illuminates for us is that there is a delicate balance to be struck between being conservative and being radical. We may wish to be conservative through, for example, the maintenance of preexisting philosophies of science. But we also may want to be radical by placing trust and resources into a theory that we see as being promising, like string theory. Should we throw away a theory that has enjoyed decades of intensive research because it falls outside of our canonical paradigm of theory assessment? Or should we think of philosophy of science as something that should *evolve* just as our science evolves? Neither of these questions have simple answers, and the balance between being conservative and being radical is surely an ongoing issue in physics. This balance is what Kuhn spoke of with divergent and convergent thinkers. And we can appreciate this balance when John Archibald Wheeler spoke of science as advancing by “daring conservatism” [49]. This delicate balance lies at the heart of this entire conversation around the status of string theory, but ultimately it lies at the heart of physics in its entirety.

Chapter 7

Concluding Remarks

This thesis was instigated by the comments made by scientists like Hawking, Krauss, and Feynman. But although there are several vocal physicists who denigrate philosophy, there are others who see the value in philosophical inquiry into physical problems. Sean Carroll, a cosmologist-physicist, asserts that “at its best, the practice of philosophy of physics is continuous with the practice of physics itself. Many of the best philosophers of physics were trained as physicists, and eventually realized that the problems they cared most about weren’t valued in physics departments, so they switched to philosophy” [12]. Richard Dawid is an example of such a person. Abner Shimony, who held doctorates in both physics and philosophy, was a brilliant example of someone who had deep understanding of both physics and philosophy. Shimony is known for his development of an empirically testable form of the Bell inequality, and in philosophy for his extensive work on naturalistic philosophy of science. Shimony is an exemplar of the type of interdisciplinary work that I am advocating for.

And there are those who recognize that the discipline of physics is reliant on a philosophical framework. George Ellis, a notable physicist, cosmologist, and mathematician, has stated that “you cannot do physics or cosmology without an assumed philosophical basis. You can choose not to think about that basis: it will

still be there as an unexamined foundation of what you do” [26]. Ellis has spoken against Hawking, Krauss, and Neil deGrasse Tyson (an astrophysicist and ‘science popularizer’) stating that, regarding a belief held in common by the three physicists that philosophy is a waste of time, “if they really believe this they should stop indulging in low-grade philosophy in their own writings” [26]. Carlo Rovelli, a theoretical physicist, has said that “the scientists that talk philosophy down are simply superficial: they have a philosophy (usually some ill-digested mixture of Popper and Kuhn) and think that this is the ‘true’ philosophy, and do not realize that this has limitations” [25]. As Smolin, Dawid, and others have argued, oftentimes scientists fall back on a poorly understood and articulated version of Popperian falsifiability; Kuhnian notions are often also used. So although some of the most famous leaders of physics have furthered the distance between physics and philosophy, there are those who have worked and continue to work to ‘close the gap.’

And collaboration between the two disciplines is already happening. A recent three-day conference between physicists and philosophers is an interesting example. On December 7th, 2015, top-level physicists and philosophers met in Munich to discuss in particular problems with the integration of string theory into both the scientific and philosophical picture [72]. Physicists George Ellis and Joe Silk prompted the meeting after the release of their opinion piece in *Nature* expressing concern with the current state of physics regarding the status of string theory [17]. Ellis and Silk believe that there is a “battle for the heart and soul of physics” [72]. The impetus behind this conference came predominantly from physicists such as David Gross (a Nobel Prize winner in physics) who have called for aid from experts outside of the fields of physics. The meeting was attended by philosophers such as Richard Dawid, Paul Teller, and Massimo Pigliucci. Ellis and Silk are reacting to what they see as being “a dangerous departure from the scientific method” [72].

This departure, according to Ellis and Silk, is particularly troublesome in string theory as well as multiverse models where they claim mathematical elegance and simplicity are being valued over testability, thereby “blurring the line between physics and pseudoscience” [72]. And as I have already argued, these are really discussions over current philosophy of science and how we should treat untestable theories (which are common in high energy physics). Both Ellis and Silk cite Karl Popper and his notion of falsification as an ideal tenet that good scientific theories must rest on, but Massimo Pigliucci objected to the use of falsifiability as a philosophy of science, arguing that it is fundamentally inadequate. Indeed, Pigliucci stated that physicist’s preoccupation with Popper “is really something that needs to stop” and that “we need to talk about current philosophy of science. We don’t talk about something that was current 50 years ago” [72]. But the mere fact that the conversation is being had is highly valuable. It is this type of collaboration that I advocate for and is the type of collaboration that I have argued will be necessary to solve some of the biggest problems in physics.

The notion of paradigm shift has been prevalent throughout this discussion. The term ‘paradigm shift’ was coined by Thomas Kuhn, who discussed science as moving through ‘normal’ and ‘revolutionary’ periods. Lee Smolin has asserted that physics as the discipline stands is currently in a state of revolution, and it is a different ‘mode’ of thinking that is required in order for the revolutionary period to reach completion. Dawid claims that string theory and high energy physics has already undergone a paradigm shift, which acts as the dividing force between critics and proponents of the theory. What I argue is that we require a paradigm in science, in physics in particular, because many of the deepest problems in physics (such as the foundations of quantum mechanics, the status of string theory, and the nature of time) are based on fundamentally philosophical issues. The solutions to these very intricate problems will not be solved without philosophical intervention. As such,

this paradigm shift requires the integration of the philosophical method (thus integrating philosophers and physicists) into the scientific endeavour. My viewpoint aligns fairly well with the conclusions that Smolin has made, although I am calling specifically for philosophical intervention. Smolin does not call outright for the integration of philosophers and physicists; rather, he calls for physicists to think more philosophically. Smolin asserts that we need to complete the revolutionary period that we are currently in but he does not support the idea that string theory is a positive step into a different paradigm. So clearly Smolin and Dawid have very different ideas about what ‘kind’ of new paradigm we are about to or have shifted into. But regardless of whether or not we are on the cusp of a paradigm shift or have already moved into one, it is clear that there is either a lack of understanding or recognition of the philosophical bases of the scientific method. It is this lack of understanding that has given rise to a crisis in physics surrounding the status of string theory. This is due to a difference in philosophical loyalties between critics of the theory (who generally adhere to a canonical paradigm of theory assessment) and supporters of the theory (who have, perhaps unknowingly, adopted a new paradigm of theory assessment). This serves my overall argument that it is necessary to have physicists and philosophers working together to solve some of these issues.

So what is it that philosophy can do in physics? Philosophy is not just, as A.J. Ayer said, “wholly critical” [4, p. 48] and does not serve as only “clarification and analysis” [4, p. 51]. And I disagree with the notion that philosophy can only be passively engaged by merely making descriptions of science and scientific process. Jerry Fodor describes this passive engagement wherein “the philosopher does not try to dictate how scientific inquiry and argument ought to be conducted. Instead he tries to enumerate the principles and practices that have contributed to good science” [18]. This description of the role of philosophy is far too narrow in scope and needlessly limiting. If philosophy is *only* concerned with describing how

scientific inquiry and argument is conducted, then the effectiveness of philosophy is surely limited. The description of the scientific process is not unimportant, but philosophy contributes much more than something of such a narrow scope.

Philosophy can be an active player in physics through, for example, the generation of new ideas. This is because philosophy is potentially a more receptive environment than physics for innovation, for ideas that go against mainstream ideas.

Collaboration between philosophers and physicists can encourage creative and innovative thinking. Philosophy can continue to serve in its current role as a separate discipline that studies scientific methodology and reasoning. And as I mentioned before, philosophers can actively serve as both public defenders and critics of science, as philosophers “working together with (not in opposition to) scientists, have a huge role to play in furthering societal dialogue about science” [50]. And in order for philosophers to *seriously* play an active role in the defence and criticism in physics, it is necessary for philosophers and physicists to work together.

But primarily, the lifeblood of philosophy is critical analysis and that is what philosophy can contribute to physics. In physics, philosophy is particularly concerned with the critical analysis of concepts, reasoning, and methodology. In physics, the critical analysis of concepts would include inquiries into the use of terms. For example, what is meant by ‘time’, ‘cause’, or ‘quantum system’? These terms are often used but are not often defined. In quantum theory, the usage of ill-defined terms is commonplace. John S. Bell, in his article “Against ‘measurement’,” recognized this muddling of terminology, stating that there were “some words which, however legitimate and necessary in application, have no place in a formulation with any pretension to physical precision: *system, apparatus, environment, microscopic, macroscopic, reversible, irreversible, observable, information, measurement*” [6]. It is likely the case that these terms, to this day,

remain in the same state—lacking the direct physical significance that is ascribed to them. But the words we use, what they mean, and what we believe them to mean (all three of which do not necessarily align) are important. Differences in the understanding of terms can lead to conceptual muddling. And conceptual muddling can lead to incorrect inferences and conclusions.

Philosophy is already actively engaged in the critique of reasoning and methodology in physics and this is one of philosophy’s most important contributions. Physical theories aim to follow good paths of reasoning. But it is not always the case that the actual structure of physical arguments is paid enough attention, leading to problematic argument structures, like the aforementioned allegations of circularity in physical theories, e.g., no-signalling theorems ([40], [48], [69]), and missing or hidden premises. The adherence to logical argument structure is one of philosophy’s primary concerns. Another role of philosophy is to critique the use of philosophies of science by scientists, and to point out the misuse of underlying philosophies of science (such as ill-defined uses of Popperian falsification). Through concern with how physicists construct their arguments, and the discussion of proper usage (and limitations) of philosophies of science, philosophy does indeed aim to inform how scientific inquiry and argument ought to be conducted (*pace* Fodor).

But most important of all of the roles of a philosopher is the role as a creative thinker. Philosophy actively encourages divergent lines of thinking as this is how much of philosophy is done. Philosophy proceeds through successions of conclusions and rebuttals to those conclusions. And the longevity of some of philosophy’s problems necessitates “philosophical imagination” [47] in order to develop novel ideas. The development of novel ideas is also integral to scientific inquiry. As such, we need philosophers who are willing to take on creative roles in science. As Carlo Rovelli stated, “as a physicist involved in this effort, I wish the philosophers who are

interested in the scientific description of the world would not confine themselves to commenting and polishing the present fragmentary physical theories, but would take the risk of trying to look ahead” [57, p. 182]. ‘Looking ahead’ means developing new and imaginative ways of looking at problems in physics. So we need philosophically literate scientists but we *also* need philosophers who are willing to unabashedly discuss and point out the conceptual problems in physics as well as develop creative new ways to solve them.

I have shown that several of the most daunting problems in physics require philosophical inquiry and the collaboration of both physicists and philosophers. I have discussed the notion of scientism and have shown that scientistic attitudes block potentially fruitful lines of inquiry, specifically philosophical lines of inquiry. These scientistic attitudes are exemplified in the comments by some of the most influential physicists including Hawking, Krauss, Weinberg, and Feynman. I conclude that scientism negatively affects the ability of physicists and philosophers to collaborate and as such, scientistic attitudes must be addressed and dissolved. The history of string theory has elucidated the fact that the status of string theory as a scientific theory is a highly divisive issue.

I have used string theory as a ‘canary in the coal mine’ of a larger crisis in physics. Superficial understandings of underlying philosophies have created a crisis in physics, as evidenced by string theory. Whether or not we can claim string theory has made a legitimate movement away from the canonical understanding of theory assessment, string theory has occupied a great deal of resources in the discipline of physics. I call it a crisis because the trajectory of science has inevitably been influenced by its preoccupation with string theory, thus underfunding other areas of research. What we can draw from this is that a lack of philosophical awareness can have a direct effect on the way science is done. This larger crisis in physics is focused primarily on how we should treat theories that have loose connections with

empirical confirmation. I have shown how the crisis in physics stems from different philosophical loyalties. Thus at its heart, this crisis in string theory is a philosophical problem, as is the larger crisis in physics as a whole, and as it is a philosophical problem it requires a philosophical solution. And many, if not most, scientists are *not* equipped to engage with such a philosophical problem as they are no longer philosophically engaged or trained as were the scientists-of-old (such as Einstein and Heisenberg). As such, we need philosophers and physicists to be collaborating; it is imperative that we have scientifically literate philosophers and philosophically literate scientists.

Further lines of inquiry could analyze the role that the hyperspecialization of the sciences has played in distancing philosophy from physics; I would posit that as disciplines within science have become more delineated, this delineation further increased the disadvantageous distancing between physics and philosophy. A very important line of inquiry could discover how clearly demarcated physics and philosophy really are. I have already asserted that the delineation between the two disciplines is more of a gradient than a dichotomy and a more in-depth discussion of this matter could be very useful and would be a positive contribution to this overall conversation. But first and foremost, we need to get philosophers and physicists talking.

Bibliography

- [1] Hanne Andersen and Brian Hepburn. Scientific method. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, summer 2016 edition, 2016. 26
- [2] Ross Andersen. Has physics made philosophy and religion obsolete. *The Atlantic*, 23, 2012. 2
- [3] Grant Anderson. Tamara Lovett found guilty of negligence. Available at <http://www.cbc.ca/news/canada/calgary/tamara-lovett-ryan-failing-provide-necessaries-trial-decision-1.3947694>. 44
- [4] Alfred Jules Ayer. *Language, Truth and Logic*. Dover Publications, 2014. 94
- [5] Barry Barnes, David Bloor, and John Henry. *Scientific Knowledge: A Sociological Analysis*. University of Chicago Press, 1996. 17
- [6] John Bell. Against ‘measurement’. *Physics world*, 3(8):33, 1990. 95
- [7] Alexander Bird. Thomas Kuhn. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, fall 2013 edition, 2013. 13, 14, 15, 17, 18, 19, 20, 21, 23, 24, 35
- [8] James Bogen. Theory and observation in science. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, summer 2017 edition, 2017. 5, 7, 8
- [9] Michael Boylan. Hippocrates. Available at

- <http://www.iep.utm.edu/hippocra/>. 26
- [10] Katherine Brading and Elena Castellani. Symmetry and symmetry breaking. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, spring 2013 edition, 2013. 52
- [11] Brian Harold Bransden and Charles Jean Joachain. *Quantum mechanics*. Pearson Education, 2000. 29
- [12] Sean Carroll. Physicists should stop saying silly things about philosophy. Available at <http://www.preposterousuniverse.com/blog/2014/06/23/physicists-should-stop-saying-silly-things-about-philosophy/>. 91
- [13] John Cramer. *The Quantum Handshake: Entanglement, Nonlocality and Transactions*. Springer, 2015. 29
- [14] Nicole Darman. 7 physics blunders. Available at <https://www.fiatphysica.com/blog/everyday-physics/physics-blunders-mistakes-in-the-news>. 29
- [15] Richard Dawid. *String Theory and the Scientific Method*. Cambridge University Press, 2015. 59, 61, 62, 72, 73, 74, 75, 76, 77, 78, 79, 80, 81, 82, 83, 84, 85
- [16] Albert Einstein. Physics and reality. *Journal of the Franklin Institute*, 221(3):349–382, 1936. 1
- [17] George Ellis and Joe Silk. Scientific method: Defend the integrity of physics. *Nature*, 516(7531):321–323, 2014. 92
- [18] Jerry A Fodor. *The Mind-Body Problem*. Scientific American, 1981. 94
- [19] Adolf Grünbaum. *The Foundations of Psychoanalysis: A Philosophical Critique*, volume 2. University of California Press, 1985. 7
- [20] Susan Haack. Six signs of scientism. *Logos & Episteme*, 3(1):75–95, 2012. 10, 27, 30, 37, 38, 39, 40, 41, 42, 43, 44, 45, 47, 48
- [21] Sven Ove Hansson. Science and pseudo-science. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford

- University, summer 2017 edition, 2017. 42, 49
- [22] Sam Harris. *The Moral Landscape: How Science Can Determine Human Values*. Simon and Schuster, 2011. 46
- [23] Michael Paul Hobson, George P Efstathiou, and Anthony N Lasenby. *General Relativity: An Introduction for Physicists*. Cambridge University Press, 2006. 66
- [24] Jim Holt. Physicists, stop the churlishness. *New York Times*, 10, 2012. 2
- [25] John Horgan. The philosophy of guessing has harmed physics, expert says. Available at <https://blogs.scientificamerican.com/cross-check/the-philosophy-of-guessing-has-harmed-physics-expert-says/>. 92
- [26] John Horgan. Physicist George Ellis knocks physicists for knocking philosophy, falsification, free will. Available at <https://blogs.scientificamerican.com/cross-check/physicist-george-ellis-knocks-physics-for-knocking-philosophy-falsification>. 92
- [27] Don A. Howard. Einstein’s philosophy of science. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, winter 2015 edition, 2015. 33
- [28] Walter Isaacson. *Einstein: His Life and Universe*. Simon and Schuster, 2008. 17, 32
- [29] Thomas S Kuhn. Logic of discovery or psychology of research. *Criticism and the Growth of Knowledge*, pages 1–23, 1970. 15, 18
- [30] Thomas S Kuhn. *The Structure of Scientific Revolutions*. University of Chicago Press, 1970. 12, 13, 14, 15, 17, 21, 22, 23, 26, 27
- [31] Thomas S Kuhn. The essential tension: Selected studies in scientific thought and change, 1977. 16
- [32] Thomas S Kuhn. Objectivity, value judgment, and theory choice. *Arguing*

- About Science*, pages 74–86, 1977. 20
- [33] Larry Laudan and Jarrett Leplin. Empirical equivalence and underdetermination. *The Journal of Philosophy*, 88(9):449–472, 1991. 83
- [34] Dave Levitan. Donald Trump’s science denial is becoming a national policy. Available at <http://time.com/4646085/donald-trump-science-denial/>. 44
- [35] Joel Lindkvist, Carlos Sabín, Ivette Fuentes, Andrzej Dragan, Ida-Maria Svensson, Per Delsing, and Göran Johansson. Twin paradox with macroscopic clocks in superconducting circuits. *Physical Review A*, 90(5):052113, 2014. 31
- [36] David Lindley. *Uncertainty: Einstein, Heisenberg, Bohr, and the Struggle for the Soul of Science*. Anchor/ Random House, 2008. 29
- [37] Kate Lunau and Katie Engelhart. Why the Higgs boson discovery changed everything. *Maclean’s*, 2015. 39
- [38] James Marcum. Thomas S. Kuhn. Available at <http://www.iep.utm.edu/kuhn-ts/>. 35
- [39] Carl Matheson and Justin Dallmann. Historicist theories of scientific rationality. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, summer 2015 edition, 2015. 18
- [40] Peter Mittelstaedt. Can EPR-correlations be used for the transmission of superluminal signals? *Annalen der Physik*, 7(7-8):710–715, 1998. 28, 96
- [41] Alan Musgrave and Charles Pigden. Imre Lakatos. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, winter 2016 edition, 2016. 8
- [42] Wayne Myrvold. Why talk to philosophers? part ii. Available at <http://www.rotman.uwo.ca/why-talk-to-philosophers-part-ii/>. 11
- [43] Wayne Myrvold. Why talk to philosophers? part iii. Available at <http://www.rotman.uwo.ca/why-talk-to-philosophers-part-iii/>. 30, 31
- [44] N/A. Let’s think about cognitive bias. Available at [http:](http://)

- [//www.nature.com/news/let-s-think-about-cognitive-bias-1.18520](http://www.nature.com/news/let-s-think-about-cognitive-bias-1.18520). 28
- [45] Regina Nuzzo. How scientists fool themselves—and how they can stop. *Nature*, 526(7572):182, 2015. 28
- [46] Patricia F O’Grady. *Thales of Miletus: the Beginnings of Western Science and Philosophy*. Routledge, 2017. 26
- [47] Kent Peacock. Personal conversation, May 2017. 21, 22, 69, 96
- [48] Kent A Peacock. Comment on “tests of signal locality and Einstein-Bell locality for multiparticle systems”. *Physical Review Letters*, 69(18):2733, 1992. 28, 96
- [49] Kent A Peacock. *The Quantum Revolution: A Historical Perspective*. Greenwood Publishing Group, 2008. 90
- [50] Massimo Pigliucci. The future of philosophy of science. Available at <http://rationallyspeaking.blogspot.ca/2010/04/future-of-philosophy-of-science.html>. 48, 49, 95
- [51] Joseph Polchinski. *String Theory: Volume 2, Superstring Theory and Beyond*. Cambridge University Press, 1998. 81
- [52] Joseph Polchinski. Quantum gravity at the planck length. *International Journal of Modern Physics A*, 14(17):2633–2658, 1999. 81
- [53] Karl Popper. *The Logic of Scientific Discovery*. Routledge, 2005. 9
- [54] Karl Popper. Science: Conjectures and refutations. *The Philosophy of Science: an Historical Anthology*, 471, 2009. 4, 5, 6, 7
- [55] John Preston. Paul Feyerabend. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, winter 2016 edition, 2016. 45
- [56] Michael Rescorla. Convention. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, summer 2015 edition, 2015. 9
- [57] Carlo Rovelli. Half way through the woods. *The Cosmos of Science*, pages

- 180–223, 1997. [97](#)
- [58] Wilfrid Sellars. *Science, Perception and Reality*. Ridgeview Publishing Co, 1991. [10](#), [25](#)
- [59] Lee Smolin. *The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next*. Houghton Mifflin, 2006. [16](#), [31](#), [32](#), [33](#), [34](#), [35](#), [36](#), [51](#), [52](#), [53](#), [54](#), [55](#), [56](#), [57](#), [58](#), [59](#), [60](#), [61](#), [62](#), [63](#), [64](#), [65](#), [66](#), [67](#), [68](#), [69](#), [70](#)
- [60] Victor J Stenger, James A Lindsay, and Peter Boghossian. Physicists are philosophers, too. *Scientific American*, 2015. [48](#)
- [61] Leonard Susskind. The anthropic landscape of string theory. 2003. [69](#)
- [62] William Talbott. Bayesian epistemology. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, winter 2016 edition, 2016. [41](#)
- [63] Stephen Thornton. Karl Popper. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, summer 2017 edition, 2017. [5](#), [6](#), [7](#), [8](#), [9](#), [10](#)
- [64] Ben Trubody. Richard Feynman’s philosophy of science. *Philosophy Now*, 114:10–12, 2016. [48](#)
- [65] John Vickers. The problem of induction. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, spring 2016 edition, 2016. [5](#), [25](#)
- [66] Rein Vihalemm. The Kuhn-loss thesis and the case of phlogiston theory. *Science Studies*, 13(1):68–78, 2000. [15](#)
- [67] Matt Warman. Stephen Hawking tells Google ‘philosophy is dead’. *The Telegraph*, 5(17):11, 2011. [2](#)
- [68] Steven Weinberg. *Dreams of a final theory*. Vintage, 1992. [21](#), [48](#)
- [69] Steven Weinstein. Superluminal signaling and relativity. *Synthese*, 148(2):381–399, 2006. [28](#), [96](#)

- [70] Steven Weinstein and Dean Rickles. Quantum gravity. In Edward N. Zalta, editor, *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, winter 2016 edition, 2016. 81
- [71] Peter Woit. *Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law for Unity in Physical Law*. Vintage, 2007. 31
- [72] Natalie Wolchover.
A fight for the soul of science. Available at <https://www.quantamagazine.org/20151216-physicists-and-philosophers-debate-the-boundaries-of-science/>, 2015. 11, 75, 92, 93
- [73] Maryam Zaringhalam. Failure in science is frequent and inevitable—and we should talk more about it. Available at <https://blogs.scientificamerican.com/guest-blog/failure-in-science-is-frequent-and-inevitable-and-we-should-talk-more-about-i>
40