The Many Dimensions of the String Theory Wars

Sophie Ritson

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Introduction

"The confrontation between string theory and its critics", writes Jarod Lanier, "is one of the great intellectual dramas of our age" (Lanier, 2013). String theory is widely regarded by many of its practitioners as the only viable option for constructing a unified theory of gravity and elementary particle physics. It has attracted a large number of high profile researchers, including many Nobel Laureates, and has been instrumental in opening up new areas at the intersection of mathematics and physics. Yet, since the 1980s, string theory has been continuously mired in controversy. It has been labelled "science" (Duff, 2013, p. 185), "speculative metaphysics" (Richter, 2006, pp. 8-9), "non-science" (Woit, 2001, p. 2), "pseudoscience" (Krauss, 2005), "beautiful" (Schwarz, 1996, p. 698), "ugly" (Woit, 2006d, p. 265), the first plausible candidate for "a final theory" (Weinberg, 1993, p. 212), and a "catastrophic failure" (Smolin, 2006b, p. 170). Indeed the debates have, as Peter Galison rightly points out, "raised deep questions about the very nature of physics as a discipline" (Galison, 1995b, p. 403).

While criticisms of string theory have been present since its origin,[1] the publication of Lee Smolin's *The Trouble with Physics* and Peter Woit's *Not Even Wrong* in 2006–2007 brought the controversy to the attention of the media and the wider public. These books were to mark the climax of what has become an increasingly public debate, which has seen physicists trade blows in blogs and online forums, the editorial pages of the *New York Times* and the popular press, popular scientific books, book reviews, public lectures, and in staged public debates (Greene & Krauss, 2007; Greene & Smolin, 2006; Smolin, Duff, & Cartwright, 2007). It was also during this time that the term 'string wars' was first used to describe a debate between supporters and critics of string theory.[2] Aaron Bergman claims he was the first to use the term in a comment on Woit's blog (Bergman commenting on (Woit, 2006a)) and then it was used by 'Alejandro' as a title of a blog post 'Sean Carroll on the String Wars' that identified 2006 as "witnessing an unprecedented large attack against string theory" (Alejandro, 2006). The title had gained traction by October 20th when 'Science Journalist in Residence' at the Kavali Institute for Theoretical Physics George Johnson titled his discussion of the critics and supporters of string theory as 'The String Wars' (audio-visual material available at (G. Johnson, 2006)).

This conventional picture, as a single debate between two incompatible sides, is also present in much of the secondary literature. This has led several historians and philosophers of science to ask if the widespread belief in string theory constitutes a new post empirical era for science. In this literature, the string theory debates are held up as evidence of emergent conceptualisation of science. Those who

[1] See (Rickles, 2014) for the early history of string theory.
[2] The first use of the term I found was in (Ginsparg & Glashow, 1986, p. 2): "Not even a politically popular "Superstring Detection Initiative" with a catchy name like "String Wars" could get us to energies where superstrings are relevant". However, this does not refer to the disputes with regards to the string theory research program. For more discussion of this paper, see chapter two.

invoke the conventional picture of the debates often assume that the critics and supporters of string theory are divided in their assessment of string theory as a science on the basis of a lack of empiricism in string theory.[3]

The many points of conflict

This thesis analyses the debates over string theory. In doing so, it attempts to correct the prevailing picture of the string wars as a debate between two incompatible sides, of pro and against string theory or of critics and supporters of string theory. Instead what is presented in this thesis is description of many points of conflict. There are more than 30 points of conflict identified, in relation to each of which there is a spectrum of responses beyond positive or negative appraisals of string theory. Furthermore the protagonists of the debates cannot be neatly divided into supporters and critics of string theory. Individuals who are united in a position on, or response to, one point of conflict are often divided in their position on, or response to, a different point of conflict. This approach is unique, as previous historical or philosophical literature has maintained the prevailing picture of the string wars as a debate between two incompatible sides.

The debates concerning whether string theory can be considered to be science, have *several points of conflict* concerning in principle testability, self-immunisation strategies and *ad hoc* manoeuvres, retrodictions, progress as constituted by solving problems, Falsificationism and metaphysics. Furthermore there are also several other debates concerning the dominance of string theory and the organisation of science, access to literary technologies and the definition of expertise, and methodological virtues as constraints. Just as there are multiple points of conflict identified in the debate over whether string theory is a science, these debates also have several points of conflict.

The points of conflict may be identified due to the way they cluster in the discourse of the debates. The terms that identify a point of conflict, such as ‘crack pot’, ‘dominant’, ‘testable’, ‘unique’, ‘science’ and ‘public’, continue to be used as though the meaning is clear despite disagreement as to how to understand the meaning of the term. None the less there is a surprising amount of agreement that these categories[4] (‘crack pot’, ‘dominant’, ‘testable’, ‘unique’, ‘science’ and ‘public’) are the appropriate categories in which to ground the debates. Galison examined the role of constraints in the debates over string theory in 1995 and concluded that:

[3] For example, Johnson and Matsubara claimed that the disconnection with data was typical of charges against string theory: “Their [the critics] main argument is string theory’s lack of new testable predictions despite heavy efforts by a huge number of devoted physicists. But the majority of string theorists do not seem deeply concerned; most still seem to be in [a] good mood” (Johansson & Matsubara, 2011, p. 199). This picture is of a single point of conflict, empiricism, where the scientific status of string theory is called in to question on the basis of methodology.

[4] One problem with the use of this term is the inherent vagueness of the term. However, the ambiguity in the term is also useless as there is a great deal of variation in the debates. An expansive understanding of a category, where it is understood to identify things regarded as having particular shared characteristics, allows for the diversity found within the debates.

> "The string debates revolve around an agreement and a profound disagreement. By consensus, theorists agree that theory must operate under a series of constraints … the dispute is over the source and appropriateness of those constraints." (Galison, 1995b, p. 374)

This statement, construed more broadly, to include the many categories referred to in the points of conflict, characterises the debates over string theory nicely. Despite the complexity of the debates there is continuity, which stems from consistent appeal to the categories invoked in appraisals of string theory. Furthermore the continuity of the categories serves to dampen the idea that string theory is some radically new enterprise or 'post empirical' science. The disputed categories are very familiar to anyone with a passing interest in the history, philosophy of sociology of science and technology.[5]

[5] For example in the case of the debate over literary technologies there is agreement that not every individual should have access to all the features of the online repository arXiv, despite disagreement as to the constitution of the category of who should have access. This example is discussed further in chapter four. In the examination of the debate over the role of 'social' factors in science, there is agreement that the social organisation of science has epistemic consequences and plays a role in heuristic appraisal of string theory and profound disagreement as to the as to how science should be organised, with particular disagreement over the constitution of expertise as heterogeneous or homogenous. This example is discussed further in chapter three.

What is string theory?

David Gross has delivered a talk at almost every 'strings' conference. These conferences happen annually (with one or two exceptions) and each is considered to be the most important string theory conference of the year. David Gross has delivered 'opening remarks', 'closing remarks' or a 'vision' talk at each of the conferences at which he has spoken. The goal of these talks has been to address the state of string theory, past present and future. For almost 30 years, Gross has been asking the question "what is string theory" without answer (David Gross, 1985b, 2012). In 1985 Gross motivated eleven questions as follows:

> "There are many unsolved problems and deep mysteries that need to be understood before one can claim success … I therefore present below, in the belief that questions are often more important that answers, a list of open questions. Most of these are well known to any worker in this field, are serving as a guide to current research and are addressed in the contributions to this workshop." (David Gross, 1985b)

His first question was "what is string theory" (David Gross, 1985b). He asked it again in Tokyo (David Gross, 2003a), in a paper titled 'Where Do We Stand in Fundamental (String) Theory?' (David Gross, 2005), in Madrid where he argued "what we still do not know what string theory is" (David Gross, 2007), and in Upsalla (David Gross, 2011). By 2012 Gross was asking "three questions that I hope will be answer in my life time". The third was "what is string theory" (David Gross, 2012).

Answering the question, what is string theory,[6] is difficult. This is because a satisfactory answer to this question will depend on precisely what is meant by the question, and the question permits multiple meanings. Broadly speaking, the question can be historical (what has the term string theory referred to historically, and how has this changed), sociological (what community identifies with the term string theory) or philosophical (what is the ontology of string theory or perhaps what methodology identifies or defines string theory). The answer to the question in each of these broad areas will likely depend on who is answering the question as no consensus has formed as to what string theory is. Often when the question is asked a standard answer is given, namely that string theory is a proposed solution to the problem of quantum gravity, and that the solution takes the form of a unified theory of quantum gravity. However, as this thesis will explore, string theory as a unified theory of quantum gravity is contested. A simple answer will not be provided here[7] as this thesis is an investigation of the articulations of answers to the question of what is string theory.

For the sake of clarity, the problem of quantum gravity is very broadly considered to be that quantum theory is inconsistent with General Relativity. Again, there is no consensus as to precisely how this

[6] Also known as superstring theory.

[7] For an early history see (Rickles, 2014), and for an introduction to the technical details see (Zwiebach, 2004). For a more advanced technical understanding see the classic text books (Green, Schwarz, & Witten, 1987, 1988; Polchinski, 1998, 2005).

problem should be understood. While there exists a number of other current approaches to research on quantum gravity, most notably (for the debates over string theory) loop quantum gravity, string theory is unique in that it attempts to solve the problem of quantum gravity by unifying gravitation with the three other fundamental forces in nature – electromagnetism, the strong force and the weak force. However whether or not the solution to the problem of quantum gravity requires unification is a point of conflict within the debates.

Again, for the sake of clarity, the standard historical answer is that the origins of string theory are broadly the period of 1968 – 1974, where string theory was a theory of the strong force. In 1974 string theory became a candidate theory of quantum gravity. The theory was also combined with supersymmetry in the following decade and renamed superstring theory, although the name string theory continued to be used. The period from 1984 to roughly 1988 is known as the 'first revolution', during which string theory went from obscurity to relative prominence. The 'second revolution' came in 1995 on the back of the duality relationships as evidence for an undiscovered theory called M-theory and the so-called AdS/CFT duality. In 2003 it was discovered that the universe was expanding at an accelerating rate, which was connected to the rise of anthropic reasoning and multiverse hypotheses in string theory. This thesis will show that the significance of each of these developments is contested in various ways.

String theory: candidate theory of everything 'TOE' and tool

One element of string theory, often unappreciated, is an understanding of the different views of string theory. For the most part, the secondary literature on string theory focuses on string theory as (an attempt at) a theory of quantum gravity. Recently, in response to criticism from Woit, Matt Strassler made explicit two ways in which string theory may be understood:

> "Application number 1: this is the one you've heard about. String theory is a candidate (and only a candidate) for a "theory of everything" — … [which] really means is "a theory of all of nature's particles, forces and space-time".
>
> Application number 2: String theory can serve as a tool. You can use its mathematics, and/or the physical insights that you can gain by thinking about and calculating how strings behave, to solve or partially solve problems in other subjects." (Strassler, 2013a)

From here on, I refer to what Strassler identifies as applications one and two as the theory of everything, or 'TOE view', and 'tool view' of string theory respectively. The TOE view identifies string theory as a candidate for a theory of quantum gravity, which has the potential to unify the Standard Model of elementary particles with General Relativity. As Strassler explains, the second view of string theory, as a tool, has found wide application:

> "String theory has made a number of important hard problems (in non-perturbative gauge theory, for instance) much easier to solve; it has helped address several long-standing conceptual puzzles in theoretical physics; and it has inspired many new ideas that have had application well outside of string theory." (Strassler, 2012a)

On the tool view, string theory is identified as being comprised of a collection of methodologies and techniques which may be used to solve previously intractable problems, such as low temperature super conductivity and quark gluon plasma calculations, but is not necessarily an ultimate theoretical description of nature.

The TOE and tool views of string theory are primarily distinguished by *aims*. The aim of string theory, as a TOE, is to solve the problem of quantum gravity with grand unification. Alternatively the aim of string theory as a tool is to apply methodological techniques from the TOE view of string theory and to apply them to other areas of physics. One such strategy is to model phenomena from other areas of physics as strings. The goal here is analogous to the strategy of transforming an intractable problem into a harmonic oscillator, solving the problem, and then transforming the solution back into the original problem space.

Moshe recently argued that the tool view of string theory was becoming more prevalent: "many of us have come to think about string theory as a method rather than a model" (Moshe commenting on (C. Johnson, 2015). An anonymous author, having recently completed a PhD in string theory, generated discussion in an opinion piece on Reddit titled 'View from an ex-string theorist' where he argued that what I have called the tool view of string theory has produced a body of work now sufficiently distinct from string theory (understood as a unified theory of quantum gravity) that it should be renamed:

> "I have one simple idea suggestion for String Theory which I believe should be implemented immediately. We need to stop calling it String Theory ... The subject is incredibly, incredibly, broad. It's now touching most areas of theoretical physics, essentially, it's tangentially related to anything involving Quantum Field Theory. *It's more a set of tools, than a theory in and of itself*." (Anonymous, 2014) (emphasis added)

The distinction between the TOE and tool view of string theory will be used throughout the thesis. Chapters two and five will examine the explicit debate over these different aims, and the question of how they might be related.

Thesis approach

The aim of this thesis is to develop a better understanding of the various debates over string theory, the so-called ‘string wars’, with a more descriptively accurate picture, as well as a deeper understanding of what is at stake in these debates. A detailed study of the string wars has yet to be completed, this is despite a growing body of literature that draws upon the string wars as evidence for a variety of claims. The approach taken here is one that does not conform perfectly within disciplinary boundaries as the approach is not exclusively historical, philosophical, or sociological. This is because the debates concerning string theory themselves have historical, sociological and philosophical dimensions to them. As such it is necessary for the approach to integrate history and philosophy of science (&HPS). Any attempt to build from a descriptive understanding of string theory, informed by history, that purely characterises the debates from a sociological perspective or a philosophical perspective would miss key details and ultimately be left incomplete, as many of the issues are inextricably tied together. For the sake of clarity, the thesis is organised into sections, with the chapters examining the debates that are identified by protagonists as concerning: philosophy of string theory; the scientific status of string theory; the sociological appraisal of string theory; literary technologies; and methodology.

Outline of chapters

Chapter one, contested philosophies, looks at debates from the ‘philosophical’ literature. The authors of this literature, such as Richard Dawid, Dean Rickles, Carlo Rovelli, Elena Castellani, Gerard ’t Hooft, Lee Smolin, Leonard Susskind, Peter Galison and many more, have a variety of disciplinary backgrounds; however, each contribution to the literature has stated philosophical aims. This chapter functions both as a literature review and, as there is no neat boundary between the primary and secondary literature, it will also function as an examination of the role of the philosophical literature in the string wars. These debates are framed by a meta-dispute as to whether philosophy of science should attempt to play a normative role. The positions taken by authors in this meta-dispute inform their appraisal of string theory: for those who argue that philosophy of science should not play a normative role, string theory is a case study that should be taken as ‘data’ that informs appraisal of ‘modern’ notions of the scientific method. Correspondingly, for those who argue that philosophy of science should play a normative role, appraisal of string theory is informed by the ‘norms of the scientific method’. This is most striking where the literature attempts to understand the lack of empiricism in string theory, or attempts to evaluate progress. In addition to this, two other thematic concerns are explored: arguments as to how the string theoretic dualities should be interpreted; and arguments for philosophically motivated constraints for a (unified) theory of quantum gravity.

Chapter two, contested boundaries, analyses the debates over the scientific status of string theory as an example of the rhetorical construction of the boundaries between science and non-science (boundary work (Gieryn, 1983, 1999)). The approach taken in this chapter is to examine the many points of conflict in this debate, where participants (such as Sean Carroll, Mike Duff, Sheldon Glashow, Brian Greene, Gordan Kane and Leonard Susskind) attempt to assert, for a variety of reasons (such as in principle testability and accusations of *ad hoc* manoeuvres), the scientific or non-scientific status of string theory. Uniquely among most studied episodes of boundary work, string theory was considered the dominant theory of quantum gravity and yet string theorists were forced to defend both their authority as the dominant research program, as well as their perceived attempt to widen the definition of science.

Chapter three, contested sociologies, examines the debates over 'sociology' and norms of scientific enquiry. This perspective examines how protagonists, such as Peter Woit, Lee Smolin, Joseph Polchinski and Clifford Johnson, invoke toy models of sociology to support (as rational) or undermine (as irrational) belief in string theory. A second site of disagreement is over the organisation of science and the constitution of public and expert populations. Protagonists in these debates argue that the social organisation plays a role in determining the ability to judge the potential for a research program to succeed or fail. The chapter frames these debates with reference to epistemic appraisal and the appraisal of the promise of theories of quantum gravity.

Chapter four, contested technologies, examines the disputes over 'expertise' and literary technologies, namely the controversy over web 2.0 technologies as a form of scholarly communication. This is achieved through a detailed case study of the introduction of the trackback feature to arXiv, the repository for physics preprints, which linked (certain) blogs to abstract pages on arXiv. This feature was introduced in an attempt to facilitate blogs' ability to perform peer review. However controversy arose when Peter Woit was denied access to the trackback feature. The trackback feature is somewhat of a red herring in these disputes. The approach taken in this chapter is to analyse the dispute that sees uniform disagreement as to how to define an 'active researcher' and a 'crackpot' and yet uniform agreement as to the existence of these categories. The analysis informs both an understanding of the string wars, where problems of ideology and knowledge are indivisible, as well as the potential function of the literary technology of the blog to perform a role akin to public peer review.

Chapter five, contested methodologies, examines the divergent appraisals of string theory as determined by non-empirical methodologies. The approach taken in the chapter is to examine the conflict as arising from deviating understandings of how methodological virtues should constrain theory construction and appraisal. Contradicting previous literature, evidence is presented for multiple points of conflict rather than holistic rejections of string theory methodology in favour of a more 'traditional' methodology. Furthermore, the string theory community is divided as to the validity of anthropic reasoning and the necessity of uniqueness as a methodological virtue. Rather than appraising string theory through a framework of a 'final theory of everything', this chapter will aim to understand how appraisal is guided by whether string theory is the most promising approach to solving the problem of quantum gravity.

Chapter One: Contested philosophies

Introduction

Literature devoted to string theory in history and philosophy of science literature has, like string theory itself, a long history. The vast majority of the literature has philosophical aims, although the positions expressed are often informed by particular historical interpretations. As string theory rose to prominence in the late 1980s, it also received its first philosophical treatments in 1988. However it is difficult to draw precise boundaries around literature discussing string theory: indeed, how to distinguish string theory from metaphysics is a point of contention in the debates over string theory (see section 1.8 of chapter two). In determining what may be considered historical and/or philosophical literature of string theory, I have followed a rough rule of thumb: where it is stated as an intention of the author (or editors) to contribute historical and or philosophical insights, I have included it here. This rule is obviously not perfect and often authors or editors have a variety of stated aims, but from a pragmatic perspective it allows for progress without getting bogged down in 'demarcation disputes' before even putting pen to page (or fingers to keyboard).

There have been a number of significant books published. An edited volume by Craig Callender and Nick Huggett (2001), titled *Physics Meets Philosophy at the Plank Scale*, sought to explore the philosophical foundations of quantum gravity, bringing both physicists and philosophers together, and featured three chapters devoted to string theory (Unruh, 2001; Weingard, 2001; Witten, 2001). Also with sections devoted to both philosophy and physics content is *Universe or Multiverse* (Carr, 2009). A third edited book, *The Birth of String Theory* (Cappelli, Castellani, Colomo, & Di Vecchia, 2012), is a collection of personal reminiscences of those involved in the development of string theory. One editor also contributed a chapter of particular note for its focus on philosophical issues: 'Early String Theory as a Challenging Case Study for Philosophers' (Castellani, 2012, pp. 63-80). The first book length treatment of the history of string theory, not written by a string theorist, came very recently with *A Brief History of String Theory: From Dual Models to M-theory* (Rickles, 2014). Dawid's *String Theory and the Scientific Method* remains the only book length treatment by a single author dedicated to a philosophical interpretation of string theory (Dawid, 2013a). Also significant for an extended examination of string theory was the 2013 special issue of *Foundations of Physics* 'Forty Years of String Theory: Reflecting on the Foundations', edited by Gerard 't Hooft, Erik Verlinde, Sebastian de Haro and Dennis Dieks. As is evident from this brief description of relevant books and journal special issues, there has been an increase in interest in the past decade. Standard text books in string theory include *Superstring Theory* Volumes I and II (Green et al., 1987, 1988), *String Theory* Volumes I and II (Polchinski, 1998, 2005), and for an accessible introduction, *A First Course in String Theory* (Zwiebach, 2004).

Curiously, there has been very little written on the controversy over string theory, and that which has been written has mostly been approached from a historical and philosophical perspective. Even the history of string theory has only very recently been addressed (Rickles, 2014). Before Rickles, Kragh (2011a) also examined the history of string theory; however, Kragh did not engage deeply with historical questions and instead opted to tell the 'standard story' that is present in many accounts of the history of string theory written by string theorists (see for example: (Greene, 1999b) (Susskind, 2005) (Cappelli et al., 2012)). In contrast to such accounts, Kragh also provides a brief overview of some controversial elements of string theory, as well as various rebuttals by string theorists. Much of the literature on the controversy over string theory addresses only one element of the controversy over string theory: the lack of experimentation in the string theory research program. One exception is Ritson and Camilleri (2015) who focus on both the methodological and sociological points of contention (analysed as an example of boundary work (Gieryn, 1983, 1999)).

Recently there have been explicit calls from physicists for philosophers to get involved in the debates over string theory. One such example is Sabine Hossenfelder's review of Dawid's *String Theory and the Scientific Method* (Hossenfelder, 2015). Despite disagreeing with Dawid's conclusion, as discussed below, Hossenfelder argues that the book is "very recommended reading for both physicists and philosophers" (Hossenfelder, 2015). In an interview discussing the recent BICEP2 results, which were initially interpreted by some as evidence for cosmic inflation and certain multiverse scenarios (including string theory's multiverse scenario), Steinhardt was also quoted as saying "I wish the philosophers would get involved" (Steinhardt, 2015). The strongest call for philosophical intervention so far has come from George Ellis and Joe Silk in a *Nature* editorial piece. They argued that greater caution should be exercised for theories that have not been tested (G. Ellis & Silk, 2014). Their proposal was that "physicists, philosophers and other scientists should hammer out a new narrative for the scientific method that can deal with the scope of modern physics" (G. Ellis & Silk, 2014, p. 323). In order to achieve this, Ellis and Silk called for a conference to be convened in 2015 with both sides of the testability debate over string theory and multiverse scenarios present. A conference was convened for December 2015, titled 'Why Trust a Theory', and was organised by philosopher Richard Dawid and Ellis and Silk. The invited participants were both physicists and philosophers.[8] The editorial also generated a series of discussions of the scientific method in the blogosphere: 'Defend the Integrity of Physics' (Woit, 2014b), 'Does the Scientific Method Need Revision' (Hossenfelder, 2014a), 'Method and its Discontents' (Orzel, 2014) and 'The Most Dangerous Ideas in Science' (Frank, 2015). Finally, *Foundations of Physics* has also called for more philosophical work dedicated to string theory. The current description of the journal reads: "we think it is time for the experts on

[8] Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics (7-9 December, 2015, Munich) http://www.whytrustatheory2015.philosophie.uni-muenchen.de/index.html

quantum gravity, quantum information, string theory, M-theory, and brane cosmology to ponder the foundations of these approaches" ("Foundations of Physics", 2015).

There is a rapidly growing field of philosophy of cosmology and much of this literature is devoted to issues concerning the multiverse. In this chapter I have kept my focus to literature that examines a concept of a multiverse as motivated by string theory. For a wider look at topic, the influential paper 'The Multiverse Hierarchy' provides a taxonomy of the various conceptions of multiverse (Tegmark, 2009). The paper is published in *Universe or Multiverse?* (Carr, 2009); an edited book that came out of a conference at Stanford in 2003 of the same name. There is also a burgeoning literature examining the concept of emergence in theories of quantum gravity, including string theory. For an introduction see (Butterfield & Isham, 2001) or for more recent work see the recent special issue section in *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 'The emergence of spacetime in quantum theories of gravity', including work such as: (Bain, 2013; Crowther, 2013; Huggett & Wüthrich, 2013a, 2013b; Lam & Esfeld, 2013; Rickles, 2013a; Teh, 2013).

It would be inaccurate to label the literature that discusses string theory as representative of a debate between philosophers as authors are not critiquing arguments presented by others. Furthermore, for the most part, the authors do not acknowledge alternate positions. However there are conflicting positions offered, including at the meta-level, with alternate philosophical methodologies advocated as to how to interpret string theory as a case study. In addition, five thematic tensions were found to be present in the literature that examines string theory: arguments as to how to interpret that lack of empiricism; arguments as to how to evaluate progress; arguments for a new or altered scientific method; arguments as to how to interpret the string theoretic dualities; and arguments for philosophically motivated constraints upon a unified theory of quantum gravity.

1. Thematic concerns expressed in the literature

1.1 String theory as case study: divergent positions on philosophical methodology

In the philosophical tradition of using historical case studies, string theory is an intriguing entity. It is both marshalled as an historical episode that informs our philosophy of science correspondingly and as an historical episode that philosophy of science can characterise both descriptively and normatively (with widely divergent conclusions). String theory both informs modern notions of the 'scientific method' and fails to live up to the norms of an orthodox scientific method. The debate over 'characterising' string theory calls into the question the unstated implicit assumption by those who attempt to use string theory to inform the philosophy of science – that string theory can be simply characterised as 'science'. Of course, the corresponding assumption is also called into question – that string theory may be normatively characterised by philosophical models. In the literature, there are examples of accounts of string theory being used as 'evidence' that philosophical understandings of science and the scientific method need to be updated. At the same time there are other examples from the literature that argue that philosophy should normatively guide historical interpretations of string theory as well as an understanding of string theory. To make matters worse, or perhaps to make matters more interesting, this is not ancient history nor is it even confined to the previous century – while the history of the string theory research program has its roots in the twentieth century, it continues to be played out to the current day where characterisations of the research program have very real implications and consequences. A question that arises from these conflicting positions: should philosophy of science play a normative role in contemporary disputes?

In 'Quantum gravity meets & HPS', Rickles argues for an 'integrative' approach to philosophy of science (Rickles, 2011a). Citing Cushing (1990) as methodological inspiration, Rickles argues that "history is of vital importance to philosophical theses" (Rickles, 2011a, p. 5). The focus of his chapter is to present the history of quantum gravity as an "excellent example" for integrated history and philosophy of science. Rickles argues that in the history of quantum gravity "we have a natural convergence of history, philosophy, and sociology [and that] a study of quantum gravity along any one of these lines (philosophical, historical, sociological) will inevitably find itself incorporating the others" (Rickles, 2011a, p. 5). This claim has certainly been borne out in the literature discussed in this chapter, much of which cannot be easily defined as exclusively philosophical, sociological, or historical. Whilst Rickles argues for the value of integrated history and philosophy of science, it is clear that he believes that string theory as a case study should inform our philosophy of science. In later work, Rickles again draws upon Cushing to advocate for what he calls the 'Cushing maxim': "Science is what scientists have done, not what a philosopher tells us the scientist meant to do, were really doing, or should have done" (Cushing quoted in (Rickles, 2015, p. 1)). Rickles argues that the history of quantum gravity research has a number of advantages. For philosophers who are interested in 'revolutionary science', quantum gravity offers an example of a "revolution in *process*" (Rickles, 2011a, p. 4) (emphasis in original). Consequently for Rickles string theory is a case study that informs philosophy of science, and it is the task of philosophers to understand (Rickles, 2008a, p. 317).

Kragh too adopts the methodology that history should inform philosophy. In 2011 Kragh examined 'The Multiverse Scenario' and 'String Theory and Quantum Gravity' (Kragh, 2011b, 2011c). These chapters are taken from a book length treatment of a series of theories as case studies that "have in common that they are ambitious attempts to describe all or most of nature on a unified basis" (Kragh, 2011a, p. 1). Kragh examines the multiverse controversy as straddling a boundary between physics and philosophy in section 10.5 of 'The Multiverse Scenario' (Kragh, 2011b, pp. 280-285). Kragh describes the controversy as one where "the very standards of science are at stake" (Kragh, 2011b, p. 281).

In contrast to the positions offered by Kragh and Rickles, in 'Against the Excesses of Quantum Gravity: A Plea for Modesty', Curiel takes a stance against uncritical acceptance of the scientific merit of theories of quantum gravity (Curiel, 2001). In place of an uncritical acceptance, Curiel argues for a normative role for the philosophy of science in determining the scientific merit of theories of quantum gravity (Curiel, 2001, p. S440). This position is informed by a belief that if "good science is not merely whatever famous scientists of the day happen to be doing – then the mettle of philosophy of science surely demands that the philosopher speak up against the practice of contemporary physicists when their practice calls for it" (Curiel, 2001, p. S440). Also in contrast to the position argued by Rickles (drawing from Cushing) (Rickles, 2011a), Ehrlich argues that the evaluation of string theory should be based on theory itself and that "decisions as to what constitutes a legitimate scientific theory are simply too important to be left to the practitioners of that field" due to vested interests and the possibility of applying double standards (Ehrlich, 2006, p. 86). This quote provoked heated discussion at the recent conference 'Why Trust a Theory' from both physicists and philosophers who sparred on what *should* be the role of philosophy of physics and areas where philosophers might legitimately contribute.

Invoking an alternate motivation, Castellani also argues that philosophy should not play a normative role. 'Early string theory as a challenging case study for philosophers' (Castellani, 2012) is the only chapter contribution to *The Birth of String Theory* (Cappelli et al., 2012) not written as a personal reflection (with the exception of the introduction and synopsis) and is for the most part a summary of the early history of string theory (as provided by the reflections of authors of the chapters that make up the book). Castellani peppers the summary with flags to points in the history she considers to be of philosophical interest. Her stated intention is to suggest future directions for philosophical treatment of the early history of string theory for "those philosophers of science who pay attention to actual scientific practice" (Castellani, 2012, p. 63). For Castellani, the methodology of this approach to philosophy of science is one that utilises case studies "in a role analogous to that of the data of experience in scientific theories" (Castellani, 2012, p. 63). The "evidence" provided by the early history of string theory is argued by Castellani to be significant from the perspective of informing general understandings of scientific methodology. The dominance of string theory is offered as justification for the significance of the "evidence" where string theory is characterised as a theory that "has dominated a significant part of theoretical research over recent decades" (Castellani, 2012, p. 63). There are indications throughout the chapter that the intended audience is wider than philosophers. Several passages are written to inform string theorists (and the theory's critics), such as the authors of the chapters of *The Birth of String Theory*, who often invoke philosophical concepts such as falsifiability rhetorically (see sections 1.2 and 1.3 of chapter two for more). This is clear from the broad and general introductions given to well-trodden philosophical concepts. The central themes of the chapter, identified by points of philosophical interest, are: scientific progress; methodology and scientific practice; and rationality.

This literature may be considered to be part of a wider controversy over epistemic authority between philosophers and physicists concerning contemporary scientific practice. Pigliucci has pointed to a recent tradition of scientists such as Steven Weinberg, Stephen Hawking, Lawrence Krauss, Neil deGrasse Tyson "despising" philosophy of science. Pigliucci has drawn comparisons to the science wars and labelled this wider controversy the "physics wars" (Pigliucci, 2015). Kragh also notes that for the most part philosophy of science is rejected by protagonists in the debate over string theory. The single exception is the debate over falsifiability, where falsifiability is invoked as a crucial demarcation criterion by Ellis, Smolin and Mario Livio and rejected by Susskind (Kragh, 2011b, pp. 282-283). Following on from Michael Heller, Kragh argues that the notion of falsifiability, as debated by Smolin, Susskind and others, is not representative of Popper's position on falsifiability and that there is little evidence to suggest that any involved have read Popper (Kragh, 2011b, pp. 283-284).

1.2 How should we understand the lack of empiricism?

Arguments as to how to interpret the lack of empiricism are present in much of literature from the very first contributions. In 'Theory Bound and Unbound: Superstring and Experiment', Peter Galison (1995) examines how some string theorists can claim, in the same breath, that string theory is a "final theory" and also that "the phenomenological success of superstring theory is not part of the justification so far" ((Weinberg, 1986) quoted in (Galison, 1995b, p. 372)). This is both a historical question, as Galison is concerned with what series of events transpired for this position to be offered, and a philosophical question, in relation to which string theory is seen as a case study that illuminates a changing or perhaps new role of theory in late 20th century physics. The subject of this piece sits inside Galison's other work that examines the changing nature of experimentation (Galison, 1987), and the argument presented develops Galison's earlier work on the relationship between theory and experiment from the other end of the spectrum (an absence of experimentation).

The discussions of the non-empirical nature of string theory are most commonly situated among other methodological virtues and the conclusions reached as a result of weighing up the merits of multiple methodical virtues. The first contribution to the philosophical literature is arguably 'A Philosopher Looks at String Theory', written by Robert Weingard in 1988 (Weingard, 1988). In his paper he aims to provide a simple descriptive account of string theory, provide some initial thoughts of the ontological implications of string field theory, and to question whether there is a (good) reason to believe that string theory would supersede quantum field theory (Weingard, 1988, p. 96).

Weingard acknowledges that if string theorists are able to provide a finite theory of quantum gravity, that unites the fundamental forces, it *would* be "tremendously interesting" (Weingard, 1988, p. 105). However he remains sceptical that this will be achieved, and argues that the history of the twentieth century has shown that unification is not sufficient to determine a successful theory (Weingard, 1988, p. 105). He argues that it is remarkable that string theory exists given that "there are no clues in our low energy world to the high energy world of strings" (Weingard, 1988, p. 105). From this text it is evident that the relationship between string theory and empiricism was found to be problematic from the outset of the philosophical literature. It is also evident that the sufficiency of unification as a methodological virtue was disputed. Weingard's characterisation of the situation is scepticism about the potential for the theory to provide or to transform into the "fundamental framework for physics" (Weingard, 1988, p. 105).

Offering an alternate position, Shapere argues that non-empirical science should not be rejected *a priori*. In 'Testability and Empiricism' Shapere argues that we should "reconsider the role of the unobservable in science" (Shapere, 2000, p. 154). Shapere gives two motivating factors for this reconsideration: research at the 'frontiers of particle physics and cosmology', such as string theory, for which observation and experimental tests seem impossible and an observation of a rejection of "traditional empiricism" (Shapere, 2000, pp. 153, 154). Under the banner term 'traditional empiricism' Shapere includes classical empiricism, twentieth century logical positivism, logical empiricism and Popperian falsifiability. For Shapere, 'traditional empiricism' should be rejected due to a lack of an adequate definition of a class of untainted observables and due to the theory ladenness of observation. As a consequence of reconsidering the role of the unobservable, Shapere argues that traditional empiricism does not hold against this new view of research in particle physics and cosmology and that 'traditional empiricism' cannot distinguish between "legitimate and merely wild speculation" made in science (Shapere, 2000, pp. 154-155). On this basis Shapere constructs a normative argument that science may appeal to the unobservable when "appropriately constrained" (Shapere, 2000, p. 155):

> "The idea of something which unobservable, and which may even have no observational consequences, can come to be accepted if it is either (a) logically or mathematically implied by something that is observable or have observable consequences, (b) needed for consistency considerations (as in superstring group selection), even though it is not implied by the observable portions of the theory, or (c) provides answers to problems concerning the observable parts of the theory with which no other solution deals successfully." (Shapere, 2000, p. 159)

The constraint identified by Shapere is background information which he defines as a limiting factor for legitimate speculation.

Shapere stresses that he does not believe there is sufficient evidence from cosmology and particle physics to justify an argument for non-empirical science (citing the example of the mathematical difficulties of superstring theory) (Shapere, 2000, p. 160). Instead Shapere argues that the scientific status of non-empirical knowledge should not be rejected *a priori* and that there were "*reasonable grounds* for believing that such a stage can in principle be reached" (Shapere, 2000, p. 161) (italics author's own). For Shapere this is where the rationality of a non-empirical science rests on suitable constraints and evidence that particle physicists and cosmologists are already employing non-empirical methodologies. Weingard and Shapere each claim that string theorists are employing non-empirical methodologies such as consistency; their divergent conclusions are as a result of interpreting the significance of non-empirical methodologies as one of several methodological virtues.

The previously discussed paper by Curiel (2001) also examines the absence of empiricism. Curiel identifies several practices he considers concerning. Specifically, Curiel is critical of appraisals of quantum gravity in general and string theory in particular that do not "recognize and advertise" the lack of connection to experiment (Curiel, 2001, p. S424). Curiel further criticises string theory for a lack of honest appraisals of the state of the field. To do this, Curiel extends his strategy of exposing instances of a lack of empirical foundation to his evaluation of string theory. Curiel examines "the loudest crows of triumph"[9] that is the computation of Bekenstein-Hawking entropy from first principles using string theory (Curiel, 2001, p. S434). In the situation where all the support for the Bekenstein-Hawking formula is theoretical Curiel takes issue with Maldacena (2000, p. 6) referring to it as "fact" that back holes are thermal objects and Ashtekar (2000, p.16) referring to Hawking evaporation of black holes as a "physical consequence" (Curiel, 2001, p. 435). Curiel's focus is not so much to give a negative characterisation of string theory or theories of quantum gravity, but rather to

[9] Curiel also argues that Strominger and Vafa's (1996) work on the 'Microscopic origin of the Bekenstein-Hawking entropy' was the cause for the renewed optimism in the string theory community in the latter half of the 90s. This alternate picture adds detail to the standard story, often told by string theorists, that a second revolution and time of great optimism began in 1995 with Witten's Strings 95 presentation in which he proposed M-theory on the back of a family of duality relationships.

implore those engaging in quantum gravity research to exercise ‘modesty’; that is to say that Curiel is calling for more honest appraisals of quantum gravity research that acknowledge a lack of connection with experiment.

Offering an alternate position to Curiel is Rickles, in the first of the explicitly philosophical treatments of string theory for the special issue of *Foundations of Physics* titled ‘Mirror Symmetry and Other Miracles in String Theory’ (Rickles, 2013b). Rickles argues that both the dominance of string theory and the further pursuit of string theory can be considered rational despite a lack of empirical support. Rickles employs a modified version of the ‘no miracles argument’ (Putnam, 1975) that he argues is significant for conclusions regarding rationality as opposed to ‘truth’ or scientific realism. The second modification is that Rickles is concerned with miracles that are “surprising *mathematical* facts” (as opposed to surprising empirical successes in Putnam’s original version)[10] (Rickles, 2013b, p. 56). A second claim made in the paper by Rickles is that string theorists employ the style of reasoning identified as the modified no miracles argument. Rickles is careful to separate his descriptive claim, where he convincingly argues that for certain string theorists “the success of the mathematical predictions are seen as evidence for the framework that generated them” (Rickles, 2013b, p. 54), and his own argument that these successes can be interpreted as an argument for the rationality of pursuing string theory (Rickles, 2013b, p. 78). The modified no miracles argument can be summarised as follows, as an inference to best explanation:

1. In the case where precise quantitative experiments are unlikely (and it is misguided to demand precise quantitative experiments of a theory of quantum gravity).
2. Where it is also the case that a combination of physical constraints and mathematical consistency in string theory has led to mathematical insight.
3. The best explanation for the mathematical fertility of string theory is that string theory is in some sense ‘true’.

It is therefore rational to pursue string theory. (Rickles, 2013b)

In addition Rickles also argues that his modified no miracles argument also adds to the “credibility” of string theory (Rickles, 2013b).

The first premise is explored in the context of quantum gravity research in general. Rickles claims that string theorists are well aware that in quantum gravity research “the scale at which unique, quantitatively determinable new predictions are made is well beyond the reach of any experiment, past, present, or (conceivable) future” (Rickles, 2013b, p. 55). To support this claim, Rickles draws on

[10] Putnam’s no miracles argument is that scientific realism “is the only philosophy that doesn’t make the success of science a miracle” (Putnam, 1975).

the well-known quote from the first string theory textbook: “quantum gravity has always been a theorist’s puzzle par excellence. Experiment offers little guidance” ((Green et al., 1987) as quoted in (Rickles, 2013b, p. 55)). Rickles also draws on a historical precedent in astronomy research from 1955 as identified by Schrödinger. In the instance where physicists are unable to interfere with their subjects, such as astronomy and string theory, Rickles argues that new methods of theory appraisal should be expected such as:

> “1. Shift to the observational methods of precisely the kind relied upon by astronomers and cosmologists.
> 2. Reduce the emphasis placed on quantitative predictions (in favour of weaker, qualitative predictions).
> 3. Attempt to utilise a range of other theoretical virtues, such as the ability of a theory to unify a broad range of disparate (old) knowledge.” (Rickles, 2013b, p. 56)

The second premise, that a combination of physical constraints and mathematical consistency in string theory has led to mathematical insight, is examined through the example of mirror symmetry (Rickles, 2013b). For Rickles the crucial point made is that “mirror symmetry was a mathematical discovery that arose from the study of (what purports to be) a physical theory, based on general physical principles combined with mathematical consistency” (Rickles, 2013b). Rickles defines string theory as a ‘physical theory’ by reference to the aim of the theory to “accommodate or be consistent” with “countless features of the world at lower energies” (Rickles, 2013b, p. 67). Rickles clearly summarises the relevance of the example as follows:

> “What the physical investigations suggested was that when one formulates a 2D conformal field theory (a string theory) on certain kinds of manifold (those with several dimensions compactified in ‘the right sort of way’) one finds that there is more than one such compact manifold for a single conformal field theory. In other words, the map from the structure of the compact dimensions to the low energy physics is many-to-one.” (Rickles, 2013b, p. 67)

The ‘surprising’ nature of this is that string theorists were able to “suggest an equivalence between mathematical objects that were previously thought to be quite distinct” and the ‘successful’ nature of this comes from the fact that “the suggestion was later confirmed and made into a rigorous theory by mathematicians” (Rickles, 2013b, p. 67).

The third premise is examined by reference to an expansion upon how the concept of ‘truth’ should be understood. Rickles explicitly excludes defining truth as “real physical truth”, but he does not pin down a definition of truth, citing the many and well-known difficulties of that endeavour in the philosophy of science literature (Rickles, 2013b, p. 72). Instead Rickles claims that truth can be understood in a variety of ways "from correspondence with some 'facts in the world' to cohere with a background web of beliefs about the world" (Rickles, 2013b, p. 72). It is on this understanding of 'truth' that Rickles embeds within his modified no miracles argument.[11]

Across the philosophical literature that explores the theme of the lack of empiricism in string theory it is common for the lack of empiricism to be explored in historical contexts as well as within the context of methodological virtues. Furthermore, arguments for various methodological virtues are often historically situated, with episodes in the history of science being marshalled as evidence. Curiel (2001) argues that non-empirical science has not historically been successful and therefore denies that consistency is sufficient to render a non-empirical theory of quantum gravity successful. In contrast Dawid draws upon the history of the Standard Model as the historical episode that supports trust in non-empirical string theory. This inductive argument asserts that the methodologies that were successfully employed in the construction of the Standard Model are also being applied in string theory, and therefore we can also trust that string theory will generate predictions that will be confirmed, as was the case with the Standard Model (Dawid, 2013a, 2013b). Whilst Dawid's descriptive claim that the methodologies employed by string theorists are the same as those employed in the construction of the Standard Model has been criticised (Benedictus, 2014; Rickles, 2015), Dawid's contributions are an example of the strategy of situating methodologies historically in order argue for (or against) non-empirical methodologies. Rickles also draws on the history of successes and failures of theories of quantum gravity to establish the rationality of non-empirical methodologies (Rickles, 2011a, 2008c).

1.2.1 Empiricism and demarcation

Despite Laudan's famous death notice for the demarcation problem (Laudan, 1983), there has been a resurgence of work, quite recently, that seeks an alternate approach to finding necessary and sufficient conditions, such as is found in the edited volume: *Philosophy of Pseudoscience: Reconsidering the demarcation problem* (Pigliucci & Boudry, 2013). Unsurprisingly, given the long tradition in philosophy of science of tying attempts to demarcate science from non-science by reference to an empirical tradition, the lack of empiricism in string theory has also been discussed.

In 'What Makes a Theory Testable, or Is Intelligent Design Less Scientific Than String Theory?', Ehrlich compares intelligent design and string theory to see if there is a consistent basis for an exclusion of intelligent design from the category of science whilst keeping string theory scientific

[11] Rickles also examines Dawid's (2007) argument for the truth of string theory in 'Scientific Realism in the Age of String Theory' on the basis that string theory is unique (in that it is a quantum theory of gravity that unifies the forces). Rickles rejects this argument on the basis "that there is no reason why there could not be multiple distinct frameworks for describing the same picture, even when we are dealing with 'theories of everything'" (Rickles, 2013b).

(Ehrlich, 2006). Ultimately Ehrlich argues that falsifiability/testability should not be considered as a sufficient criterion for demarcation and that speculative science has a role to play. He argues that the motivations of the practitioners should also be considered and that string theory plays a scientific role in offering “encouragement to experimenters to improve the sensitivity of specific sorts of observations” (Ehrlich, 2006, p. 88). However, for Ehrlich, there is an expiration date on speculative science, if a theory keeps encountering instances where it does not work and does not ‘hit the jackpot’ of a confirmed prediction: after twenty years it is unclear whether speculative science should still be considered science (Ehrlich, 2006, p. 88).

In ‘The Internal and External Problems of String Theory: A philosophical view’, Reiner Hedrich (2007) argues that in three decades string theory has made no empirically testable predictions but at the same time has been worked on by a large number of physicists. He argues that this calls into question whether those who are called, and call themselves, physicists are producing physics: “Has physics with its unification program and under strict pursuance of its traditional methodological strategies possibly transcended the context of the empirical sciences and entered that of a mathematically inspired metaphysics of nature?” (Hedrich, 2007, p. 264). Hedrich aims to understand the history of string theory, its current state, its problems (dividing them into internal and external) and its prospects, combining these to answer the aforementioned question (Hedrich, 2007, p. 264).

Hedrich claims that string theory suffers from debilitating internal problems: the landscape problem, *ad hoc* manoeuvres to maintain internal consistency and the beginnings of self-immunisation. Furthermore string theory suffers from an absence of external problems to serve as motivators (Hedrich, 2007, pp. 265-266). The only possible contender for an external problem is finding a consistent unification between quantum mechanics and general relativity, and Hedrich claims that string theory is unsuccessful on this front given a lack of solutions to external problems (Hedrich, 2007, p. 267). Hedrich also argues that the landscape problem has rendered string theory incapable, in principle, of delivering solutions to an external problem. Combined with a lack of empiricism, this leads Hedrich to characterise string theory as a “mathematically inspired metaphysics of nature” (Hedrich, 2007, pp. 270-271). One consequence of this is three possible outcomes for string theory: it could make contact with experiment, resolve its internal problems and begin to solve external problems (aka in Hedrich’s characterisation of the situation, become successful); fail to provide any description of nature or; finally, do neither and, due to its lack of empirical content and history of ‘accommodations’, it could stabilise in this position (Hedrich, 2007, pp. 274-275). If this final option is allowed to occur, then the impact upon the character of physics is such that there has been a significant error in quantum gravity research at some stage. The final piece in Hedrich’s argument is the 30 year history of string theory and the many physicists that have worked on it to generate an unbalanced amount of success, suggesting the third option may already be occurring (Hedrich, 2007, p. 276). To counteract this, Hedrich argues for a plurality of approaches to quantum gravity research.

In 'String theory and general methodology: A mutual evaluation', Johansson and Matsubara attempt to interrogate string theory with various approaches from classical philosophy of science (Johansson & Matsubara, 2011)[12]. In turn the paper examines string theory from a Logical Positivist's perspective, from a Popperian perspective, a Kuhnian perspective and a Lakatosian perspective. They concluded that a Logical Positivist would find that string theory is unsuccessful due to a lack of improvement in using the theoretical framework of string theory as a tool for making observable predictions (Johansson & Matsubara, 2011, p. 9). In order to examine string theory from a Popperian perspective, Johansson and Matsubara present the views of Feynman (as a Popperian) and Susskind (as an anti-Popperian). The crux of the issue is identified as: when is it time to give up on a 'speculative metaphysical' idea if it has yet to develop a testable hypothesis? Johansson and Matsubara argue that a commitment to 'in principle falsifiability' will not resolve this issue as it does not discriminate as to when to give up (Johansson & Matsubara, 2011, p. 11).

The latter two perspectives have each received some attention in the literature. Approaching a Kuhnian analysis from a different perspective to that adopted by Dawid (Dawid, 2009), Johansson and Matsubara ask if the string theory paradigm is in a period of crisis and likely to be replaced in a revolution. The authors both dismiss the idea that string theory may be in a crisis state, and a normative reading of Kuhn that would prescribe a rejection of string theory (or any paradigm) (Johansson & Matsubara, 2011, p. 13). The authors come to similar conclusions as those made by Cartwright and Frigg in their Lakatosian analysis of string theory (Cartwright & Frigg, 2007), arguing that string theory, despite indications that it is in a degenerating state, still deserves to be pursued. Additionally Johansson and Matsubara consider how to approach comparisons between research programmes in quantum gravity research where none of the competitors has been able develop an empirical dimension (Johansson & Matsubara, 2011, p. 19). This raises the interesting question of how should we understand the rationality of pursuing one approach of quantum gravity over another. Johannsson and Matsubara's answer to this difficulty is to argue for a position similar to that expressed by loop quantum gravity theorist Lee Smolin, namely that a plurality of approaches should be encouraged (Johansson & Matsubara, 2011, p. 29).

Both the advocacy for a plurality of approaches and the Lakatosian analysis of string theory as a research program are somewhat unsatisfying. This is because they fail to equip those who engage with the approach with an ability to appraise string theory as an ongoing research program or to compare string theory to alternative theories of quantum gravity. There are practitioners within the field of quantum gravity who do have to make decisions as to what approach is most likely to be successful, and an understanding of what would constitute a successful theory of quantum gravity would at least

[12] One chapter from Matsubara's thesis very closely resembles this paper. The thesis chapter entitled 'Methodology and Research in Quantum Gravity' also includes details of loop quantum gravity so as to broaden the focus beyond string theory to quantum gravity (Matsubara, 2013b, pp. 31-54).

go some way to understanding how appraisal of string theory is occurring. Furthermore there is a long history of failed attempts of theories of quantum gravity, and a historical understanding of the appraisal of a failed theory of quantum gravity would add insights into how the field has changed.

1.3 How should we evaluate progress?

Continuing with authors using the strategy of historically motivated arguments, Rovelli also argues that insights from history can inform a concept of cumulative progress in science (Rovelli, 2001). In 'Quantum Spacetime: What do we know?' Rovelli motivates the problem of quantum gravity as "a major challenge, perhaps *the* major challenge in today's fundamental physics" (Rovelli, 2001, p. 101) (italics author's own). The tone is optimistic in Rovelli's discussion of the lack of any empirical access to the regimes in which quantum gravity phenomena are likely to appear. Quantum mechanics and general relativity are taken to be "extremely general facts" that provide "information" about quantum gravity (Rovelli, 2001, p. 101). Rovelli outlines the problem of quantum gravity as framed by three historical developments: Einstein 'finding' special relativity by merging Maxwell's theory of electromagnetism and the Galilean insight of relative velocities and equivalent inertial systems (Rovelli, 2001, pp. 103-104); the replacement of classical mechanics with the deeper understanding provided by quantum mechanics (Rovelli, 2001, pp. 104-105); and the introduction of no absolute referent of motion (background independence) as part of the ontology of general relativity (Rovelli, 2001, pp. 105-109). Rovelli argues that the historical insights he provides should inform methodological norms, and as such he criticises the path followed by perturbative string theory which in his view does not follow the insight provided by general relativity. Instead Rovelli argues that "right way to go" is to attempt to formulate a background independent theory rather than "hope" to recover general relativity "down the road" (Rovelli, 2001, p. 109).

Rovelli explicitly commits to a cumulative notion of progress in physics that goes beyond merely saving the phenomena: "we do not just save the verified empirical content of the old theory, but more. This 'more' is a central concern for good physics" (Rovelli, 2001, p. 116). Rovelli argues that historically good physics has been grounded in "confidence in old theories" to again reiterate the central thesis of his paper that background independence is required for "any fundamental description of our world" (Rovelli, 2001, pp. 117, 118). For Rovelli, good science is conservative rather than 'pessimistic' or 'radical' and he finds evidence in the history of science to support this position. Under this notion it is necessary for previous successes to be retained for progress to occur.

Curiel (2001) has criticised Rovelli's (2001) interpretation of the history of advances in theoretical physics. Providing a contradictory historical interpretation, Curiel argues that revolutions in physics have only taken place in instances where the "impetus of masses of seemingly inter-related experimental evidence that could not otherwise have been explained by the best going theories of the day" (Curiel, 2001, p. 429). Leveraging historical examples in support of this interpretation, Curiel denies the weight of a consistency argument for a theory of quantum gravity in the absence of empirical evidence for phenomena not explained by either a quantum or gravitational theory (Curiel, 2001, p. 431). Whilst Curiel is not explicitly concerned with progress (as discussed earlier Curiel is concerned with the absence of empiricism), his argument concerns progress as it amounts to a sceptical position that advances will occur for a theory of quantum gravity without an empirical basis.

Cartwright and Frigg explicitly address the issue of how to understand progress in string theory where argue that in evaluating string theory, the most important question is to ask if the research programme is progressing from a broadly Lakatosian perspective (Cartwright & Frigg, 2007). In a popular and short piece, 'String theory under scrutiny', Cartwright and Frigg (Cartwright & Frigg, 2007) draw on a simplified history of philosophy of science to claim that the most important question to asked of string theory is whether the research program is progressive (Cartwright & Frigg, 2007, p. 15). Cartwright and Frigg claim that sting theory is progressive in terms of elegance, simplicity as well as some sign of progress in unifying and explanatory power (Cartwright & Frigg, 2007, p. 15). Contrasting string theory with Maxwell's unification of electricity and magnetism they find it lacking in empirical applications. However, they acknowledge that this may an unfair to the aims of string theory. Cartwright and Frigg therefore argued that the question becomes one of truth and on this front string theory fairs poorly given its lack of predictions.

Despite advocating for the cogency of Lakatosian perspective Cartwright and Frigg are unable to come to a definite conclusion. However, in contrast with Rovelli's conception of progress, they identify truth (identified by confirmed predictions) and unifying and explanatory power as important elements of progress, but question whether unification and simplicity may be considered sufficient for progress. Consistent with Cartwright's other work (Cartwright, 1999) the paper questions simplicity and unification as an arbiter between theories. The paper concludes with a slightly negative although nuanced appraisal of string theory, recognising that Lakatos claimed that budding research programs should be treated leniently but asking how long this can go on in practice (Cartwright & Frigg, 2007, p. 15).

Castellani's chapter also presents an arguments as how philosophers should evaluate progress in (early) string theory. Castellani claims that early string theory provides an interesting example of a program that has both periods of apparent progress and as well as times in which the program subsides. Whilst there have been many well-known attempts to descriptively or normatively construct a philosophy of progress in science, descriptive and normative treatments of programs that fail have not received the same attention and typically focus on a construal of negative progress (such as degenerating research programs). Castellani points to historical episodes where early string theory was considered to be a failed program (Castellani, 2012, p. 65). Castellani also points to periods during early string theory in which theoretical progress was considered to occur, examining the role of generalisations, analogies and conjectures (often occurring concurrently in the history) (Castellani, 2012, pp. 67-74). In an example of point perhaps made for a non-philosophical audience, Castellani claims that the early string theory case study provides evidence for “pluralistic scientific methodology” in that the evidence shows “once again, how traditional methodological schemes for describing scientific progress are too rigid and limited” (Castellani, 2012, p. 998). This claim is not novel, as Castellani writes, to those acquainted with the philosophy of science literature since the 1970s so may be considered to be directed to some string theorists (and critics) whose awareness of more recent developments in philosophy of science is often not apparent.

Castellani argues that the history of early string theory is rich with examples of decisive conjectures, discoveries, and the corroboration of results providing additional “data” to examine traditional problems in the philosophy of science (Castellani, 2012, pp. 73-74). In particular Castellani claims that the detailed recollections contained within the book provide a philosopher with the rational steps as outlined by each author which “undoubtedly speak in favour of some ‘rationality in scientific discovery” (Castellani, 2012, p. 75). Offering one example, the alternate paths taken by Susskind, Nielsen and Nambu in conjecturing that the underlying dynamics of dual theory, Castellani asserts that an argument for the rationality of discovery is strengthened by three independent individuals reaching the same place (Castellani, 2012, p. 75). Whilst there is no support offered to back up this assertion, this is not Castellani’s stated intention (which is to point to potentially fruitful areas of inquiry). Castellani also presents an argument as to how progress should be evaluated (Castellani, 2012). Castellani argues that the historical case studies that have typically informed a notion of progress have been success stories. Instead Castellani argues instances of failed programmes should be informative in developing a conception of progress. Ultimately, Castellani argues for a conception of progress that draws upon a pluralistic conception of scientific methodology (Castellani, 2012, p. 998).

In ‘On the Foundations of Superstring Theory’, ’t Hooft finds a series of ‘miracles’ and ‘disappointments’ to construct a normative understanding of progress based on methodological norms that either stimulate or constrain progress (’t Hooft, 2013). He considerers questions of the foundations of string theory to have wider importance beyond philosophy, arguing that “it is of tantamount importance to carry out as many critical investigations as is possible, to analyse this situation and to reach an agreement that is no longer disputed by a vast majority of the experts” (’t Hooft, 2013, p. 47). Here ’t Hooft is careful to clarify whom should be considered part of the community of experts with a snide remark directed at outsiders: “string theory has been, and will always be, disputed by numerous onlookers in the sideline who failed to grasp many of its subtle technicalities. It goes without saying that we ignore them” (’t Hooft, 2013, p. 47). However the crux of ’t Hooft’s argument is that foundational issues should be addressed rather than what he perceives as a methodology couched in avoidance. The practitioners of string theory are characterised by ’t Hooft as following a methodology of discovering ‘stringy miracles’, of which he includes black hole microstates and cosmological scenarios, and if the ‘logical jumps’ are not comprehensible they are labelled ‘conjectures’. Arguing that this an easy way to score success, ’t Hooft criticises this methodology as blocking “true understanding” and says that “it may well form an obstacle against further progress in the future” (’t Hooft, 2013, p. 47).

Whilst ’t Hooft claims that his paper is not a criticism of string theory (’t Hooft, 2013, p. 47), there are critical passages throughout. The critique is not directed at the failure of string theory to ‘explain’ the Standard Model and the values of the fundamental constants or the failure to develop a “definitely testable prediction”, ’t Hooft is of the position that “such explanations and predictions are still way out of reach for respectable theories of physics” (’t Hooft, 2013, p. 47). This is consistent with an earlier paper authored by ’t Hooft advocating for non-empirical methodologies under certain conditions (’t Hooft, 2001). The critique is directed at an absence of a clearly articulated theory of string theory. Perturbative string theory is criticised for “not defining a theory” (’t Hooft, 2013, p. 50). The duality relationships are identified as ‘artillery’ against the lack of a non-perturbative formulation of string theory. However, ultimately ’t Hooft finds the duality relationships between string theories inadequate where the string theories lack “rigorous foundation” (’t Hooft, 2013, p. 50).

A final point of criticism is the number of distinct compactifications of the additional dimension each of which yield a corresponding universe: “not only is this unsatisfactory; it is something of a disaster for the theory, because the compactification ambiguity leads to a permanent large-scale ambiguity in the realization of these theories” (’t Hooft, 2013, p. 51). This critique differs from the points he had made prior which focused on the lack of explicitly formulated foundations to string theory. Instead this is criticism of a ‘result’ of string theory that ’t Hooft thinks may signal that “the ‘true theory’ is something totally different from string theory” and instead that one or more of the realisations of the compactifications of string theory should be thought of as an approximation. Each variant of ’t Hooft’s critique may be considered to be premises for his ultimate argument:

1. A more solidly founded structure of string theory must be sought.
2. The lack of selection mechanism for the landscape of string theories is unsatisfactory.

Therefore we should consider string theory, not as a theory of quantum gravity but rather as a general mathematical framework for a class of theories. (’t Hooft, 2013)

As such the paper amounts to an attempt to characterise string theory with reference to a series of methodological norms. The characterisation, as set up by ’t Hooft acknowledges but does not consider that the ‘explanatory’ and ‘predictive failures’ are relevant. ’t Hooft’s appraisal of string theory draws upon a non-empirical conception of progress, where a lack of rigour and sources of ambiguity in the theoretical construction of string theory are constitutive of blocking progress.

Mike Duff too draws upon a non-empirical conception of progress but comes to an alternate conclusion. The invited contribution is written as a response to a debate in which he participated in with Nancy Cartwright and Lee Smolin[13] (Smolin et al., 2007). In 'String and M-theory: Answering the Critics', Duff attempts to counter various critiques of string theory and show "why we need a unified theory of the fundamental interactions and why string and M-theory currently offer the best hope" (Duff, 2013, p. 182). In support of this position, Duff evaluates string theory on the basis of 'theoretical progress', where progress is understood by Duff as constituted by solved problems. Duff draws upon one of the examples relied upon by 't Hooft, but he instead argues that this example constitutes progress: "by providing the first microscopic derivation of the black hole entropy formula first proposed by Hawking in the mid-1970s. Solving long outstanding theoretical problems such a[s] this indicates that we are on the right track" (Duff, 2013, p. 184). The contrasting positions of Duff and 't Hooft highlight the ambiguity of the significance of solving theoretical problems for progress. See (Camilleri and Ritson, 2015) for more on this ambiguity with reference to the string theory debates.

The unsatisfying indeterminate conclusions drawn by Cartwright and Frigg (2007) and Johansson and Matsubara (2011), when drawing upon a Lakatosian perspective of progress to characterise string theory, is further highlighted by authors such as 't Hooft and Duff who are able to come to definite positions but invoke different understandings of progress. Like Castellani, Dawid (2013) has called for progress to be reconfigured. He argued that in the case of string theory traditional conceptions of progress, which he calls a "canonical understanding of scientific progress" (Dawid, 2013b, p. 93), do not match how progress is evaluated by current physicists and therefore should be updated (Dawid, 2006, 2007, 2013b). This highlights the lack of a thematic framework by which philosophers may explore progress in contemporary scientific practice. An updated model of progress needs to be descriptively accurate. This includes an appreciation of a pluralistic scientific methodology and non-empirical theory assessment. However this task of updating a philosophical conception of progress is likely to be difficult given the competing notions of progress identified by current practitioners such as Duff, 't Hooft, and Rovelli. Present within the literature are arguments for how philosophers should understand progress based on string theory as a case study and arguments for appraisals of string theory each based on a different understanding of progress. This further highlights the divide in the literature as to the normative role of philosophy of science.

1.4 Arguments for a change in methodology

Several authors make the descriptive claim that the methodologies employed in string theory, and in particular the non-empirical methodologies employed in string theory, should be interpreted as being distinct from previous conceptions of scientific methodologies. Several authors also claim that the

[13] The debate occurred as part of Smolin's book tour promoting *The Trouble with Physics* in which Smolin agreed to speak on the condition that string theorist was also present.

controversy over string theory should be interpreted as evidence for a change in methodology. The divergent normative conclusions drawn from this perceived shift in 'the scientific method' are often dependent on the string theory in taken to play by the author.

As early as 1995 Galison was the first to argue that the controversy surrounding string theory was evidence for a "profound and contested shift in the position of theory in physics" (Galison, 1995b, p. 372), one that would have been incomprehensible in the 19th or even early 20th century. Instead of locating their defence in the constraints of experiment, practitioners argue for the constraints provided by theory – most importantly, consistency (Galison, 1995b).

Physicist turned philosopher Richard Dawid's first contribution to the philosophical literature was 'Underdetermination and Theory Succession from the Perspective of String Theory' (Dawid, 2006). Dawid begins the paper with his case for a philosophy of string theory, arguing that despite the theoretical incompleteness of string theory and both its current and potential future inability to make contact with experiment, we should not wait for it to become a 'mature' science (Dawid, 2006, pp. 1-2). Dawid points to the dominance and prevalence of string theory in the high energy physics community and the potential for string theory to become a "highly influential field of science for many decades to come" (Dawid, 2006, p. 2). He further argues that the prominence of string theory (despite lacking claims to 'traditional' theoretical acceptance criteria) indicates that it is plausible that a paradigm shift has occurred in high energy physics due to "size and duration of the 'string-phenomena'" (Dawid, 2006, p. 2). Essentially Dawid mounts a quasi-sociological argument for a philosophy of string theory. Implicitly, he also argues that string theory should exert an influence upon philosophical notions of what constitutes science. His argument for the legitimacy of string theory as a case study for the philosophy of science is also a characterisation of the scientific status of string theory.

Dawid offers up a concept he calls 'scientific underdetermination':

> "The claim of scientific underdetermination in a certain field at a given time asserts that it would be possible to build several or many distinct theories which qualify as scientific and fit the empirical data available in that field at the given time. Since these alternative theories are merely required to coincide with respect to the presently available data, they may well offer different predictions of future empirical data which can be tested by future experiments." (Dawid, 2006, p. 4)

Based on this principle, Dawid identifies two types of theories: *well-established* theories that have had their predictions empirically confirmed and *speculative* theories which have not. He then concludes that because string theory is well-established, the 'principle of scientific underdetermination' has been significantly devalued by the absence of alternatives. Dawid's next claim is that the self-confidence that has led to string theory being well-established is drawn from string theory being the only choice (Dawid, 2006, p. 5); an inductive argument drawn from the empirically successful Standard Model; and the internal coherence of string theory. He also claims that the combination of all three arguments is used by string theorists to claim that ‘scientific underdetermination’ has been “devalued” (Dawid, 2006, p. 9).

Dawid then asks what would be the consequences for the scientific underdetermination principle if string theory were successful. Dawid claims that “the only structurally unique theory known in science today is string theory” (Dawid, 2006, p. 11) and if successful will be highly predictive. He therefore concludes that we can expect the theory never to be replaced:

> “This suggests the termination of the progressing sequence of scientific theories. It must be expected that a highly predictive structurally unique theory that fits the present experimental data should describe all future experiments correctly as well. The pessimistic meta-induction thus fails and one must feel compelled to call any empirically successful structurally unique theory a serious candidate for a final theory.” (Dawid, 2006, p. 12)

This final theory claim is used to further weaken scientific underdetermination and to argue for “a new conception of scientific progress” (Dawid, 2006, p. 14). David characterises progress in science not by theory change and theory choice, but instead by intra-theoretical progress (Dawid, 2006, p. 15). Dawid argues that the classical notion of theory confirmation should be reassessed due to the devaluation of ‘scientific underdetermination’, which has implied a significant increase in authority for purely theoretical confirmation (Dawid, 2006, p. 15). This final step, where Dawid argues that the devaluation of scientific underdetermination implies a rise in the authority for theoretical confirmation, amounts to a circular argument. Dawid argues that there is an increase in authority for purely theoretical confirmation in the case of string theory. The premises for his argument are that scientific underdetermination has been devalued and that this is on the basis that string theory is well-established. Without a clear understanding of the how two claims, that string theory is well-established and that there has been a rise in confidence in theoretical confirmation, are distinct the argument is begging the question.

Dawid further developed his position in his 2007 paper ‘Scientific Realism in the Age of String Theory’ in which he aimed to do two things: to describe a newly emerging principle he calls the principle of theoretical uniqueness; and to show how, based on this principle, string theory is suggestive of a realist claim that is quite similar to structural realism (Dawid, 2007, p. 1). Dawid uses the vague word ‘suggest’, which enables him to switch between two claims throughout the paper: in parts he appears to be describing a new attitude (meta-paradigm (Dawid, 2007, p. 8)) among physicists, and in others he seems to be constructing a stronger argument in favour of his concept of ‘theoretical uniqueness’ and how this supports his version of structural realism. This is reliant upon characterising string theory as transforming our understanding of what is considered to be ‘scientific’ (Dawid, 2007, pp. 6-8).

Dawid again draws upon his concept of scientific underdetermination (Dawid, 2007, p. 9). Essentially Dawid defines scientific underdetermination so as to claim that all known data can be explained by a multitude of theories and that we can only make a ‘truth claim’ about these theories by testing their predictions against nature. Dawid acknowledges the existence of other forms of underdetermination (Quinean and Humean) but makes no comment as to their relevance or impact (and somewhat intriguingly makes no mention of Duhem) (Dawid, 2007, p. 9). Dawid argues that while scientific underdetermination “seems to be a matter of fact” the degree to which it has an impact on scientific progress is “highly non trivial” (Dawid, 2007, p. 10). Limitations, such as the predictive success of science, combined with a no miracles argument, may give some confidence in selecting an empirically unconfirmed theory (Dawid, 2007, p. 10). Dawid’s notion of ‘confidence’ is tied to his concept of “scientific underdetermination” in that he argues that belief in a theory as “true in a literal sense” (Dawid, 2007, p. 8) increases with belief scientific underdetermination is limited (Dawid, 2007, p 13).

Dawid applies his general argument for confidence in an empirically unconfirmed theory to the specific case of string theory by reiterating his earlier arguments: the argument of no choice; the precedent set by the Standard Model of particle physics; and the internal coherence of string theory to signal limitations to scientific underdetermination (Dawid, 2007, p. 11). For Dawid:

> “…in the eyes of its exponents, string theory provides reasons for assuming unusually strong limitations to scientific underdetermination which justify a higher emphasis on theoretical arguments for evaluating the viability of scientific statements.” (Dawid, 2007, p. 10)

Note that the arguments have remained the same but the language has changed – Dawid has gone from offering up support for the ‘devaluing’ of scientific underdetermination to offering up support for the ‘limitations’ of scientific underdetermination. The strategy of the argument has shifted slightly; while still semi-sociological – the argument is still phrased as being “in the eyes of its exponents” – the claim is less focused on the role of the exponent and the values they hold and instead focuses on the function of the argument: limitation. This is a stronger argument for confidence in string theory, rather than a claim of identifying a pattern of reasoning.

In 2009 Dawid published ‘On the Conflicting Assessments of the Current Status of String Theory’ to further develop his previous concept of a meta-paradigmatic rift in the physics community (Dawid, 2009). One problem with the paper is a lack of primary source material: in attempting to characterise the debate Dawid draws upon only four books written by physicists (Greene, 1999a; Penrose, 2004; Smolin, 2006c; Woit, 2006d) and oversimplifies what we will see is a complicated debate.

Dawid draws on Kuhn (Kuhn, 1962) and builds on his previous characterisation of the dispute – as a meta-paradigmatic rift between two opposing sides (Dawid, 2009, p. 984). Dawid aims to define the conflict with the goal of contributing to the fertility of the debate as well as revealing new insights into scientific progress (Dawid, 2009, p. 986). In order to do so, he elects to ignore the fact that Lee Smolin once worked in string theory (Dawid, 2009, p. 987). Dawid justifies this by claiming that "the divergent position of one individual scientist would be of limited interest to the philosophy of science field and would not suffice to motivate the fundamental debate that arose in recent years" (Dawid, 2009, p. 988). I claim that this move is not justified for two reasons. First, Dawid has chosen to focus only on the 'popular' literature and Smolin's 2006 popular book, *The Trouble with Physics: The Rise of String Theory, The Fall of a Science, and What Comes Next*, had a significant impact. In addition to this, as one of only three listed sources of critique, Smolin as one individual scientist makes up a significant percentage of critical sources. Furthermore the critique presented by Smolin is described by Smolin himself to be partially motivated by his initial work in string theory and why he changed his mind. Secondly, avoiding sources of complexity undermines an attempt to accurately characterise a debate and results in oversimplification.

Dawid claims that under a scientific paradigm scientists share a notion of what may be considered scientifically legitimate (Dawid, 2009, p. 986). He therefore characterises the dispute over string theory as being a "classical" paradigm in conflict with an "emerging paradigm" (Dawid, 2009, p. 986, p. 993) where both sides agree on the problems string theory faces but differ as to the conclusions drawn (Dawid, 2009, p. 987). Three interrelated concepts – success, progress and empiricism – are evaluated in different terms in each paradigm (Dawid, 2009, pp. 988-991). The dispute over string theory, according to Dawid, broadly stems from the lack of contact with experiment and issues of empiricisms. According to Dawid's description, only the "old" paradigm or the "classical paradigm" finds the lack of empiricism to be in conflict with standards of scientific legitimacy. The "emerging" or "new" paradigm, while acknowledging the situation, does not come to an appraisal of conflict with standards of scientific legitimacy. Also implicated are sociological factors, where the dominance of string theory is used both against and for string theory (Dawid, 2009, pp. 989-990). Dawid argues that physicists who have not worked in string theory cannot appreciate the new paradigm as "string physicists have been led toward a novel conception of scientific theory appraisal by their scientific research, which they had carried out in accordance with all standards of scientific reasoning" (Dawid, 2009, p. 992). Thus those who did not experience the shift find no justification for it and remain in the 'classical paradigm'. This is where it is problematic that Smolin's previous work in string theory is disregarded, as Smolin did for a time work within what Dawid calls the emergent paradigm. Yet, despite this, Smolin has been critical of string theory.

Dawid concludes that the success of string theory and the new paradigm are bound together; consequently string theory will not be accepted without the acceptance of the new paradigm (Dawid, 2009, pp. 992-995). One problem is that Dawid does not consider what has led other theories of quantum gravity to gain traction, given that the lack of connection with experiment is a problem for any theory of quantum gravity. Even if Dawid argues that string theory is the 'dominant' theory of quantum gravity, he cannot deny that that others, such as loop quantum gravity, have also obtained some measure of acceptance in the high energy physics community, or at the very least have been considered worth working on. Furthermore, there is a long history of attempts at theories of quantum gravity that have been rejected, not on the basis of a lack of connection with experiment, but on the basis of theoretical weaknesses, i.e. non-renormalisable theories.

Dawid attempts to characterise assessment in the "new" paradigm in 'Theory Assessment and Final Theory Claim in String Theory' (Dawid, 2013b). The stated aim of the paper is to outline a non-empirical "conception of theory assessment" (Dawid, 2013b, p. 81) based on the descriptive claim that this "new" conception of theory assessment, characterised by an absence of empiricism, has emerged in the string theory community, constituting an "new" paradigm in high energy physics. The goal of the paper is therefore to understand "the character of this new paradigm" (Dawid, 2013b, p. 82). The conclusion reached is that the theory assessment, as described within the paper, should be understood as 'rational' and, as such, the paper deviates from the aim of description into a normative argument for the legitimacy of argument that string theory is a final theory (Dawid, 2013b, p. 97). As such, contrary to the aims stated in the paper, I argue the paper should be considered to be an attempt to legitimate the claim that it is rational to believe that string theory is a final theory.

The paper draws upon concepts developed in Dawid's previous contributions to the literature: his notion of 'scientific underdetermination' (Dawid, 2006); the 'argument for no choice' from (Dawid, 2007) is renamed the 'argument of no alternatives'; and the descriptive argument for a paradigm shift (Dawid, 2009). The paper is also similar to his earlier work (Dawid, 2007) in that it often slips from descriptive claims of arguments offered by string theorists to normative claims concerning the rationality of this kind of reasoning. Dawid presents the string theory community as unified in its pro-trust arguments (Dawid, 2013b) (as he did in his 2009 paper, in which he argued for the existence of a new paradigm), but he offers up little evidence of the existence of individual string theorists making these arguments. The only offerings are Polchinski's textbook (Polchinski, 1998), in relation to which Dawid claims that "all arguments to be discussed can be identified in a more or less explicit form in Polchinski's classical textbook" ((Polchinski, 1998) cited in (Dawid, 2013b, p. 87)), and the books (Greene, 1999b; Hawking & Mlodinow, 2010; Kaku & Thompson, 1997) to which he refers in support of the existence of the argument for a final theory. Similarly, throughout the paper Dawid makes several references to a 'canonical understanding of scientific progress', but does not attribute this position to an author or authors. It is therefore difficult to understand against which position he is setting up his comparison.

The argument is divided into three parts beginning with section two: 'The Role of Scientific Underdetermination' (Dawid, 2013b, pp. 83-85). In this section, Dawid outlines an argument for the role of 'scientific underdetermination' in theory assessment, defining this style of reasoning as:

> "Assessments as to how likely it is that no or few alternative theories can be fit to the available data thus lie at the root of all considerations regarding the prospective viability of a so far empirically unconfirmed or insufficiently confirmed theory. We want to call such assessments 'assessments of scientific underdetermination'." (Dawid, 2013b, p. 83)

Dawid states that in the 'canonical understanding' of scientific progress, assessments of scientific underdetermination were only considered to have played a role in the context of discovery as assessments of the viability of a theory. Dawid argues that if "assessments of underdetermination are capable of raising the probability for a theory's viability it seems difficult to deny that assessments of scientific underdetermination, if powerful enough, can amount to establishing (probable) scientific knowledge" (Dawid, 2013b, p. 85). On this basis Dawid argues that it would irrational to reason that assessments of scientific underdetermination can be made for the viability of a theory and then not also employ this reasoning in the 'context of justification'.

In the third section, 'Trust in String Theory', Dawid outlines one context in which "assessments of underdetermination have attained a status in contemporary fundamental physics that makes them a crucial element of the acquisition of scientific knowledge" (Dawid, 2013b, p. 85). Dawid briefly outlines the "theoretical incompleteness" and "precarious empirical status" of string theory and remarks that it is remarkable that string theory has had such a "highly influential position", and that there is a high degree of "trust" in string theory (Dawid, 2013b, p. 86). It is this "high self-confidence" that Dawid locates as the point of conflict in the debates over the status of string theory. Dawid argues that "the dispute on the status of string theory is *in fact* based on diverging understandings of the role of assessments of scientific underdetermination in science" (Dawid, 2013b, p. 86) (emphasis added). It is here that the lack of historical evidence is a significant flaw in the argument; no support is offered for the strong claim that it is a *fact* that the basis of the disputes over string theory are as Dawid asserts.

Dawid argues that there are three arguments for the trust in string theory. The first argument is 'the argument of no alternatives' (or that there currently exists no competitor theory of quantum gravity that also unifies the fundamental forces[14]). Second is 'the argument of explanatory interconnections', which suggests that the interpretation of black hole entropy and connections to supersymmetry and supergravity would be 'miraculous' had they not arisen from a theory that was 'entirely misguided'. The final argument concerns 'the meta-inductive inference from other cases of predictive success'

[14] Dawid's argument for why loop quantum gravity is not a competitor is outlined in (Dawid, 2013b, p. 87) and is discussed in the corresponding section below.

which draws the first two arguments together to claim that, historically, the Standard Model was once a non-empirical theory that had no alternatives and provided unexpected explanatory interconnections, but eventually become empirically verified. The inductive argument is to infer that the two arguments are 'reliable indicators' of viability of a theory (Dawid, 2013b, pp. 87-91). This leap to trust in string theory is only permissible in the absence of empirical confirmation where all three conditions are satisfied, as each argument is not argument is not sufficient in isolation (Dawid, 2013b, p. 92). The three arguments offered are nominally Polchinski's arguments, but, in the absence of convincing evidence that Polchinski actually offers these arguments, the arguments may be interpreted as Dawid's own and, as such, the paper can be read as a normative appraisal of string theory. This does not preclude the possibility that certain string theorists do in fact make these arguments;[15] I only argue that this renders Dawid's descriptive claims unconvincing.

In the fourth section, 'The Final Theory Claim', Dawid argues "that the rising importance of assessments of scientific underdetermination devaluates the epistemic arguments against final theory claims and thus gives new credence to the latter" (Dawid, 2013b, p. 93). The way in which Dawid describes epistemic arguments against final theory claims in general resembles the argument for pessimistic meta-induction (Laudan, 1981):

> "Statements on a theory's full universality can only be made with respect to the set of phenomena known at the time. Such statements do not imply, however, that new phenomena which reach beyond the allegedly universal theory cannot be discovered in the future". (Dawid, 2013b, p. 93)

In the particular case of string theory, the epistemic argument against a final theory claim is that any reason to believe that string theory is a final theory must be based on string theory itself and as such cannot establish that an alternate theory will not supersede string theory. Dawid argues that these arguments are flawed in that they rely on the 'canonical paradigm of theory assessment' that "can never address the question of finality", as the in principle data set will always be greater than the available data set (Dawid, 2013b). This argument is not explained further, which is problematic because, without further clarification, it reads as simply restating the 'epistemic argument' and then draws a different conclusion.

Problems with the negative argument aside, the focus of the section is on the positive argument for the role of scientific underdetermination and it is here that Dawid draws together all the strands of his argument, the premises of which I take to be:

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[15] The many arguments concerning the dominance of string theory will be examined in chapter three. In particular the arguments for string theory as a worthwhile pursuit versus string theory as pathological will be examined.

1. That the "canonical paradigm of theory assessment does not square with the actual research process in contemporary high energy physics" (p. 93) and that there is a "shift in the overall dynamic.
2. There exists "rational arguments" for "local limitations to scientific underdetermination" (p. 93).
3. That "a final theory claim must be based on assessment of 'global' limitations to underdetermination" (p. 93).
4. That "the final theory arguments presented … can be understood as arguments which block the distinction between local and global underdetermination" (p. 95) on the basis of the following:
   a. In the case of "a fundamental theory that is fully universal in the sense that it covers all information on parameter values which determine the size of known phenomena" (p. 95), and there does not exist an empirically distinguishable alternative theory, this "significantly reduces the options for alternative theories which do not affect local limitations to scientific underdetermination" (p. 95).
   b. The potential for an alternate theory that differs in energy levels not experimentally accessible is excluded by "the argument for a minimal length scale based on duality" because "there is no way to make sense of the statement that this theory remains viable beyond its characteristic scale but stops being viable far beyond that scale" (Dawid, 2013b, p. 95).[16]
5. Thus the "arguments which suggest limitations to local scientific underdetermination must be acknowledged as arguments against unlimited global scientific underdetermination as well" (Dawid, 2013b, p. 96) which, according to premise three above, support a final theory claim.
6. Furthermore "the viability of internal final theory claims can be related to empirical data via the meta-inductive argument and thereby can attain a certain degree of trust" (p. 96).

As I understand it, the argument can be summarised as follows: on the basis of the descriptive claim that there is a new research process occurring in string theory; that there are rational arguments for string theory that limit local underdetermination (the no alternatives argument; the argument for unexpected explanatory interconnections; and the meta-inductive inference from other cases of predictive success); and that string theory is a global theory, it is rational to trust, to a certain degree, that string theory is a final theory.

The problems with the descriptive claims are outlined above. The problems are with the rational reasons for belief that string theory will now be explored. Even if taken as independent argument, I

[16] The following section (1.5) will examine other interpretations of the duality relations including arguments against structural realism on the basis that the duality relationships are an example of underdetermination (this notion of underdetermination is significantly different to Dawid's 'scientific underdetermination').

argue that Dawid has produced a version of the 'success argument' (Putnam, 1975) where he has argued that in the past the reasoning identified has resulted in empirical successes, therefore the only explanation for that reasoning is that there is some basis for the reasoning. He does this quite explicitly in his conclusion where he draws on his argument of 'meta-inductive inference from other cases of predictive success' and states:

> "The predictive success of scientific theories in the research field constitutes a necessary precondition for the viability of any other argument on scientific underdetermination by establishing the connection between entirely theoretical assessments and the performance of theories in an empirical context". (Dawid, 2013b, p. 98)

However, rather than interpreting his success argument as an argument for some version of scientific realism (as he does in (Dawid, 2007)), Dawid argues that the "shift in the overall dynamic of the research process" is a rational shift. Many of the counter arguments to the success argument may be offered up to counter Dawid's argument, such as examples where there was no alternative, surprising theoretic connections and an approach that was derived from an empirically successful theory. Examples include the belief that the universe was not expanding at an accelerating rate, and Weyl's attempt to unify electromagnetism and general relativity by constructing a gauge invariant geometry (that was empirically unsuccessful).[17] This follows the strategy employed by Laudan in his argument for Pessimistic Induction (Laudan 1981). Current alternate examples that both conflict to a certain extent with string theory and also fit Dawid's criteria can also be provided; this is the critical strategy employed by Smolin in his review of *String Theory and the Scientific Method*, where he argued that Dawid's arguments apply equally well to loop quantum gravity (Smolin, 2014b).

Dawid claims that loop quantum gravity is not genuine alternative to string theory as it is not a unified theory (2013b, p. 87). By contrast Smolin, in his argument, claims that there are no viable alternatives to solving the problem of quantum gravity other than loop quantum gravity. This highlights an inherit ambiguity in the 'No Alternatives Argument'. Whilst Dawid presents it as fact that there are no alternatives to string theory, it is not known *a priori* that the solution to the problem of quantum gravity will be a unified solution. Without further justification, this criterion used to exclude loop quantum gravity is arbitrary.

An additional problem is that Dawid's argument is a weak argument in favour of the rationality of pursuing string theory. This argument is weak in two senses: first, very few critics of string theory argue that it is not rational to pursue string theory; they see value in other approaches but do not argue that all research into string theory should cease, and so the argument does very little to defend string theory from existing critique. Secondly, the argument does not do justice to the complex web of

[17] For introductory historical details, see (J. L. Bell & Korté, 2015). For details concerning unification see (Morrison, 2000, pp. 114 - 121).

arguments pertaining to positive non-empirical theory assessments that, instead of arguing that it is rational to pursue string theory, argue that string theory is the most promising approach to understanding the problem of quantum gravity (where this assessment is based the fruitfulness rather than as a candidate final theory). These arguments will be examined in chapter five.

An argument against a change in methodology came from Gubser in 2013, who argued that string theorists *are* committed to ordinary standards. It is the opening section of Gubser's paper, 'In Cautious Praise of String Theory', that is of most interest (Gubser, 2013, pp. 140 - 141). As a brief introduction into the conflict over string theory, Gubser opens with quotes of the contrasting appraisals of string theory such as "string theory is (a candidate for) the 'theory of everything' vs. string theory is 'not even wrong'" (Ellis and Woit quoted in (Gubser, 2013, pp. 140 - 141). Gubser find the arguments mounted by the critics of string theory unconvincing as they do not undermine arguments offered up by string theorists in the positive appraisal of string theory, namely that "string theory is uniquely positioned to solve the biggest problems in fundamental physics" (Gubser, 2013, p. 141). Gubser contends that instead of refuting this claim, the critics condemn string theory on the basis that if fails to meet "ordinary standards, norms, and principles of theoretical physics", because it is "untested and excessively mathematical" (Gubser, 2013, p. 141). This position, Gubser argues, is based on a mischaracterisation of string theory, and as such amounts to a straw man argument. The mischaracterisation is not in the description of string theory as 'untested' or 'excessively mathematical' but in the description of string theory as uncommitted to ordinary standards.

Gubser proposes that perhaps the string community has not been as open as it might be about the concerns facing string theory and that this has given critics a window of opportunity. Despite this, Gubser argues that the concerns are widely held in the community. In contrast to Dawid's characterisation of string theorists in the debates, Gubser argues that "of course ordinary standards *should* be applied to string theory, no matter how amazing its theoretical reach might become" (Gubser, 2013, p. 141) (italics author's own). The weakness of Gubser's argument is precisely that of which he accuses his opponents: he mischaracterises the position of the critics. One mischaracterisation is that he lumps all the critics of string theory into holding a single position, and, secondly, he oversimplifies the critique of string theory down to the proposition that string theory is "untested and excessively mathematical" (Gubser, 2013, p. 141).

Dawid's *String Theory and the Scientific Method* was the first book-length treatment by a single author dedicated to a philosophical interpretation of string theory (Dawid, 2013a). As much of the content has already been addressed,[18] I will attempt to be brief in my explication of the position offered by Dawid and indicate where I believe his position has shifted or been expanded.

A new inclusion to Dawid's argument is clarification of the role he wishes the dispute over string theory to play as non-essential for his conclusion. Instead Dawid takes the dispute over the status of string theory as "something philosophically interesting [that] is happening in physics today that is capable of creating serious divides within the physics community at a deep conceptual level" (Dawid, 2013a, p. 19). It is on the back of this that Dawid claims that the dispute is intended to serve as a 'test case' for the argument within the book. Therefore if the book does not provide a convincing explanation of the dispute, then the argument still stands as a description of an emergent conception of the scientific method. This clarification helps to expose the one-sided description of the string wars. Dawid is not as concerned with the various positions outlined by the critics as he is ultimately concerned with what he sees as a new scientific method which is being practised and/or advocated for by string theorists. Dawid examines only part of the critiques outlined by Penrose and Smolin, considering them as representative of all critics, and even then ignores elements of the their positions (see footnote 19 in (Dawid, 2013a, p. 22)) to present an overly simplified picture of the critics of string theory.

Dawid's descriptive claim – that string theorists employ the 'Argument of Unexpected Explanatory Coherence', the 'No Alternative Argument', and the 'Meta-Inductive Argument' – is improved in the book length version. This improvement comes from the explicit identification of a paper in which Dawid claims Polchinski's argument amounts to conceptualisation of the 'No Miracles Argument' ((Polchinski, 1999) cited in (Dawid, 2013a, p. 32)). Dawid argues that:

> "Three main contextual reasons for the trust string theorists have in their theory may be distinguished. While all three arguments are "common lore" among string physicists, it is difficult to pinpoint a "locus classicus" for each of them." (Dawid, 2013a, p. 31)

Instead of a 'locus classicus', Dawid points to the same references utilised in (Dawid, 2013b), chapter one of (Polchinski, 1998) and chapter one of (Greene, 1999a), but these examples remain unconvincing for two reasons. First, there is no descriptive argument that refers to the content of the chapters and, second, there is no argument offered as to why the chapters should be considered representative of the community of string theorists as a whole, especially given that Dawid denies (contra Smolin) that the string theory community follows a small number of "prophets" (Dawid, 2013a, p. 26). This criticism was also identified by Rickles, in his review of *String Theory and the*

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[18] 'Underdetermination and theory succession from the perspective of string theory' (Dawid, 2006) in chapters 1 and 3; 'Scientific realism in the age of string theory' (Dawid, 2007) in chapters 1, 2 and 3; 'On the conflicting assessments of the current status of string theory' (Dawid, 2009) in chapter 1; and 'Theory Assessment and Final Theory Claim in String Theory' (Dawid, 2013b) in chapter 6.

Scientific Method: "If the issue is why string theorists 'really believe' and 'trust' their theory, then surely this demands a thorough analysis of the history and, one would expect, interviews and such like. Instead, there are certain inaccuracies that this ahistorical approach propagates" (Rickles, 2015, p. 4).

For Dawid there are three "crucial" questions he would like to investigate with reference to his three core arguments: the 'Argument of Unexpected Explanatory Coherence', the 'No Alternative Argument', and the 'Meta-Inductive Argument':

> "Can these arguments be called genuine scientific reasoning? Do they amount to theory confirmation? Finally, can we find a philosophical background story behind the described rise of non-empirical theory assessment?" (Dawid, 2013a)

In order to do this, Dawid sets up a dichotomy in the philosophy of science literature between 'classical paradigm of theory assessment', which he claims is "shared by most philosophers today" (Dawid, 2013a, p. 42) and is characterised by a Bayesian approach, and empirical data as essential for belief in a scientific theory. In opposition to this position, Dawid draws upon older literature, *The Structure of the Scientific Revolutions* (Kuhn, 1962) and *Progress and its Problems (*Laudan, 1977), as examples of positions that argued that the role of theory appraisal has not been sufficiently considered.

In essence the structure of Dawid's argument is in two parts and as follows.

> There are two meta-paradigms of theory assessment:
>
> a. The first is characterised by a commitment to traditional empirical methodologies. This meta-paradigm is driven by consideration from the 'classical paradigm of theory assessment'.
> b. The second is characterised by a commitment to the non-empirical methodologies: the 'Argument of Unexpected Explanatory Coherence', the 'No Alternative Argument', and the 'Meta-Inductive Argument'. This meta-paradigm is driven by consideration of 'scientific underdetermination'.

Through an analysis of 'scientific underdetermination' Dawid argues that "limitations to underdetermination are taken to be sufficiently strong for justifying the belief that the theory in question will get empirically confirmed once the critical experimental tests can be carried out" (Dawid, 2013a, p. 62). On this basis I argue that *String Theory and the Scientific Method* outlines a normative argument as to the legitimacy of string theory and as such is best interpreted as taking a side in the string wars as opposed to a potential explanation of the string wars. A similar position is also argued in Benedictus' review, in which he describes Dawid as "an active exponent in the ongoing string theory debate" (Benedictus, 2014, p. 590).

Dawid's book has drawn the attention of some within the physics community. At the 2014 strings conference, David Gross gave a 'vision' talk titled 'A Perspective on String Theory' in which he discussed the arguments for the 'Argument of Unexpected Explanatory Coherence', the 'No Alternative Argument', and the 'Meta-Inductive Argument' and encouraged all members of the audience to read *String Theory and the Scientific Method* (David Gross, 2014). The book was also discussed and reviewed on several blogs including (Hossenfelder, 2015) (Woit, 2013b) and (Motl, 2013). Unsurprisingly Motl's review is mostly positive and Woit's is mostly negative. Woit also wrote a critical review for *Scientia Salon* (Woit, 2014e). Hossenfelder is critical of Dawid's attempt to separate out philosophical and sociological issues – a move she considered to be untenable. These responses are unsurprising as Dawid is offering up a justification of string theory methodology. Hossenfelder is also critical of Dawid's conflation of what she calls 'the Scientific method' with "the rest of the scientific process that happens in the communities" (Hossenfelder, 2015). Ultimately Hossenfelder rejects the conclusion that the scientific method needs to updated (Hossenfelder, 2015). Hossenfelder highlights the importance of the inclusion of the sociological elements of the disputes over string theory. The debate over whether the dominance of string theory may be taken as evidence of scientific judgement or pathological science will be examined in chapter three.

An alternative argument for a change in methodology is present in Kragh's paper 'Testability and Epistemic Shifts in Modern Cosmology' from 2014. Kragh proposes and defines a new term, "epistemic shift", and asks if the introduction of anthropic multiverse cosmology constitutes an 'epistemic shift' (Kragh, 2014). Kragh defines an epistemic shift as a change in "evaluation criteria that conflict with and go beyond those ordinarily accepted" (Kragh, 2014, p. 48). The evidence for an 'epistemic shift' which Kragh identifies are "suggestions that traditional criteria of evaluation of scientific theories (or of theories claimed to be scientific) are no longer adequate and should therefore be replaced" (Kragh, 2014, p. 48). Kragh claims that the implication of a shift in standards of theory choice implies "a new meaning or definition of what counts as science" (Kragh, 2014, p. 48). His notion of an epistemic shift is both less and more radical than a Kuhnian paradigm shift: less radical in that competing standards are not necessarily incommensurable, and more radical in that an epistemic shift may alter the significance of empirical tests otherwise considered to be stable across paradigm shifts (Kragh, 2014, p. 49). To a certain extent, his epistemic shift resembles Dawid's 'meta-paradigm' (Dawid, 2007, p. 8) in that both describe a move away from empiricism and characterise the significance of the move as an 'emergent' or new definition of science.

As a way of further explaining what constitutes an 'epistemic shift', Kragh dedicates a section to examples from the history of cosmology. This serves both to enrich the notion of an epistemic shift and to provide support for the claim that the situation in multiverse cosmology is not unique (Kragh, 2014, p. 49). Kragh also redefines the episodes outlined in *Higher Speculations* (2011) as further examples of epistemic shifts. He then provides a brief history of the rise of the anthropic multiverse as motivated by string theory. He argues that string theory played the role of legitimating the previously unpopular anthropic multiverse when "the string landscape was offered as theoretical evidence" (Kragh, 2014, p. 51). Concerning the issue of the relationship between testability and the multiverse, Kragh argues that:

> "All (or, bearing Eddington and Milne in mind, nearly all) physicists agree that testability is an epistemic value of crucial importance. They consider it an indispensable precondition for a theory being scientific: a theory which is cut off from confrontation with empirical data just does not belong to the realm of science. Testability may admittedly not be relevant to all aspects or in all phases of the development of a theory, but ultimately it cannot be ignored. Multiverse and string theorists are no exception to this *rhetorical consensus* which finds expression time and again in the literature. But one thing is rhetoric, another is scientific practice and the interpretation of the concept of testability." (Kragh, 2014, pp. 51-52) (italics author's own)

Kragh identifies seven questions that he believes "physicists and cosmologists do *not* agree on" (Kragh, 2014, p. 52) (original emphasis). Whilst the list is useful as a broad introduction to the dispute over the role of testability as an 'epistemic virtue', Kragh's lack of attribution of authorship is problematic as it is not clear who, if anyone, asks the questions described by Kragh.

Kragh moves on to discuss the role of Falsificationism in the disputes. In contrast to the previous section, Kragh provides a detailed description of the naïve Popperian rhetoric that is employed in the debates including the rejection of Falsificationism by Susskind, Hawking and Schellekens (Kragh, 2014, p. 53). For Kragh the significance of this dispute for philosophy of science is that it raises the question of "which people or groups have the 'right' to define the rules of science and thus to decide whether or not a particular theory discussed by the scientists is in fact scientific" (Kragh, 2014, p. 54). The central argument of the paper may be summarised as follows: if the controversy over testability and the multiverse is settled such that the anthropic multiverse is considered scientific, then the controversy will constitute an epistemic shift.

One difficulty with the literature that makes the descriptive claim, that the methodology employed in string theory implies a change in methodology, is the very diverse ways of determining potential novelty. Galison describes a "profound and contested shift in the position of theory in physics" (Galison, 1995b, p. 372). For Shapere, string theory is "radical" and "if such theories are accepted or even considered to be legitimate objects of scientific study, regions of the total universe must be admitted which are unobservable in principle" (Shapere, 2000, p. 153). Dawid explicitly argues for novelty: a “meta-paradigmatic rift” (Dawid, 2009, p. 984) where “string physicists have been led toward a novel conception of scientific theory appraisal by their scientific research” (Dawid, 2009, p. 992). Later Dawid further argues that given “the actual situation in string theory” we may conclude that the argument “seems to suggest a shift in the overall dynamic of the research process” (Dawid, 2013b, p. 96). Rickles describes a “revolution in process” (Rickles, 2011a, p. 4). For Kragh it is described a controversy where “the very standards of science are at stake” (Kragh, 2011b) and later as an “epistemic shift” (Kragh, 2014). Kragh identifies the evidence for an ‘epistemic shift’ as being “suggestions that traditional criteria of evaluation of scientific theories (or of theories claimed to be scientific) are no longer adequate and should therefore be replaced” (Kragh, 2014, p. 48). Kragh claims that the implication of a shift in standards of theory choice implies “a new meaning or definition of what counts as science” (Kragh, 2014, p. 48). Against this position, Gubser argues the descriptive claims that “ordinary standards” are being applied to string theory and the normative claim that “ordinary standards should be applied to string theory” (Gubser, 2013, p. 141).

There are two difficulties with arguments that claim that the methodology employed in string theory is novel. It is very difficult to determine at what point something is sufficiently different so as to be considered ‘new’. Are new elements sufficient? Or is something novel only when holistic change has occurred? The authors above are not clear as to how they understand novelty. Does novelty require a holistic change, where there the previous standards are rejected in favour of different and previously unused standards? Dawid draws upon Kuhn’s notions of paradigms but does not specify if he commits to Kuhn’s holistic change during a paradigm shift (Kuhn, 2012, p. 149) or if there may be some continuity between his competing meta-paradigms. This highlights the first difficulty with claims of novelty, should novelty be understood as a claim of a new definition of science or instead as the introduction of novel elements to a conception of science that has continuity. Furthermore several authors use the word ‘shift’, which seems to imply a change has occurred to an alternative state, such that comparison may be made to a previous state. This implies a previous state that was sufficiently static so as to allow comparison rather than an understanding of scientific methodology as something that is continuously evolving. Any understanding of science as defined by a static methodology will face significant difficulties, as Laudan persuasively argued when he proclaimed the death of the demarcation problem (Laudan, 1983). A commitment to a more radical position, like that of Feyerabend (Feyerabend, 2010), is not required to see that the history of science will not permit for a static definition of science.

1.5 How should the string dualities be interpreted?

There is broad agreement within the literature that the string theoretic dualities should be interpreted as significant for core issues in philosophy of science: underdetermination, scientific realism (in particular structural realism), and fundamentality. Despite this broad agreement there is little consensus as to how the string theoretic dualities should be interpreted. Much of the literature is tentative, offering several possible interpretations contingent upon future developments such as (Matsubara, 2013a; Rickles, 2011a; Susskind, 2013) and (Rickles, 2013a; Witten, 1996). There are also several definitions of underdetermination within the literature such as Dawid's construction of the concept of 'scientific underdetermination' (which is present in several of his papers and book) (Dawid, 2006, 2007, 2009, 2013a, 2013b). For Rickles the string theoretic dualities are presented as interesting case studies in underdetermination because they do not fit the "vanilla underdetermination" mould. On this basis I conclude that how the dualities should be interpreted with respect to underdetermination, structural realism, emergence and fundamentality is an open issue present within the literature.

There are two useful general introductions to the philosophical implications of duality and duality in string theory: 'A philosopher looks at string theory' (Rickles, 2011a) and 'Dualities and Intertheoretic Relations' (Castellani, 2010). Castellani examines physical dualities as a potential class of examples with significance for traditional philosophical concerns such as 'what is meant by a theory', understanding intertheoretic relations, fundamentality and realism. The impetus cited for the paper is the 'core role' that duality relationships have played in fundamental physics since the 1960s, particularly in the development of string theory and quantum field theory (Castellani, 2010, p. 9). The paper examines the electromagnetic duality in the case of classical electrodynamics and quantum electrodynamics. Castellani identifies two possible interpretations: the first being the "physicist's received view" in which the duality relationship is one between two expressions of the same theory, the second is that the duality relationship is understood as one between distinct theories. The significance of the first interpretation for philosophy of science, Castellani argues, is that it requires a rethink of the relationship between theory and ontology. The significance of the second is a challenge to theories of realism. Castellani further argues that this latter interpretation represents a class distinct from previous understanding of symmetry (Castellani, 2010, p. 19). The influence of the contribution by Castellani is evident from the next paper to be discussed which builds on this work (Rickles, 2011a, p. 66).

In 'A Philosopher Looks at String Dualities', Rickles outlines various dualities, focusing on the string theoretic dualities, with the goal of establishing some potential philosophical implications (Rickles, 2011a). The paper is a lucid introduction both to the string theory dualities and the potential consequences that may be derived from the string theoretic dualities for well-known avenues of inquiry in philosophy of science, particularly structural realism. Rickles begins with an introduction to the concepts of symmetry, gauge and duality, providing definitions of each as distinct from each other. Symmetries are defined as "structure preserving transformations … that keep the systems state within the set of physically possible states" (Rickles, 2011a, p. 54). Gauge symmetries are defined to be distinct from symmetries in that "they do not map between physically distinct possibilities but map one representation of a state to another representation of the *same* state" (Rickles, 2011a, p. 54) (italics author's own). By contrast, dualities are defined as a class of symmetries that relate "putatively distinct physical theories, rather than states (or solutions) *within* a single theory" (Rickles, 2011a, p. 55) (italics author's own). This distinction is of interest to Rickles because, like gauge symmetries that point to "multiplicity in the descriptive machinery available for some system", duality symmetries generate dual descriptions that, unlike gauge symmetries, are "not in competition for physical reality" (Rickles, 2011a, p. 55). The implications of this characteristic of duality symmetries in string theory are explored with reference to underdetermination, fundamentality and scientific realism. Two advantages of dualities are also identified: the computational value of a relationship that allows a theorist to solve intractable problems in the dual theory and then translate the solution back, and the uncovering of new (possibly non-perturbative) physics (Rickles, 2011a, p. 56).

The nature of the equivalence between two dual theories is further explored by way of further defining a duality relationship. Rickles states that the word 'dual' is used to denote two theories that "generate the same physics, where 'same physics' is parsed in terms of having the same amplitudes, expectation values, observable spectra, and so on" (Rickles, 2011a, p. 56). Hence the two theories are deeply equivalent at both measurable and unmeasurable quantities, a "logical consequence" of which is "observational equivalence" (Rickles, 2011a, p. 56). Following from Vafa (Vafa, 1998), Rickles distinguishes between two, non-trivial, general kinds of duality: internal and external. Internal duality, sometime called self-duality, is where one part of a theory is dual to a distinct, other part of the theory. External duality relates to distinct theories (Rickles, 2011a, p. 57). The paper examines one example of a non-string theoretic duality, electromagnetic duality, and the T, S and Mirror string theoretic dualities (Rickles, 2011a, pp. 57-63). Rickles highlights mirror symmetry as a duality of particular interest to philosophers as it implies that two theories are "physically equivalent with respect to all observables (not just empirically indiscernible)" (Rickles, 2011a, p. 63). The significance of this is argued to be ontological indistinguishability. These sections are a useful resource as a clear introduction for philosophers of science wishing to first acquaint themselves with the string theoretic dualities, including introductions into two focal points in the literature concerning dualities and string theory: underdetermination and consequence for realism and emergence, fundamentalism, and reductionism.

1.5.1 Underdetermination and consequences for realism

Rickles presents the string theoretic dualities as interesting case studies in underdetermination because they do not fit the "vanilla underdetermination" mould. Rickles argues that the crucial difference is that the theories are considered complementary rather than competitive, instead he argues that there are several possible interpretations. A positivist may remain unfazed by the "multiplicity of representational schemes" (Rickles, 2011a, p. 64). An alternative, that Rickles endorses, is to take the string theoretic duality relations as “evidence that there is some deeper set of underlying physical facts of which the string theories are offering glimpses” (Rickles, 2011a, p. 64). Rickles also provides a description of the David Gross’s interpretation: “the web of dualities is taken to restore the uniqueness that was thought to characterize the earliest incarnation of string theory” (Rickles, 2011a, p. 65). Rickles outlines two difficulties with this interpretation: that the conjectured underlying theory, M-theory, to which the string theories under this interpretation would be solutions, has yet to be found. Secondly, he suggests that in the cases where dual theories have different topological spaces, “it is highly non-trivial” to interpret them as “one and the same physical situation” (Rickles, 2011a, p. 65).

The string theoretic dualities are presented as interesting case studies that possibly contradict notions of fundamentality. Rickles explores the example of the S-duality where strongly and weakly coupled theories are considered dual. In this instance “there is no fundamental distinction between elementary and composite descriptions” (Rickles, 2011a, p. 66). The example becomes particularly interesting when applied to string theory, where the S-duality transformation is between different structures. Rickles asks “what structure could possibly underlie this duality, expressing as it does an equivalence between so seemingly different a pair of descriptions as ‘elementary’ and ‘composite’?” (Rickles, 2011a, p. 66) This raises the issue of implications for structural realism. An interpretation of the dualities as pointing to deeper structures, Rickles argues, is not inconsistent with structural realism. However in the case that the theories are underdetermined structurally, Rickles argues that it may not be possible to rescue structural realism by recourse to the potential for a deeper structure (Rickles, 2011a, p. 66). Ultimately Rickles leaves the reader to formulate their own position, having provided a clear outline for future work.

In ‘Realism, Underdetermination and String Theory Dualities’, Matsubara also examines potential interpretations of the string theoretic dualities concerning scientific realism. He concludes that certain interpretations reveal some form of underdetermination and that they spell trouble for structural realism (Matsubara, 2013a). The paper, originally a chapter in Matsubara’s PhD thesis (Matsubara, 2013b), argues that a resolution of semantic issues is essential in order to interpret underdetermination with reference to scientific realism (Matsubara, 2013a, p. 486). The paper takes the common approach of: ‘if’ string theory is proven correct ‘then’ the following would be implied. Matsubara phrases this common ambiguous justification (the ambiguity is located in the complex contingency of what would constitute a ‘proof’ of a theory of quantum gravity) in an alternative but just as ambiguous way: “if [string theory] is taken seriously as a description of the real word” (Matsubara, 2013a, p. 475). Without independent justification, ‘if’ ‘then’ reasoning relies on any conclusion made being contingent upon the success of string theory. Matsubara chooses to ignore the debate over whether string theory is worth pursuing (Matsubara, 2013a, p. 472). One problem with this approach is that the string theoretic dualities are often invoked by participants in the debates, especially the AdS/CFT duality, to argue that string theory is worth pursuing and that string theory should be 'taken seriously' as a description of the real world.

The formulation of underdetermination Matsubara wishes to explore is that "between theories or formulation with respect to all possible data" (Matsubara, 2013a, p. 473). Under this formulation of underdetermination a problem arises for the scientific realist (depending on the particular formulation of scientific realism), as two distinct theories are completely evidentially equivalent. Dualities are characterised positively and negatively (with a two-part test). Matsubara argues that "for there to be a relevant kind of duality we are not allowed to consider a duality to exist between two systems distinguishable on an empirical level" (Matsubara, 2013a, p. 479). The harmonic oscillator is offered as an example of a duality that fails this relevance test (Matsubara, 2013a, pp. 478-479). Matsubara further argues that "when it is *clear* that we are discussing two alternative coordinate descriptions of one and the same system it will not be considered a duality" (Matsubara, 2013a, p. 478) (italics author's own). The example[19] offered that violates this relevance test is taken from Maxwell's equations. Matsubara argues that a duality "that is more of a question of conventionality in terminology" violates the second condition offered (Matsubara, 2013a, p. 480). 'Relevant' dualities are positively characterised as:

> "These dual descriptions, if they are understood in a straightforward or literal way, present views of the world containing different kinds of objects and different descriptions of spacetime which can even differ in topology. Nevertheless they are thought to describe the same physics. This means that they give rise to the same set of particles, symmetries, scattering amplitudes and other empirically measurable, or at least potentially empirically measurable, quantities." (Matsubara, 2013a, p. 480)

For Matsubara these dualities are "more profound" (Matsubara, 2013a, p. 480). Explicit in this characterisation of dualities in string theory is a demarcation of physics as a particular set of *in principle* experimentally measurable quantities. It is based on this demarcation that Matsubara argues that string theory has physical content (Matsubara, 2013a, p. 477).

Matsubara argues that there are two paths of interpretations of the dualities, where 'physically equivalent theories' differ in terms of fundamentality, geometry and number of dimensions: literal and non-literal. The path of literal interpretation results in underdetermination with two potential interpretations where "two descriptions have the same empirical content, or at least potential empirical content, but besides that we can *not* say that they have an important X in common, where X could be a shared structure" or where "we accept them as two genuine alternatives that have an important X in common, where X could be a shared structure" (Matsubara, 2013a, p. 484). That

[19] Matsubara sources both examples from Zweibach's introductory text book (Zwiebach, 2004).

which follows for the former is epistemic anti-realism. In the latter (presumably if ‘X’ is made precise), that which follows is structural realism (Matsubara, 2013a). The path of non-literal interpretation is to argue that the different systems are “instead descriptions of the same underlying reality which is given in terms of X” (Matsubara, 2013a). Matsubara argues that which follows from this interpretation is ontological structural realism. For more on the difference between epistemic and structural realism see (Ladyman, 1998). Matusbara’s distinction between possible interpretations is useful in that he outlines the various contingencies of interpreting the dualities and their epistemic consequences, in particular the role of semantic questions. However given the many uncertainties involved, from the unproven nature of the dualities themselves to the uncertainty surrounding string theory itself, the ‘if’ ‘then’ conclusions within the paper rest on precarious ground.

1.5.2 Fundamentality and reductionism

‘String Theory’, Leonard Susskind’s invited contribution to the *Foundation of Physics* special issue, is a short piece that addresses two issues of interpretation (Susskind, 2013). Susskind outlines an argument for interpreting the successes of string theory (examined in section 1.1 of Chapter five) and an argument for anti-reductionism as an interpretation of the duality relationships. Susskind also outlined an extended version of the arguments presented in a lecture given on January 10th 2011, as part of the Stanford Continuing Studies Program available through iTunes U, the day before the paper was received by *Foundations of Physics* (Susskind, 2011).

Susskind defines reductionism as a commitment to a “hierarchy of structure” where “big complicated things are made of smaller simpler things; and that the properties of the bigger things are explainable in terms of the laws governing the smaller things” (Susskind, 2013, p. 177). For Susskind there can be two variations of a reductionist commitment: one where the hierarchy of structure has no end point and one where an end exists and therefore there is a fundamental entity. On this definition of reductionism, Susskind argues that “string theory is telling us that in a deep way reductionism is wrong” (Susskind, 2013, pp. 177-178) and that this is due to ambiguities in string theory, where the choice of elementary objects is a matter of convenience. Or, as he describes it in the talk, “which is fundamental and which is composite doesn’t have a unique answer” (Susskind, 2011). Ultimately Susskind’s interest in an anti-reductionist position is expressed as an argument against the necessity of uniqueness.

It is not surprising for Susskind to end his reflections with some speculation as to the controversy over uniqueness and multiverse theories. For Susskind the controversy centres on the success or failure of theories to explain:

> “What does string theory say? Different people have different views. Some say that string theory has been a failure: it has not explained any of this. Others say give us more time, we

> are getting closer every day. But there is a third view that is gaining ground." (Susskind, 2013, p. 180)

This third view, that Susskind has defended in various places (some examples (Susskind, 2005), embraces complexity as virtuous. The anti-reductionist position for which he advocates for can be read as providing support for a multiverse as a consequence of a string theoretic picture. Susskind acknowledges that this picture is controversial:

> "Some people are horrified by this complexity, and even more so by the idea of a multiverse populated by bubble-universes that fill the landscape. Others find the idea exciting because if fits nicely with cosmologists' speculations about eternal inflation, and environmental selection. At the moment it is too soon to say who will be right. But what we can say is that if the multiverse concept proves correct, it will be an enormous success for string theory. If it proves wrong then it's back to the drawing board." (Susskind, 2013, p.180)

This final claim is perhaps the most extraordinary and, in keeping with the rest of the paper, lacking in precision. Susskind effectively proposes the multiverse as a falsifiable test of string theory where, if the idea were found to be incorrect, it would result in abandoning the string theory research program as it currently stands. This proposal will be discussed further in sections 2.1 and 2.2 of Chapter five.

Following on from the earlier described work, Matsubara also examines and interprets the significance of the S-duality as well as the T-duality and AdS/CFT correspondence in terms of fundamentality. Under the S-duality (where theories with different coupling constants are found to be dual), type IIB string theory is dual to itself and the heterotic SO (32) is dual to type I string theory. Of interest to Matsubara is that "what seems to be fundamental objects in one formulation gets mapped to composite objects in another" (Matsubara, 2013a, p. 482). Matsubara interprets the significance of this duality as meaning that "what is treated as fundamental building blocks depends on the description" (Matsubara, 2013a, p. 483). This is almost identical to Susskind's anti-reductionist interpretation (Susskind, 2011, 2013) discussed earlier, using the same phrasing but omitting explicit talk of anti-reductionism in favour of fundamentality. The T-duality is interpreted similarly, where instead of fundamentality being dependent on formulation geometry is now at stake: "it follows according to string theory that the description of the geometry and topology of spacetime can differ in formulations that are thought to be physically equivalent" (Matsubara, 2013a). Finally, Matsubara argues that it follows from the AdS/CFT conjecture that the number of dimensions is dependent on formulation (Matsubara, 2013a, p. 483).

In 'AdS/CFT Duality and the Emergence of Spacetime', Rickles turns his attention to the AdS/CFT duality (Rickles, 2013a). The majority of the paper is dedicated to outlining "the standard capsule definition of superstring theory" (Rickles, 2013a), the general principle of holography, the gauge/string duality, the AdS/CFT duality and the emergence of spacetime and locality in AdS/CFT (Rickles, 2013a). The high descriptive content of the paper is perhaps reflective of the infancy of the field at the time – the assumed knowledge is minimal. Unlike Matusbara (2013), Rickles offers a detailed examination of the positions offered by Oriti and Horowitz and Polchinski, and, in particular, the exchange of questions and answers between the three (Oriti, 2009, pp. 229-231). Of course the descriptive content of the paper is also set up to facilitate a particular interpretation of the AdS/CFT duality as a potential example of 'structural underdetermination' which Rickles describes as a case where "the empirical content is identical, and yet the extra-empirical content differs at a structural (not just object based) level" (Rickles, 2013a, p. 319). Rickles rejects this interpretation as providing a genuine counter example to structural realism on the basis that the AdS/CFT duality 'provides a window' to an "invariant core" and the method of identification of a core structure is a "methodology for scientific discovery" (Rickles, 2013a, p. 320). So whilst the unifying core of string theory has yet to be found Rickles argues there is some evidence for its existence.[20]

A question that needs to be asked here is: what is the status of the Ads/CFT duality as an example where "the empirical content is identical, and yet the extra-empirical content differs at a structural (not just object based) level" if an invariant core is not found? If a consensus forms that string theory, in a formulation in which the AdS/CFT duality remains not significantly unchanged, is 'correct' how will the example be interpreted? I suspect that the 'correctness' of string theory, an ambiguous concept in and of itself, cannot be determined independently of the discovery of a 'unified core' and as such is to a certain extent contingent. This question obviously cannot be resolved until consensus forms around string theory and until a better understanding of 'what string theory is' is formed (and this is for future historians to tell). What is evident is the advantage of studying a controversy in which consensus has yet to be formed and just how evident it is that a particular consensus is not inevitable.

[20] Rickles extends and expands this argument in the forthcoming 'Dual Theories: "Same but Different" or " "Different But Same"?' (Rickles, Forthcoming)

2. Philosophically motivated constraints of a (unified?) theory of quantum gravity

The final section of Rickles' 2011 chapter is dedicated to the role of 'constraints' and "the role they play in establishing scientific beliefs, and accounting for changes in science" (Rickles, 2011b). In the interesting example of quantum gravity research Rickles' focus is on finding a constraint that can function analogously to experiment in theory selection and development (Rickles, 2011b). Here Rickles examines string theory as an example of the theory of quantum gravity and role of the 'finiteness' and 'absence of anomalies' in the history of string theory. Where the former was responsible for giving string theory "credence" early in history, the latter was initially considered an obstacle, but the subsequent removal of anomalies resulted in a "significant degree of motivation" (Rickles, 2011b, p. 36). In this section, Rickles brings the separate strands of his argument together to argue that "only a close investigation of the historical details can reveal which constraints guided some particular theory choice. The constraints will often be sociological as well as mathematical and empirical" (Rickles, 2011b, p. 37). This approach is compelling as the claim is borne out by the existing literature in two ways. First, and as mentioned earlier, literature that examines string theory as a theory of quantum gravity blurs traditional disciplinary boundaries. Secondly, much of the existing literature, in particular Dawid (Dawid, 2009, 2013a, 2013b), could be further improved by blurring the boundaries even more and introducing thorough historical details. This criticism of Dawid is discussed in more detail in the relevant sections.

Within the prescriptive philosophical literature, a variety of constraints can also be identified for a theory of quantum gravity or a unified theory of quantum gravity. Often the arguments that support the constraints are drawn from historical examples of unification or failed attempts at quantum gravity. These constraints can be grouped into two categories: constraints upon the properties of a theory and constraints upon methodology.

Mauldin's 1996 piece 'On the Unification of Physics' is not a direct attempt to philosophically interrogate string theory. Instead its relevance may be attributed to its broad coverage of a postulated historical shift, that string theory as a grand unification attempt is claimed to be part of, and the resultant philosophical questions that arise from this "velvet revolution" (Mauldin, 1996, p. 125). The piece maps out the philosophical territory of unification in late twentieth century physics and suggests future directions for philosophers of physics into the twenty first century.

Mauldin aims to examine a gradually emergent transformation that "centers around the remarkable idea that the aim of a physical theory is to achieve unification" (Mauldin, 1996, p. 129). Importantly he argues that this aim is directing theoretical work without question. His concerns are descriptive but, not in a historical sense, he aims to ask and answer the question "what is it?" (Mauldin, 1996, p. 130) His concerns are also prescriptive: questioning whether there is basis to the commitment to unification. Mauldin aims to draws boundaries around this concept on unification by showing both what is not and what it ideally is – through analysis of two 'perfect' examples. Drawing on this he examines the evidence for unification.

Beginning with what unification is not, Mauldin argues that consistency is necessary but not sufficient as consistency is a weak criterion (Mauldin, 1996, pp. 130-131). Similarly, individually or collectively, an "employment of a single fundamental dynamics" and/or "nomic correlation" (Mauldin, 1996, p. 132) are also deemed insufficient but provide a lower limit. The upper limit is found in the perfect examples of the special theory of relativity and the general theory of gravity (SR and GR). It is claimed that in SR "the electric and magnetic fields are 'unified' by being, in a way, eliminated entirely from the fundamental ontology, and by being replaced by a single, frame independent entity" (Mauldin, 1996, p. 133). GR is claimed to be unification as "*reduction* of gravity to inertia" (Mauldin, 1996, p. 133) (italics author's own) whereby two entities (spacetime and the gravitational field) are replaced by a new entity (curved spacetime). This upper bound, or deep unification, is a claim that unification requires a transformation where two or more theoretical entities are shown to have the same ontological status.

For the special issue of *Foundation of Physics*, Carlo Rovelli was asked to contribute a critique of string theory (Rovelli, 2013). In the paper, titled 'A Critique of String Theory', Rovelli selects to omit appraisals of the applications of string theory to QCD,[21] strongly interacting fluids and mathematics (Rovelli, 2013, p. 9). Instead Rovelli examines string theory as a theory of quantum gravity through comparison to his own research program loop quantum gravity, and separately as a unification attempt. Rovelli stresses that string theory is a proposed solution to two problems: quantum gravity and unification and that solving the unification problem does not imply a successful solution to the problem of quantum gravity (Rovelli, 2013, pp. 9-10, 15-16). Rovelli draws upon two reviews of string theory that focus on string theory as a theory of quantum gravity: (Blau & Theisen, 2009) and (Mukhi, 2011). This choice highlights Rovelli's contribution as one that does not seek to review of string theory per se, as each of the problems and successes of string theory discussed are well-known. Instead Rovelli seeks to shape how these problems and successes should be interpreted, particularly in comparison to loop quantum gravity. Indeed the paper within the special issue that delves deepest into criticism and of string theory is authored by a string theorist (see (Giddings, 2013)).

In turn Rovelli examines ultraviolet finiteness, quantum geometry, the overall picture, describing this world, unification, applications, predictions and concludes "it does not work, therefore let's develop it further". What is evident in each section of analysis is Rovelli's methodical norms. In 'quantum geometry', Rovelli examines the well-trodden issue between string theorists and loop quantum gravity theorists, background independence, and claims that the issue is not whether a background

[21] I have called this the tool view of string theory; see the introduction for an outline of the two views of string theory: tool and TOE.

independent formulation of string theory exists but that background independence is “not yet properly understood” by string theorists (Rovelli, 2013, p. 12). The problem, for Rovelli, is: “in all these cases, instead of addressing the real problem, which is to learn how to do physics where background spacetime plays no role, the strategy is to try to circumvent the problem” (Rovelli, 2013, p. 12). Unsurprisingly Rovelli argues that the strategy employed by loop quantum gravity is superior as the problem is addressed “upfront” resulting in a “conceptually clear, fully general relativistic, and well defined” picture of quantum gravity (Rovelli, 2013, p. 13). Here we see both an expression of Rovelli’s methodical norms and how those norms shape his appraisal of string theory and loop quantum gravity.

Rovelli also draws upon the history of science in his appraisal of methodologies: “extrapolation has always been the most spectacularly effective tool in science. Maxwell equations, found in a lab, work from the atomic to the galactic scale. Up to contrary empirical indications, always possible, a good bet is that what we have learned may continue to hold” (Rovelli, 2013, p. 14). Rovelli tells a cautious tale of the history of attempting to solve two problems with a single solution so as to critique the idea that the problem of quantum gravity can only be solved with the unification problem. For Rovelli “the philosophy underlying loop gravity is that we are not near the end of physics, we better not dream of a final theory of everything, and we better solve one problem at the time, which is hard enough” (Rovelli, 2013, p. 16). Again we see that Rovelli’s appraisal of string theory is not located in a description of the current state of the unification attempt, but instead it is with the commitments that underpin the attempt.

Finally, Rovelli offers a series of criteria by which the success or failure of string theory (and loop quantum gravity) may be judged: “completeness, internal consistency, full agreement with known low-energy physics, simplicity, and, ultimately, experience, will tell” (Rovelli, 2013, p. 19). It is unlikely that many string theorists would disagree significantly with Rovelli’s list (this claim is explored further in chapter five). What we see in Rovelli’s comparative appraisal of string theory and loop quantum gravity as unified theories of quantum gravity is a series of methodological norms through which the significance of succusses and problems for a theory of quantum gravity are interpreted.

In ‘A Perspective on the Landscape Problem’, Smolin also identifies a series of constraints for a theory of quantum gravity built on an extensive definition of the landscape problem, as he understands it, situated within a particular historical context (Smolin, 2013, pp. 21-31). On the basis of that definition of the landscape problem, Smolin identifies constraints for both the form of a proposed solution and the methodology for determining a solution. Smolin also evaluates three attempts at solution, eternal inflation, cosmological natural selection and cyclic cosmologies, with respect to the constraints identified (Smolin, 2013, pp. 31-41). Using a similar argument, Smolin criticises string theory as an inadequate unified theory due to an insufficient response to the three constraints he imposes upon a unified theory: background independence, a non-perturbative formulation and providing a solution to the landscape problem (as he has defined it) (Smolin, 2013, pp. 41-42). The final constraint is argued as a constraint upon a unified theory on the basis of Smolin's argument that the landscape problem, as it arises in string theory, should be understood as a generalised problem that applies to any unified theory (Smolin, 2013, p. 42). Smolin argues that it was "inevitable that as physics as physics progressed we would have encountered the problem of explaining how the universe chose its laws" and calls this the "generalized landscape problem" (Smolin, 2013, p. 42). The paper is useful as a clear outline of Smolin's position as a protagonist in the string wars. Smolin's inclusion of particular historical details effectively foregrounds key concerns for the author.

Susskind too outlines an argument that consistency should constrain theory. He provides a list of explanatory successes achieved by string theory: quark confinement, liner regge trajectories and hadronisation. In each of the examples Susskind utilises an understanding of explanation that is reminiscent of the understanding employed by Hempel,[22] that is, the behaviour that is expected when a string world picture is applied. Confinement is to be expected in stringy hadrons as in this picture the elastic strings cannot be broken unless a new quark anti-quark pair is generated (Susskind, 2013, p. 175). The list of explanatory successes is provided as justification for the claim "that hadronic matter really is string like" (Susskind, 2013, p. 174). Here Susskind appears to be offering a naïve justification for a string-based ontology but he goes on to temper the claim, assuming what Susskind calls a 'narrow definition' of string theory where string theory is defined as follows:

> "Just to be precise about what constitutes string theory, let me give a narrow definition—no doubt much too narrow for many string theorists. But it has the virtue that we know that it mathematically exists. By string theory I will mean the theory of supersymmetric string backgrounds including 11-dimensional M-theory and compactifications that preserve some degree of supersymmetry. These back[g]rounds are generally either flat (zero cosmological constant) or anti de Sitter space with negative cosmological constant." (Susskind, 2013, p. 176)

Under this definition of string theory, Susskind argues that "there is no doubt: string theory is *not* the theory of nature" as nature is not supersymmetric and the cosmological constant is currently

[22] In his now ubiquitous account of explanation as a logical argument, Hempel argues the key to understanding an explanation is that, "given the particular circumstances and the laws in question, the occurrence of the phenomenon *was to be expected*; and it is in this sense that the explanation enables us to *understand why* the phenomenon occurred" (Hempel, 1965, p. 337) (italics author's own).

understood to have a positive value"[23] (Susskind, 2013, p. 176) (italics author's own). For Susskind that which is important is mathematical consistency and this is sufficient to believe that "string theory has had a profound … and lasting influence on how gravity and quantum mechanics fit together" (Susskind, 2013, p. 176).

Susskind is frustratingly vague as to how to interpret consistency precisely. He argues that "one should not underestimate the importance of having a mathematically consistent structure that contains both quantum mechanics and gravity" (Susskind, 2013, p. 176) but neglects to argue for what role consistency should play in our estimation. Similarly Susskind claims the application of non-realistic string theory models "proved" that, contrary to Hawking's contention, information is not destroyed in black holes and that "this was no small thing" (Susskind, 2013, p. 176), but neglects for provide an argument as to how his reader should positively understand the development. What we are able to take from Susskind is that he considers consistency sufficient to inform "what kinds of things are possible" and "what kinds of structures to expect" (Susskind, 2013, p. 176).

It is clear that there are several positions within the literature as to how to interpret consistency as a constraint. Maudlin argues for constraints on a unified theory with arguments for lower and upper limits. Consistency and nomic correlation are argued to be weaker criteria that are necessary but not sufficient (Mauldin, 1996, p. 132). The upper limit, sufficient to determine unification and necessary for "perfect" unification, requires a transformation where two or more theoretical entities are shown to have the same ontological status (Mauldin, 1996, p. 133). Hedrich also argues that a constraint on a theory of quantum gravity is that it should be motivated by external problems, where external problems are defined as problems not identified in the construction of the theory. One such problem, identified by Hedrich, is finding a consistent theory of quantum gravity, and thereby Hedrich also identifies consistency as a constraint (Hedrich, 2007, pp. 265-267).

Rovelli draws upon two of the same examples of unification as Maudlin (electromagnetism and general relativity) to argue that an attempt at constructing a theory of quantum gravity should be constrained by formulating a background independent approach. This constraint is a methodological norm by which the process of theory construction should follow a certain path, that is to say that, on Rovelli's view, background independence must be established initially rather than 'hop[ing]' it will be recovered at a later date (Rovelli, 2001, p. 109). Curiel identifies 'modesty' as a kind of constraint that he argues should be imposed on the string theory community. Curiel's focus is not so much a negative

[23] The discovery of the positive cosmological constant had a significant impact on the string theory research program as discussed in section 1.8 of chapter two. Briefly prior to 2003, string theory studies focused on models with vanishing or negative vacuum energy. The discovery of the positive cosmological constant forced a new strategy, achieved in the now famous KKLT paper (Kachru, Kallosh, Linde, & Trivedi, 2003). One consequence of this work was that there was now a very large number (10^{500} is often quoted, however the precise number is not known) of vacuum states, each allowing for a different version of the theory. Here Susskind is choosing to focus on string theories that do not utilise the KKLT mechanism.

characterisation of the string theory but rather Curiel is calling for more honest appraisals of quantum gravity research that acknowledge a lack of connection with experiment (Curiel, 2001). In addition to the constraining properties of a theory of quantum gravity, Hedrich also identifies methodical constraints argued to bear upon the scientific status of a theory of quantum gravity. Hedrich argues that *ad hoc* manoeuvers to maintain internal consistency and self-immunisation mark the violation of methodical norms (Hedrich, 2007, pp. 265-266).

3. Final points

3.1 'Philosophical' contributions to the string wars

Much of the 'philosophical literature' can be interpreted as contributions to the string wars. This claim is evidenced by the arguments offered within the literature both against and in support of the rationality of pursuing string theory and in relation to evaluations of methodology. Furthermore this ambiguity in demarcating the literature is further evidence of the porous nature of the points of conflict within the string wars. This is also evidenced by the way in which philosophical contributions have become part of the string war discourse. This goes beyond the well-known phenomenon of scientists assuming philosophical terminology such 'falsifiability', whilst remained ignorant of many of the details of Popper's arguments, in appraisals of research programmes.

Recently philosophical publications have stimulated discussion, particularly across various blogs where the content of the papers is dissected and then discussed in the commentary section. The special issue of *Foundations of Physics* and *A Brief History of String Theory* (Rickles, 2014) were both discussed on Woit's blog (Woit, 2012a, 2014a). Dawid's book has generated the most discussion (Dawid, 2014; G. Ellis & Silk, 2014; David Gross, 2014; Hossenfelder, 2014a, 2014b, 2015; Motl, 2013; Orzel, 2014; Woit, 2014e). Also, Dawid is unique in his participation in the discussions occurring on blog posts discussing his work (see Dawid's comments and exchange with Woit at (Hossenfelder, 2014b)). Intentionally or not, much of the recent philosophical literature dedicated to string theory has become part of the string wars.

3.2 The descriptive accounts of the string wars are overly simplistic

There has only been one serious attempt made to write a history of the string theory research program and it was published very recently, so its impact is yet to be felt in the literature (Rickles, 2014). For the most part, where attempts are made to provide a descriptive accounts of the history of string theory, a 'standard story', which assumes the accuracy of the popular and personal accounts of the protagonists, is present in much of the literature (such as in (Dawid, 2009, 2013a; Johansson & Matsubara, 2011)). Entirely missing from the history and philosophy of science literature are the debates within the string theory community as to theory evaluation and what would constitute a successful theory. These debates centre on the multiverse and the admissibility of anthropic reasoning and sometimes see critics and supporters unite on particular points.

Most of the literature assumes a simplistic 'us against them' approach in dealing with the appraisal of string theory. This picture of the debates characterises the critics and supporters of string theory as a united whole. In the case of Dawid (2009), the positions of a small number of critics and supporters are considered to be entirely representative and any deviation, such as Smolin's previous work in string theory, is ignored. Dawid presents a picture of the debates over string theory where the critics are united under one conservative meta-paradigm and the supporters of string theory are united by an emergent meta-paradigm (Dawid, 2007, 2009, 2013a, 2013b). In the case of Gubser the critics of string theory are considered united in their critical appraisal of string theory as 'untested' or 'excessively mathematical' (Gubser, 2013, p. 141). Kragh's examination of the debates over string theory examines the positions of numerous protagonists in the debates over string theory and identifies two points of conflict. However Kragh also oversimplifies the debates by arguing that the focus of the controversy is the non-empirical nature of string theory.

One exception is in section 11.3 of 'String theory and Quantum Gravity', where Kragh outlines the controversy over string theory from 1985. Kragh identifies two points of conflict within the debates: "the theory's glaring lack of connection to experiments" and "the way enthusiastic string theorists spoke of and promoted it as 'the only game in town'" (Kragh, 2011c, pp. 305-306). However Kragh argues that the focus of the debates is the non-empirical nature of string theory; he argues that "most of the critical comments have focused on the theory's lack of testability and its failure to produce results concerning the world as it is experienced" (Kragh, 2011c). The landscape and the introduction of anthropic reasoning is identified as a 'turning point' in the controversy over string theory. Kragh describes the shift in the controversy as a potential rejection of uniqueness as an "epistemic desideratum" from the original aim of string theory as "a theory which consistently and uniquely described all of nature and was controlled only by requirements of self-consistency" (Kragh, 2011c, p. 314).

In a section on loop quantum gravity, Kragh also briefly looks at the controversy between string theory and loop quantum gravity as rival theories of quantum gravity (Kragh, 2011c, pp. 316-320). Drawing upon a critique of Rovelli, Kragh locates the controversy at a non-empirical level on the basis that "when it comes to predictions and testable consequences it is probably fair to say that [there is] not a great deal of difference between string theory and loop quantum gravity" (Kragh, 2011c, p. 319). As with the previously discussed sections, Kragh's contributions are of significant potential utility for philosophers of science given the high level of detail contained within the descriptions of the debates. Kragh also situates various contributions written by philosophers within the debates such as the previously mentioned 'Testability and Empiricism' (Shapere, 2000).

It will be the aim of this thesis to build a more descriptively accurate understanding of the debates over string theory, in which both the controversy over a lack of empiricism and also the many other points of conflict are explored. As Rickles argues, in the case of theories of quantum gravity history, philosophy and sociology converge and "only a close investigation of the historical details can reveal which constraints guided some particular theory choice. The constraints will often be sociological as well as mathematical and empirical" (Rickles, 2011b, p. 37).

Chapter Two: Contested boundaries

Preface

An earlier version of this paper was published in 2015 in *Perspectives on Science* in a modified form with co-author Kristian Camilleri (Ritson & Camilleri, 2015). This version has been edited and updated to include recent events and publications.

Introduction

This chapter focuses on one central issue that has acquired prominence in the public controversy over string theory. This issue, which can be traced back to the 1980s, concerns string theory's lack of experimental support, which has led some critics to cast doubt on its very status as science. Certain critics and defenders of string theory have engaged in a debate over whether string theory legitimately counts as science. Thomas Gieryn has aptly described this kind of discursive activity as "boundary work". In this chapter Gieryn's notion of boundary work (1983, 1999) is used and expanded upon by drawing attention to the dialectical nature of demarcation discourse in the debates over string theory. While there is widespread agreement that string theory currently makes no testable predictions, a variety of responses are found as to what conclusions should be drawn from this state of affairs. A range of nuanced positions and rhetorical strategies have emerged in response to such criticisms over the past decade, which attempt to attack and defend string theory's legitimacy as a science.

In drawing attention to dimensions of the controversy, this chapter aims to bring to light the discursive strategies and rhetorical arguments employed by protagonists on both sides of the debate in their attempt to construct an ideological definition of science. As discussed in section 1.1 of chapter one, string theory has also attracted the attention of historians and philosophers of science attempting critical analysis of string theory. As argued in section 3.1 of chapter one, some of the prescriptive philosophical literature can be interpreted as contributions to the debates over the scientific status of string theory. This chapter does not offer a philosophical analysis of the demarcation problem in the context of string theory, nor does it attempt to provide an assessment of theoretical problems that have plagued string theory in the quest to find a unified theory. Instead this chapter focuses on how rhetorical discourse has been deployed in the controversy over string theory. The focus on this 'rhetorical' aspect does not imply that there are no substantive philosophical or scientific issues at stake. As Peter Galison has rightly pointed out: "This is a debate about the nature of physical knowledge" (Galison, 1995b, p. 403).

This chapter cannot to do full justice to the complex, dynamic and shifting nature of the debates over string theory. New theoretical developments, alternative approaches to quantum gravity, recent experiments at the Large Hadron Collider, and the funding for applied physics have transformed the intellectual debate in the last few years. The discussions about the predictive consequences of supersymmetry, for example, have a complex history of their own, and continue to unfold to the present day. Indeed, whilst there were growing signs that the controversy has subsided since 2008, recently the controversy has re-emerged prompted by calls to abandon falsifiability (Carroll, 2014a, 2014b) and an editorial in *Nature* (G. Ellis & Silk, 2014). Despite the complex history it is clear that the methodological aspects of the recent controversy surrounding string theory represent an intriguing and rather peculiar example of boundary work. In this controversy, unlike most studied cases of boundary work, it is the prevailing orthodoxy in a well-established field that has been forced to defend its legitimacy as a science. This makes this boundary debate particularly interesting from both a historical and sociological perspective.

1. The discourse of demarcation

1.1 The concept of boundary work

Certain critics of string theory have argued that in the absence of empirical foundations or testable experimental predictions, string theory represents a serious crisis in physics and even fails to qualify as science. In response to such criticisms, some defenders of string theory have deployed a series of argumentative strategies to reaffirm its status as a science. To this extent, physicists have engaged in what the sociologist of science, Thomas Gieryn, has called 'boundary work' (Gieryn, 1983, 1999). Gieryn's notion of boundary work has proved extremely useful as an analytic tool in sociological and rhetorical studies of certain scientific controversies. Simply put, boundary work refers to the attempt by scientists to demarcate science from non-science. While the demarcation problem is normally a subject reserved for philosophers, Gieryn pointed out that in certain situations, scientists embroiled in a controversy will attempt to construct a 'boundary between science and nonscience' for "ideological" reasons (Gieryn, 1999, p. 26).

Recognising that the label 'science' carries with it intellectual legitimacy, professional opportunities and material resources, scientists endeavour to construct the boundary so as to ensure that their own work qualifies as scientific, while at the same time discrediting other theories or activities (branding them non-scientific or pseudoscientific). As Prelli puts it: "scientists engage in boundary work, not for the lofty epistemological reasons philosophers often cite … but as a rhetorical means of solving practical problems that can block achievement of professional goals" (Prelli, 1989, p. 91). Boundary work, as Prelli explains, trades on the inherent ambiguities of demarcation:

> "If it were possible to draw a sharp line of demarcation between science and nonscience, there would be little ambiguity involved in classifying discursive aims and claims as "Scientific" or other; hence, there would also not be any need for rhetoric to clarify the scientific standing of those aims and claims. However, wherever we seek to differentiate "science" from "nonscience", there will always be working ambiguities. In these rhetorical situations, scientists will likely choose rhetorical strategies that help construct "boundaries" that are favourable to their own professional goals and interests and unfavourable to their competitors." (Prelli, 1989, p. 91)

As is argued below, the debates over string theory offer an interesting case of boundary work. In this controversy, no single view of what constitutes science is found, but instead "its boundaries are drawn and redrawn in flexible, historically changing and sometimes ambiguous ways" (Gieryn, 1983, p. 781). In his book *Defining Science*, Charles Taylor develops this dimension of boundary work further, by drawing attention to the way in which "the intersubjective negotiation of demarcation standards" reveals the dialectical nature of demarcation discourse. To this extent "rhetorical demarcation practices are both rhetorically and historically adaptive" (Taylor, 1996, p. 92). This nicely captures what has unfolded in in debates concerning the scientific status of string theory, in which participants have responded in a variety of ways. Here the "contours of science are shaped by the local contingencies of the moment: the adversaries then and there, the stakes, the geographically challenged audiences" (Gieryn, 1983, p. 5).

1.2 Is string theory really science?

The rise to prominence of string theory in the 1980s contrasted sharply with the era of physics preceding it. Whereas the success of the Standard Model of particle physics had been largely based on experiment, the quest to unify physics with a theory of quantum gravity, which gathered momentum in the mid-1980s, embraced a different ideal, in which a lack of contact with experiment was not considered to be problematic (Kragh, 2011c, pp. 300-301), instead relying on theoretical consistency checks. String theorists argued that many of the experimental successes of the past century including the Standard Model would be encompassed by a new, unified theory, which would reveal itself as a mathematically (internally) and theoretically (externally) consistent framework; a logically isolated theory. Yet, a number of physicists were less than enthusiastic about these new directions – experimentalists tended to either ignore them, or treated this emerging style of theoretical physics with suspicion, if not downright hostility. Perhaps not surprisingly, high energy experimental physicists expressed serious concerns about string theorists' preference for theoretical abstraction over the laboratory (Richter, 2006, p. 8). After no more than four decades, Smolin points out, there is still "no realistic possibility for a definitive confirmation or falsification of a unique prediction from it by a currently doable experiment" (Smolin, 2006b, p. 179).

Nobel Laureate, Sheldon Glashow, was perhaps the leading figure among an earlier generation of physicists to voice concerns about the legitimacy of string theory in the 1980s. Together with Paul Ginsparg, Glashow published a critical paper claiming that string theory "unless it allows an approximation scheme for yielding useful and testable physical information, might be the sort of thing that Wolfgang Pauli would have said is "not even wrong"" (Ginsparg & Glashow, 1986, p. 39).[24] These sentiments were echoed by the former director of the Stanford Linear Accelerator Centre, Burton Richter, who declared: "some of what passes for the most advanced theory in particle physics today is not really science" (Richter, 2006, pp. 8-9). Much of this criticism stems from a broadly Popperian point of view. As Glashow put it: "I have been brought up to believe that systems of belief which cannot be falsified are not in the realm of science" (Glashow quoted in (Chalmers, 2007, p. 35)). In 2001 Peter Woit also voiced similar concerns:

> "String theory not only makes no predictions about physical phenomena at experimentally accessible energies, it makes no predictions whatsoever. This situation leads one to question

[24] This Pauli quotation has been quoted extensively and was used by Woit as the title of a blog he began on March 17, 2004 which was dedicated to discussions about and criticisms of string theory (Woit, 2004 - Present). The quotation also served as the title of his book published in 2006 in the UK and US (Woit, 2006d, 2006e).

> whether string theory really is a scientific theory at all. At the moment [string theory] is a theory which cannot be falsified by any conceivable experimental result." (Woit, 2001, p. 2)

Here Woit explicitly calls into question whether string theory can be properly regarded as a scientific theory. Yet opinion is divided, even among critics, as to what to make of the lack of testable predictions. For Dan Friedan, the repeated failure of string theory to "give any definite explanations of existing knowledge of the real world" and to "make any definite predictions" means that it "has no credibility as a candidate theory of physics" (Friedan, 2003, p. 10). Gerard 't Hooft, on the other hand, notes that while string theory "has not led to genuine explanations of well-known features of the Standard Model," nor has it made any "definitely testable predictions". However for 't Hooft this is not a source of concern, he argues that "such explanations and predictions are still way out of reach for respectable theories of physics" ('t Hooft, 2013, p. 47).

On the 27th of April 2006 a group of string theorists uploaded a not as yet peer reviewed paper titled 'Falsifying String Theory Through WW Scattering' in what seemed to be an explicit attempt to counter the criticism of string theory as unscientific (Distler, Grinstein, Porto, & Rothstein, 2006b). This strategy may be identified by use of the term 'falsifiability' in the title which within the context of the oft cited demarcation criterion in the disputes over the scientific status of string theory. Indeed the title of paper was almost immediately contested by Woit who claimed that whilst the paper may contain a falsifiable test it could not be considered a legitimate falsifiable test of string theory as the paper did not contain any string theory (Woit, 2006b). After peer review the paper was published in *Physical Review Letters* (Distler, Grinstein, Porto, & Rothstein, 2007) and a preprint uploaded to the arXiv (Distler, Grinstein, Porto, & Rothstein, 2006a) with a new title: 'Falsifying Models of New Physics Through WW Scattering'.

The difficulties in drawing any clear demarcation between science and non-science emerge clearly when taking into account the fact that string theory is a collection of techniques and mathematical insights into an attempt to construct a unified theory of quantum gravity and elementary particle physics. Here the demarcation discourse shifts from an assessment of whether string theory qualifies as a scientific theory, to an assessment of whether it legitimately qualifies as a scientific research program. This approach formed the basis of the critical appraisal of string theory conducted by Nancy Cartwright and Roman Frigg in an attempt to determine, in the Lakatosian sense, if it is progressing or degenerating qua research program (Cartwright & Frigg, 2007, p. 20). As Woit recognises: "By the falsification criterion, superstring theory would seem not to be a science, but the situation is more complex than that. Much theoretical activity by scientists is indeed speculative" (Woit, 2006d, p. 213). What counts as scientific can be broadened to include forms of speculative theorizing "that would definitely make superstring theory a science" (Woit, 2006d, p. 213). Here Woit offers the following remarks:

> "So the question of whether a given speculative activity is science seems not to be one admitting an absolute answer, but instead is dependent on the overall belief system of the scientific community and its evolution as scientists make new theoretical and experimental discoveries. … [I]f a large part of the scientific community thinks a speculative idea is not unreasonable, then those pursuing this speculation must be said to be doing science. The speculation known as superstring theory continues to qualify as science by this criterion." (Woit, 2006d, pp. 214-215)[25]

As Woit points out, in the case of string theory, the demarcation of science from non-science becomes a matter of scientific judgement. Because string theory is not an established theory, but a work in progress, its legitimacy cannot be judged simply on the basis of whether the theory in its current form makes predictions or has successfully survived attempts at falsification. Rather, the question of whether string theory qualifies as science, or is worth pursuing, is one that ultimately must be decided by the scientific community. Such judgements may of course be contested, and in ambiguous cases, boundary work assumes critical importance.

The sticking point for many is not whether string theory as it currently stands is falsifiable, but whether it is showing signs of heading in the right direction.[26] As Johansson and Matsubara recently pointed out, even within a broadly Popperian viewpoint "speculative assumptions, even metaphysical ones, are admissible in science, if they help develop testable hypotheses" (Johansson & Matsubara, 2011, p. 204). Yet, critics have been sceptical of claims that string theory will eventually lead to testable predictions. Such concerns were raised as early as 1986 by Ginsparg and Glashow, who expressed the fear that string theory "may evolve into an activity … to be conducted at schools of divinity by future equivalents of medieval theologians." The over-reliance on speculative theorizing, they contended, "may end, with faith replacing science" (Ginsparg & Glashow, 1986, p. 7). While Glashow has softened his tone more recently, he has continued to harbour serious reservations about current trends in theoretical physics. He acknowledges that string theory has provided useful results in mathematics and quantum field theory, however his commitment to testability is unwavering – it remains to be seen whether string theory "may someday evolve into a testable theory (aka science)" (Chalmers, 2007, p. 37).

[25] The demarcation criterion employed by Woit bares resemblance to that outlined by Cushing in 'The Justification and Selection of Scientific Theories' and *Theory Construction and Selection in Modern Physics* (Cushing, 1989, 1990). Cushing argues that "A good description of scientific practice, must stand or fall largely on having a proper spirit or emphasis in its representation of science … We should recognize that science is what scientists have done, not what a philosopher tells us the scientists meant to do, were really doing, or should have done. Successful theories are made to work; they don't just work on their own or because nature demands it." (Cushing, 1989, pp. 17-18).

[26] This point is explored at length in 'The Role of Heuristic Appraisal in Conflicting Assessments of String Theory' (Camilleri & Ritson, 2015).

Here it is worth reflecting on the rhetorical use of language. Critics have often resorted to insulting comparisons with religion, theology, intelligent design, and speculative metaphysics in an attempt to label string theory as unscientific.[27] Glashow's repeated comparisons with medieval theology during the 1980s serve as a case in point. By 1986 string theory had, in his view, become a "new version of medieval theology where angels are replaced by Calabi-Yau manifolds" (Glashow, 1985, p. 143). Reiterating this point with Ginsparg, he argued: "Superstring arguments eerily recall 'arguments from design' for the existence of a Supreme Being" (Ginsparg & Glashow, 1986, p. 7). In 1988 Glashow again criticised string theory, characterising it as a form of inquiry "more appropriate to departments of mathematics or even to schools of divinity than to physics departments" (Glashow quoted in (Galison 1995, p. 399)).

Burton Richter engaged in a similar strategy in a *Physics Today* article entitled, 'Theory in Particle Physics: Theological Speculation versus Practical Knowledge' (2006), and more recently cosmologist Lawrence Krauss infuriated many string theorists by drawing comparisons between string theory and intelligent design in his New York Times op-ed entitled 'Science and Religion Share Fascination in Things Unseen' (Krauss, 2005).[28] As Gieryn points out, this kind of strategy is typical of boundary work: "just as readers come to know Holmes better through contrasts to his foil Watson, so does the public better learn about 'science' through contrasts to 'nonscience'" (Gieryn, 1983, p. 791). By inviting the comparison between string theory and medieval theology or intelligent design, Glashow, Richter and Krauss attempt to establish a damning association.

1.3 String theory is a testable in principle. Just not yet in practice

Both critics and supporters of string theorists engage in rhetorical strategies that exploit the inherent ambiguity in the criterion of falsifiability. Many defenders of string theory have argued that, contrary to what critics allege, string theory is falsifiable in principle. Brian Greene concedes that string theorists "have not as yet made predictions with the precision necessary to confront experimental data" (Greene, 1999a, p. 211), but he remains hopeful that with further technological developments and a deeper understanding of its underlying mathematical structure, string theory will become capable of making falsifiable predictions (Greene, 2006). It is simply the case that current experimental techniques do not yet allow us to test certain aspects of the theory. All we can say at this point is that string theory is not testable yet. By rhetorically drawing the distinction between falsifiability in practice and falsifiability in principle, string theorists can affirm their commitment to

[27] Michael Duff has responded to such criticisms: "Support for superstrings and M-theory is based on their ability to absorb quantum mechanics and general relativity, to unify them in a mathematically rigorous fashion, and to suggest ways of accommodating and extending the Standard Models of particle physics and cosmology. No religion does that" (Duff, 2011b, p. viii).

[28] The context of these disputes is the high profile dispute in the United States over the scientific status of intelligent Design that was argued all the way to United States District Court for the Middle District of Pennsylvania in 2004. The judge (Jones) in the case found that intelligent Design violated a ground rule of science: "methodological naturalism" (*Kitzmiller, v. Dover Area School District*).

falsifiability as a criterion for demarcating science from non-science, while maintaining the view that string theory qualifies as science.

String theory is not the first theory to be in this position, as advocates like to point out. In this vein, Mike Duff contends that "gravitational waves (1916), the cosmological constant (1917) … [and] the Higgs boson (1964)" serve as instructive examples of theoretical predictions that were untestable when they were first announced (Duff, 2013, p. 191). Leonard Susskind and Brian Greene also defend string theory in their popular accounts along similar lines. Greene argues: "The history of science is filled with ideas that when first presented seemed completely untestable … ideas that we now accept fully but that, at their inception, seemed more like musings of science fiction than aspects of science fact" (Greene, 1999a, p. 226). Here Greene suggests that confining ourselves to hypotheses that could be tested at the time they were proposed would be detrimental to the progress of science.

Figure 2.1: 'Universe on a string'. Image attributed to Christoph Niemann from 'Universe on a string' (Greene, 2006)

In order to defend string theory, Veneziano, drew a distinction between predictions and testable predictions to resolutely maintain that "string theory is falsifiable" (Veneziano, 2010, p. 18). As Veneziano points out, contrary to what is sometimes maintained by some critics, "string theory makes definite predictions, like for instance the existence of very heavy (by particle physics standards) 'string excitations', or modifications of gravity at very short distances". The question is "whether any conceivable experiment, now or in the foreseeable future, will ever be able to test those predictions" (Veneziano, 2010, p. 18). David Gross expands on this point by pointing out that critics tend to impose unfairly high standards of predictive power: "String theory is full of qualitative predictions, such as the production of black holes in the LHC [Large Hadron Collider] or cosmic strings in the sky, and this level of prediction is perfectly acceptable in almost every other field of science" (Gross quoted in (Chalmers, 2007, p. 36). Only in experimental particle physics is it the case that "a theory can be thrown out if the 10th decimal place of a prediction doesn't agree with experiment".

The real issue, as Veneziano and many other string theorists see it, is that "the theory is not developed enough" to make precise predictions that "can be studied by presently available techniques" (Veneziano, 2010, p. 18). Progress in string theory will therefore depend "not on improvement in experimental techniques, but rather of the theory itself " (Veneziano, 2010, p. 21). This is a view shared by many string theorists. As Mike Duff explained during an oral debate with Smolin and Cartwright, "it frequently takes a long time for an original theoretical idea to mature to a stage where it can be cast into a smoking gun prediction, that they can test experimentally" (Smolin et al., 2007, p.

11). Here the testability of string theory turns not on whether we are capable of finding new experimental techniques to test predictions of the current theory, but whether the mathematical structure of string theory can be refined and developed to make sufficiently precise testable claims.

1.4 Self-immunisation strategies and *ad hoc* manoeuvres

The introduction of supersymmetry into string theory in the 1970s constitutes one of the more important developments and forms an important part of discussions of the testability of string theory.[29] The introduction of supersymmetry into string theory enabled physicists to develop string theories that included both bosons and fermions, and was immediately seen to have potentially experimentally testable consequences. In supersymmetric theories, each known elementary particle has a partner (known as a superpartner). If the symmetry were exact, the partners would have the same mass, would be available at experimentally available energies. Given that this is not the case, some form of spontaneous symmetry breaking must take place for the theory to hold (Polchinski, 1998, pp. 512-513). In order for the predicted particles of supersymmetry to exist, they must be heavier than all particles previously observed.

During the 1990s and especially in the lead up to the construction of the LHC, supersymmetry was frequently presented as a testable consequence of string theory. String theorists were optimistic that supersymmetric particles might be discovered by the next generation of particle accelerators within the next decade. This meant that the predictions of string theory could become testable in the foreseeable future. John Schwarz, for instance, expressed the view that “supersymmetry is the major prediction of string theory that could appear at accessible energies”. Here he pointed out that “the characteristic energy scale associated to supersymmetry breaking should be related to the electroweak scale”, and that one could therefore expect “that some of these superpartners should be observable at the CERN Large Hadron Collider” (Schwarz, 2001, p. 147). Witten referred to supersymmetry as a “genuine prediction” of string theory (Witten, 1998, p. 1124). Articles such as ‘String Theory Is Testable, Even Supertestable’ reinforced the impression that within a matter of years, one could have an experimental test of string theory (Kane, 1997, p. 50).

Yet, it is important to note that supersymmetry can, at best, provide limited support for the testability of string theory. As string theorist Gubser explained in 2010, “supersymmetry and string theory are logically distinct. But they are deeply intertwined. Discovering supersymmetry would mean that string theory is on the right track”. While it is possible there could be “supersymmetry without string theory”, such a scenario “would be too great a coincidence to be believed” (Gubser, 2010, p. 120). Brian Greene explains that “if the superparticle partners are found, string theory will not be proved correct”, but it “will give circumstantial evidence that this approach to unification is on the right

[29] Pierre Ramond first introduced the idea of supersymmetry into hadron theory in 1971, enabling the Dual Resonance Model of strong interactions to incorporate fermions (half integer spin particles like electrons and protons).

track" (Greene, 2011). The testability of supersymmetry, configured as a prediction of string theory, is claimed to provide qualified support for a connection between string theory and experiment.

Yet in spite of the hopes of a generation of string theorists, superpartners were not discovered in the first run of the LHC (2010-2013).[30] String theorists point out that there are many factors, quite separate from those posed by string theory, which make discovering supersymmetric particles at experimentally accessible energies especially difficult, such as the problem of separating the electroweak scale from the GUT/Planck scale. As Brian Greene explains, "even if superpartner particles are not found by the Large Hadron Collider, this fact alone will not rule out string theory, since it might be that the superpartners are so heavy that they are beyond the reach of this machine as well" (Greene, 1999a, p. 222). Schwarz had also foreshadowed this possibility in 1998: "even though I do expect supersymmetry to be found, I would not abandon this theory if supersymmetry turns out to be absent". Here Schwarz remained convinced that string theory "must certainly be correct" as it is "the unique mathematical structure that consistently combines quantum mechanics and relativity" (Schwarz, 1998, p. 2). Philosopher Reiner Hedrich sees this kind of commitment as symptomatic of a strategy of self-immunisation against empirical control: "should there be no indications for these particles, one could simply insist that, obviously, they have masses beyond the range of the experimental device" (Hedrich, 2007, p. 269).[31]

Some critics of string theory see this as a kind of manoeuvring as *ad hoc* and typical of its historical development. String theorists have consistently reacted to, and neatly sidestepped new developments. Supersymmetry can only provide support for string theory if it is found, but would not falsify string theory if not found. Smolin identifies this as a weakness: "while supersymmetry is not precisely unfalsifiable, it is difficult to falsify" in practice because "negative results can be – and often are" accommodated simply "by changing the parameters of the theory" (Smolin, 2007b, p. 322). The different roles of supersymmetry throughout the history of string theory illustrate this point. Supersymmetry was originally introduced to string theory to render the theory free of instabilities and to include fermions, whereupon it became so integral to the theory as to be a "genuine prediction". Yet the absence of any experimental evidence for supersymmetry does not pose a fatal threat to the theory.

1.5 Retrodictions and counterfactual histories

Some defenders of string theory have sought to respond to these attacks on its scientific legitimacy by using a different strategy. Rather than point to the possibility of making novel predictions, they

[30] Recent developments at the LHC have cast doubts over finding evidence for supersymmetry at an energy scale below 1 TeV.
[31] A similar strategy was employed in the defence of Copernican astronomy against Tycho's objection that we cannot observe stellar parallax. Here it was assumed that the orbits of the planets must be 700 times larger than was thought to be the case in the geocentric universe.

instead have instead emphasized that string theory predicts certain observed phenomena for which experimental evidence "already exists" (Greene, 2004, p. 378). In this sense, string theorists often define gravity as a 'prediction' of string theory. As Witten contends: "these theories have (or this one theory has) the remarkable property of predicting gravity" (Witten, 1996, p. 24). In a 'Bloggingheads' interview with Woit, philosopher of science, Craig Callender, recounted a personal experience as a graduate student when he was asked to drive Witten to and from a conference in 1994 or 1995. While driving Witten Callender asked "why should I believe string theory, what experimental evidence is there of this?" Witten's answer was "that things fall" (Bloggingheads., 2009, p. Time: 35 minutes).

Here it is important to appreciate that string theory originated, not as a theory of gravity, but as a theory of the strong nuclear force. In 1974 John Schwarz and Joël Scherk claimed that the massless spin-2 particle could be interpreted as the graviton – the theoretical exchange particle of the gravitational field. The prediction of a massless spin-2 particle, which initially had been seen as an anomaly of the theory, was now seen as pointing to a unified theory of quantum mechanics and gravitation. Gravity emerged, surprisingly, as a necessary consequence of the theory. Both Greene and Witten acknowledge that this kind of prediction is better termed 'retrodiction' given the phenomena of gravitation was already well-known to physicists (Greene, 1999a, p. 225).

Here physicists employ counterfactual histories in their writings to convey the impression that string theory can predict phenomena that are already known to exist. Witten has speculated that perhaps other advanced life forms in the galaxy discovered string theory first and "a theory of gravity found as a stunning consequence" (Witten paraphrased in Greene 1999, p. 211). Brian Greene has also speculated along these lines: "had history followed a different course – and had physicists come upon string theory some hundred years earlier – we can imagine that these symmetry principles would have been discovered by studying its properties" (Greene, 1999a, p. 375). The intended impact of this argument is to make the string theory's lack of predictive power a consequence of its contingent history. This is an attempt to undermine criticism that string theory is not scientific because it does not make predictions. Instead string theory is a casualty of the history of science and in this context the ability to 'retrodict' is deeded to be sufficient to make a claim to be scientific.

1.6 String theory makes progress by solving problems

Some critics, such as Nancy Cartwright and Roman Frigg (2007), have typically portrayed string theory as a degenerating research program, in a Lakatosian sense, for its failure to make novel testable predictions. According to Lakatos:

> "A research program is said to be progressing as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success ('progressive problem-shift'): it is stagnating if its theoretical growth lags behind its empirical growth, that is, as long as it gives post hoc explanations of either chance discoveries or of

> facts anticipated by, and discovered in, a rival programme ('degenerating problem-shift')". (Lakatos 1978, p. 112)

Cartwright and Frigg concluded in their Lakatosian analysis of string theory that string theory can be characterised as a degenerating research program (Cartwright & Frigg, 2007, p. 20). Yet string theorists maintain that string theory has made considerable theoretical progress over the last three decades, in solving long standing problems, such as non-renormalizability, that had plagued earlier efforts in quantum gravity. In a critical review of Smolin's book, Polchinski pointed out that in spite of the absence of experimental predictions, string theory has continued to make progress because it has been "able to solve some key problems that otherwise seemed insurmountable" (Polchinski, 2007a).

This view, adopted by most string theorists, is in many respects close to view of scientific progress articulated by Larry Laudan, which highlights that scientists working within a research tradition attempt to solve conceptual, as well as empirical, problems (Laudan, 1977). As Duff puts it, string theory has continued to "make remarkable theoretical progress", through the development of new symmetry principles, new techniques in perturbation theory, the classification of Calabi-Yau manifolds, and the discovery of dualities between different kinds of theories (Duff, 2013, p. 184). Indeed, in a recent interview, Brian Greene declared that the "enormous amount of progress in string theory" over the past decade had only strengthened his conviction "that this is a worthwhile direction to pursue" (Greene quoated in (Moskowitz, 2011)).

Here I draw attention to two classic examples of problem solving from the history of string theory. In 1984 Michael Green and John Schwarz published a landmark paper, in which they solved one of the crucial problems that had confronted earlier versions of string theory, and indeed all previous attempts to unify quantum theory and general relativity (Green & Schwarz, 1984, p. 49). Green and Schwarz showed that certain quantum mechanical anomalies in superstring theory (which violated gauge invariance) could be made to cancel each other out with the application of one of two symmetry groups if they were formulated in ten dimensions. For the first time, physicists could construct finite, perturbative string theories that encompassed a symmetry group from the Standard Model and which neatly avoided the renormalisation problem of infinite self-energies for the gravitational field (Chalmers, 2007, p. 38). This result, which heralded the beginning of the "first superstring revolution", is often cited by string theorists as, more than anything else, responsible for the enormous interest in string theory during the 1980s.[32]

[32] Some contest this claim and instead argue that it was Witten's involvement in string theory, just prior to the publication of Green and Schwarz's paper, which generated the excitement over string theory. This point of conflict is discussed further in section 2.1 of chapter three.

A second often-cited triumph of string theory is the resolution of the paradox of black hole entropy first raised by Stephen Hawking in the 1970s. The development of new non-perturbative tools such as, what would be come to known as, the anti-de Sitter/conformal field theory correspondence conjecture (AdS/CFT duality) in the latter half of the 1990s made possible the application of string theory to thermodynamic properties of black holes at the quantum level, and provided “the first microscopic derivation of the black hole entropy formula first proposed by Hawking in the mid-1970s” (Duff, 2013, p. 184). This result is often touted as one of the resounding successes of string theory. As Duff has put it, “Solving long outstanding theoretical problems such this indicates that we are on the right track” (Smolin et al., 2007, p. 9).

While some critics portray string theory as languishing in a state of crisis, highlighting its failure to make testable predictions, defenders argue that string theory has made theoretical progress and has solved many of the key problems that have stood in the way of the realisation of a unified theory. Susskind and Duff have also responded to the charge that string theory is not a science by pointing out that many of the mathematical tools developed by string theorists have been applied in many other branches of physics and mathematics. To this extent, string theory has already proved it worth as a science “whether or not ‘a theory of everything’ is forthcoming” (Duff, 2013, p. 199). Susskind points out that “string theory has had relevant things to say to a wide community of physicists and mathematicians, from black hole theorists to nuclear physicists to particle phenomenologists to geometers” (quoted in Chalmers 2007, p. 47). As Mikhail Shifman explains, string theory “exhibits a very rich mathematical structure, and provides us with new, and in a sense superior, understanding of mathematical physics and quantum field theory” (M Shifman, 2012, p. 10).

The anti-de Sitter/conformal field correspondence (AdS/CFT duality), first proposed by Juan Maldacena in 1997, marked a major theoretical breakthrough by providing physicists with a non-perturbative definition of string theory (J. Maldacena, 1997, 1999). However it has also found practical application in areas of cosmology and condensed matter physics, by making possible calculations in strongly coupled gauge theories that would otherwise be intractable (Chalmers, 2007, p. 42). Through this new tool, it has become possible to model certain aspects of the strong force in situations in which quarks behave as if they are free particles, which cannot be solved analytically in perturbative quantum field theory. String theory research has also led to new advances in algebraic geometry, the topology of higher dimensional spaces, conformal field theory, and quantum information theory (Chalmers, 2007, p. 42). Maldacena’s paper was the top cited paper in high energy physics (hep-th) every year baring 2001 from 1998 to 2010 (as measured by inSPIRE), which illustrates the reach beyond string theory as a TOE.

Smolin and Woit have been keen to point out that these projects are spin-offs and have increasingly become largely divorced from the original program of string theory unification (Woit, 2011b).

Topological string theory, for example, uses "simplified versions of string theory" that "do not unify the forces and particles observed in nature" (Smolin, 2006b, pp. 195-196). Smolin argues that in evaluating the progress of string theory, one must "separate the question of whether string theory is a convincing candidate for a physical theory from the question of whether or not research into the theory has led to useful insights for mathematics and other problems in physics" (Smolin, 2006b, p. 177). Yet in shifting the terms of the debate in this way, even Peter Woit has conceded that there is "a reasonable case to be made for continuing interest in string theory" (Woit, 2012c). If string theory has proved so useful for branches of physics whose scientific status is not in question, it can be argued it forms a legitimate part of physics.

The key issue here is here is the relationship between string theory as a TOE and as a tool. For Duff, problem solving achieved in tool string theory is considered sufficient to render the string theory research program as a whole progressive and to secure the scientific status of string theory. Furthermore progress in tool string theory is taken as evidence that TOE string theory is 'on the right track', that is to say progress in tool string theory is taken as evidence that the project of developing string theory as a theory of quantum gravity is making progress towards the 'truth' (what Camilleri and Ritson have described as teleological progress (Camilleri & Ritson, 2015). Duff's claim is distinct from the claim expressed by Rickles (Rickles, 2013b) where he takes progress in tool string theory as evidence that the string theory research program as a whole is a rational pursuit. When Smolin and Woit appraise string theory, they are in agreement with the Susskind, Shifman and Duff that there has been progress in tool string theory but then disagree as to how to interpret this in terms of the scientific status of the string theory research program. For Smolin and Woit progress in tool string theory is sufficiently divorced from TOE string theory that it should not be considered as relevant evidence for the TOE view of string theory.

1.7 Against Falsificationism

As should be clear from the preceding section, much of the criticism of string theory's legitimacy as a science has revolved around the question of whether string theory is falsifiable. This may well strike many readers as somewhat odd, given that very few philosophers of science would subscribe to a Popperian view of science today. Yet as Peter Godfrey-Smith observes, whereas Popper no longer commands the status he once did within academic philosophy of science, among professional scientists "Popper's standing is quite different." As the string theory debates show "Popper's philosophy is a resource drawn on by scientists in internal debates about scientific matters" (Godfrey-Smith, 2007). It is not clear that those that draw upon falsification as a demarcation criteria have read Popper: where individuals such as Ellis and Silk draw upon falsification they draw upon naïve falsification that has little to do with the evolving position outlined by Popper (Popper, 1959, 1963). This amounts to falsification understood as a slogan: "as the philosopher of science Karl Popper argued: a theory must be falsifiable to be scientific" (G. Ellis & Silk, 2014, p. 321). Nevertheless, a few string theorists, most notably Leonard Susskind, have explicitly attacked the appeal to falsifiability, and have argued that the criticisms of string theory as unfalsifiable are based on a fundamental misunderstanding of the way science works. To this end, Susskind has strongly defended the scientific status of string theory, labelling critics like Smolin and Woit as the "Popperazzi" (Susskind, 2005, p. 192).

Here Susskind responds to the critics by construing their arguments as 'philosophical' objections, which are largely irrelevant to the actual practice of science. Quoting Feynman, he states: "philosophers say a great deal about what is absolutely necessary for science, and it is always, so far as one can see, rather naive, and probably wrong" (Feynman quoted in Susskind 2005, p. 192).[33] By labelling the criticism as philosophical and not scientific, Susskind engages in what Gieryn has called "a second-order cartographic squabble" about "who really has the epistemic authority to map science" (Gieryn, 1999, p. 28). Scientists, not philosophers, in Susskind's view, may determine what legitimately counts as science and what does not:

> "Good scientific methodology is not an abstract set of rules dictated by philosophers. It is conditioned by, and determined by, the science itself and the scientists who create the science. What may have constituted scientific proof for a particle physicist of the 1960's – namely the detection of an isolated particle – is inappropriate for a modern quark physicist who can never hope to remove and isolate a quark. Let's not pull the cart before the horse. Science is the horse which pulls philosophy." (Susskind, 2005, p. 192)

In defence of this view, Susskind attempts to marshal support from the history of science in refuting falsifiability as a satisfactory criterion of demarcation. This imposes too stringent and restrictive a criterion on what constitutes science. Here Susskind compares the Darwinian and Lamarckian theories of evolution, insisting that Lamarck's erroneous view of the inheritance of acquired characteristics was falsifiable, while Darwin's theory of natural selection was not. Naturally enough, Susskind allies himself with the victor: "Lamarckian theory is scientific because it is falsifiable", but "the theory is easily falsified – too easily" (Susskind, 2005, p. 194). Susskind's basic strategy here is to draw attention to the way that different scientific disciplines draw different methodological and epistemological norms and standards based on the nature of their inquiry. What holds for experimental particle physics will not hold for string theory.

Susskind also argues that confirmation, not falsification, should be the desired goal: "Falsification in my opinion is a red herring, but confirmation is another story. By confirmation I mean direct positive evidence for a hypothesis rather than the absence of negative evidence" (Susskind, 2005, p. 195). His claim is that it is possible to find evidence that confirms Darwinian evolution, but impossible to have

[33] There is a certain irony here, given that Feynman was one of the physicists who expressed serious concerns about the legitimacy of string theory in the 1980s (Feynman, 1987).

a test that could falsify it without the ability to travel back in time (Susskind, 2005, p. 194). The rhetorical nature of this argument should be obvious. By drawing examples of good science, which are not falsifiable in any simple sense, Susskind attempts to defend the legitimacy of string theory as a science.[34]

Sean Carroll has also argued that the falsifiability criterion is too stringent and restrictive (Carroll, 2014a, 2014b). When asked for the Edge Annual Question, 'what scientific idea is ready for retirement', Carroll argued that a commitment to falsifiability was "as non-scientific as it gets":

> "String theory and other approaches to quantum gravity involve phenomena that are likely to manifest themselves only at energies enormously higher than anything we have access to here on Earth. Some scientists, leaning on Popper, have suggested that these theories are non-scientific because they are not falsifiable.
>
> The truth is the opposite. Whether or not we can observe them directly, the entities involved in these theories are either real or they are not. Refusing to contemplate their possible existence on the grounds of some a priori principle, even though they might play a crucial role in how the world works, is as non-scientific as it gets." (Carroll, 2014a)

Just as Susskind offered confirmation as a superior demarcator, Carroll also defends the legitimacy of string theory as a science by proposing that scientific theories should be "definite" and "empirical" (Carroll, 2014a). String theory is argued to be "definite" as according to string theory "in certain regions of parameter space, ordinary particles behave as loops or segments of one-dimensional strings. The relevant parameter space might be inaccessible to us, but it is part of the theory that cannot be avoided" (Carroll, 2014a). Carroll takes care in separating the notion of falsifiability from the notion of "empirical", arguing that "in the real world" the relationship between experiment and theory is complex and what is crucial is "the ability to account for the data" (Carroll, 2014a). Each definition is set up so as to legitimise an undertaking that in part describes that which is either in principle or outright unobservable and in part describes that which is observable as scientific. Carroll takes it so far as to argue that a commitment of falsifiability is non-scientific: "it would be completely non-scientific to ignore that possibility just because it doesn't conform with some preexisting philosophical prejudices" (Carroll, 2014a).

Carroll's proposal was criticised using an alternate rhetorical strategy: humour, in an April Fool's Day joke paper uploaded to the arXiv, string theory and the multiverse are mocked as "some of the most obviously correct physical theories [that] make no testable predictions". The paper "quotes a lot of

[34] Historians and philosophers of science, such as Peter Galison (Galison, 1995b) and Richard Dawid (Dawid, 2013b) (Dawid, 2013a) have also suggested that the emergence of string theory in the 1980s brought with it a more radical departure from the strictures of a traditional empiricist methodology than even Susskind recognises. These are discussed in section 1.4 of chapter one.

famous people" to argue that abandoning falsifiability on the basis of string theory and the multiverse does not go far enough and that "that we should also dispense with other outdated ideas, such as Fidelity, Frugality, Factuality and other "F" words" (Scott, Narimani, & Frolov, 2015, p. 1).

1.8 The landscape of string theory: physics or metaphysics?

In spite of Susskind and Carroll's attempts to rescue string theory from the "Popperazzi", concerns about the slide from physics into speculative metaphysics continue to be raised. One of the major difficulties that has confronted string theorists since the 1980s is that there is no way of deriving a unique set of properties which describe the properties like mass and charge of the known elementary particles and forces from the mathematical framework of string theory (or M-theory). As Brian Greene explains, physicists have found that the equations of superstring theory "have many solutions", each "corresponding to a universe with different properties" (Greene, 1999a, pp. 284-285). Initially it was hoped that theoretical constraints and consistency requirements would enable physicists to pick out a single solution that corresponds to our universe, however the recent discovery that the cosmological constant has a positive value only served to exacerbate the problem. While some string theorists, such as David Gross, have argued that we should not abandon the hope that string theory will lead to a unique vacuum state, many physicists now see this increasingly remote possibility. Taking into account the more than one hundred million known Calabi-Yau spaces together with the problem of vacuum degeneracy, it is now estimated that there are in the order of 10^{500} string theories, perhaps more, each one describing different set of particles and forces (Conlon, 2006, p. 47).

Sean Carroll and Michael Green have argued that while this might seem disastrous, we should not despair about the inability to derive the parameters of the Standard Model. Carroll argues we may well be forced to abandon the "the hope that string theory would predict a unique vacuum state". However, much as we would have liked to make such predictions, "the inability to do so doesn't render string theory non-scientific" (Carroll, 2005b). Here Carroll draws an analogy with quantum field theory, in which "the observable spectrum of low-energy string excitations and their interactions … depends not only on the fundamental string physics, but on the specific vacuum state in which we find ourselves" (Carroll, 2005b). Michael Green makes a similar point in drawing a comparison with general relativity: "This supposed problem with a theory having many solutions has never been a problem before in science. There is a "landscape" of solutions to generate general relativity, yet nobody says the theory is nonsense because only a few of them describe the physics we observe while the rest appear to be irrelevant" (Green quoted in Chalmers 2007, p. 44). Yet, as Green points out, the case in string theory is admittedly different, insofar as "each different solution defines a different set of particles and fields", not merely a different spacetime geometry (quoted in Chalmers 2007, p. 44).

In recent years a number of string theorists, most notably Susskind, have interpreted this situation, not as a failure of string theory, but as an indication that our conception of the universe must be radically revised. Since 2003 Susskind has argued that the failure of string theory to explain the particular combination of particles and forces described by the Standard Model reflects a deeper reality that no such unique combination exits in nature. As he puts it, “blinded by the myth of uniqueness,” string theorists in the 1980s and 90s “continued to hope that some mathematical principle would be discovered that would eliminate all but a single possibility”. It now appears that “although the theory may be correct, their aspirations were incorrect. The theory itself is demanding to be seen as a theory of diversity, not of uniqueness” (Susskind, 2005, p. 274). Here Susskind advances the controversial view of the multiverse, in which the different solutions of the theory represent different universes, or pocket universes, which may exist in different spacetime regions or at different epochs, or some combination of the two. Thus the apparent failure of string theory to predict a unique set of properties corresponding to the Standard Model has, for Susskind, opened up fundamental new insights in cosmology.

The consequences of this view are indeed startling, and have divided the string theory community. Indeed many string theorists like David Gross have strongly opposed the multiverse, and argued that despair is premature. As Mikhail Shifman explains, Susskind’s proposal constitutes “probably the most dramatic change of paradigms from Newton times. In a sense it was born out of desperation” (M Shifman, 2012, p. 11). Here the “failure of the original program” becomes “a triumph” (Shifman 2012, p. 11). Yet, there is a cost. The other universes “are causally disconnected from ours, so there is no physical way to confirm their existence or non-existence in experiment” (M Shifman, 2012, p. 11). Steven Weinberg goes so far as to suggest that the multiverse may well constitute “a new turning point” in our conception of science, forcing “a radical change in what we accept as a legitimate foundation of a physical theory” (Weinberg, 2009, p. 30). Critics of string theory see this as further evidence of the extraordinary lengths string theorists will go to in order to protect the theory from falsification (Kragh, 2011a, p. 303). Rather than a “theory of everything”, string theory may well degenerate into a “theory of anything”, or perhaps “a theory of nothing” (Smolin, 2006b, p. 150).[35] Smolin calls for physicists to strongly resist “special pleading that the standards of science should be lessened to admit explanations with no falsifiable consequences, in order to keep alive a bold speculative idea” (Smolin, 2013, p. 24).

Smolin insists that speculative cosmological scenarios (such as eternal inflation, cyclic and pluralistic cosmological models, and cosmological natural selection) are admissible in physics, but they can only be taken seriously if they “make falsifiable or strongly verifiable predictions” (Smolin, 2013, p. 23). Indeed there have been recent attempts to develop models that do just this (Smolin, 2013; Susskind, 2013) and some cosmologists, such as Aurélien Barrau, argue that “the multiverse remains within the realm of Popperian science. It is not qualitatively different from other proposals associated with the usual ways of doing physics” (Barrau, 2007).

[35] Reiner Hedrich has argued that string theory has morphed from a prospective theory of physics into a “mathematically inspired metaphysics of nature” (Hedrich, 2007, p. 269).

Whilst Woit was prepared to grant that string theory could be considered science as long as a large part of the scientific community considered the speculations of string theory reasonable (Woit, 2006d, pp. 214-215), he is significantly more critical of the multiverse hypothesis as a response to the landscape due to what he sees as circular reasoning (Woit, 2012d). The circularity, according to Woit, arises because the landscape has rendered string theory untestable as "the multiverse implies that all the things you would think that string theory might be able to predict turn out to be unpredictable local environmental accidents", yet because the multiverse is unobservable it "must be justified in terms of another theory that can be tested and this is string theory". So, according to Woit: "the multiverse can't be tested, but we should believe in it since it's an implication of string theory, but string theory can't be tested because of the multiverse" (Woit, 2012d). For Woit this renders any claim for a multiverse based on string theory as pseudoscientific (Woit, 2012d).

Ellis and Silk also expressed concern, in an op-ed piece in Nature titled 'Scientific Method: Defend the integrity of physics', at what they perceived to be a "change in how theoretical physics is done" away from empiricism (G. Ellis & Silk, 2014, p. 321). Citing string theory and the multiverse interpretation as catalysts for their concern, they argue that "physicists, philosophers and other scientists should hammer out a formal narrative for the scientific method that can deal with the scope of modern physics" and that this hammering out should take place at a convened conference (G. Ellis & Silk, 2014, p. 323).[36] Woit has also a pleaded for physicists to be vigilant in upholding "strong internal norms of rationality" in an effort "to ensure that science continues to deserve that name" (Woit, 2006d, p. 216). Here Ellis, Silk and Woit make explicit appeal methodological norms of scientific inquiry.

2. The string theory debates and the ideology of physics

While many of the points of disagreement between critics and defenders of string theory turn on complex, highly technical matters not discussed in this chapter, the debate raises a number of issues that go well beyond the sphere of theoretical physics, such as prescriptions concerning the nature of scientific progress and the demarcation of science from non-science. Critics have attempted to highlight what they see as serious methodological problems of string theory and have called into question both its legitimacy as a science and its institutional dominance and virtual monopoly of resources. In defending string theory against these attacks, string theorists have employed various strategies in attempting to construct a boundary between science and non-science which casts their own activities in a favourable light. The dialectical nature of boundary work is in evidence here, as both critics and defenders of string theory have responded to one another in changing ways.

[36] This conference occurred in Munich in December 2015. Both Ellis and Silk were among the conference organisers and both Ellis and Silk spoke at the conference (G. Ellis, 2015; Silk, 2015).

The string theory controversy also brings into sharp focus the importance of the public nature of boundary work and the rhetorical function of popular science. As Gieryn has noted, in controversies of this kind, “scientists describe science for the public and its political authorities, sometimes hoping to enlarge the material and symbolic resources of scientists or to defend professional autonomy” (Gieryn, 1983, p. 781). This seems an entirely apt description of the recent public controversy over string theory.

The attempts to define what constitutes good science can be considered as ideological in Gieryn’s sense, insofar as protagonists are motivated in part by “the pursuit of professional goals: the acquisition of intellectual authority and career opportunities” (Gieryn, 1983, p. 781). But the debates about string theory may also be said to be ideological in the sense that the protagonists on both sides attempt to set out their views on science, which they hope may shape the direction of physics in the future. In this regard, Woit has said that he would like his book to be thought of as useful reading for those interested in entering the field so that they can make better-informed decisions. Smolin describes his work as “a serious book,” which attempts to deal with the current crisis in physics, “not a popularisation”. Indeed, Smolin explains that his decision to write *The Trouble with Physics* was motivated primarily by philosophical and sociological concerns. His aim was to present “a view of what science is and how science works” (Smolin et al., 2007). The responses by Greene, Susskind, Polchinski, and Duff also take up this challenge. It is evident here how these works of popular science do more than merely disseminate complex scientific ideas for the wider public (Daum, 2009, p. 100). They present different ideologies of physics.

While Gieryn’s notion of boundary work provides a useful way of framing a certain aspect of the debate, there is an important sense in which the string theory controversy differs in certain crucial respects from most of the cases typically studied by sociologists of science. In most scientific controversies in which we find scientists engaging in boundary work, the boundary dispute is generally over whether an unorthodox or minority view or approach should be regarded as science, pseudoscience, or pathological science. UFOology, parapsychology, intelligent design, and cold fusion all represent cases of this sort. The “ideological attempts to define science”, as Gieryn explains, are largely motivated by the desire “to justify and protect the authority of science by offering principled demarcations from poachers or impostors” (Gieryn, 1999, p. 26). However, in the case of string theory, it is the dominant research program in a well-established field of science that has been forced to defend its credentials as “scientific” (Taylor, 1996, pp. 177-179).

This presents an intriguing departure from most studied episodes of boundary work. String theory currently enjoys a privileged status by virtue of being the dominant paradigm within theoretical physics. Yet string theorists have found themselves forced to defend the scientific legitimacy of their research against charges that it has degenerated into a form of “metaphysics”, “non-science,” or “pseudoscience”. In doing so, string theorists have attempted to “loosen” the methodological definition of science, while critics try to impose a stricter definition. This appears to be the reverse of the usual practice in boundary disputes, in which the prevailing scientific orthodoxy attempts to impose more stringent demarcation criteria in an effort to exclude certain intellectual activities they deem pseudoscientific (Taylor, 1996, p. 91). In this way, the string theory debates serve to enrich our understanding of the nature of boundary work, and the specific historical contexts in which scientists engage in the ideological discourse over what legitimately counts as science.

Chapter Three: Contested sociologies

Introduction

Many of the points of disagreement between critics and defenders of string theory turn on complex, highly technical matters, and the demarcation of science from non-science. This chapter, however, focuses on the debates over the sociological norms of scientific inquiry and prescriptions concerning the nature of scientific progress. Where the debates are identified as sociological, they are so identified because they are characterised as sociological by individuals in the debates. Critics have called into question factors contributing to string theory's institutional dominance and effective monopoly of resources. In defending string theory against these attacks, string theorists have employed various strategies in attempting to construct a boundary between good science and bad science, which casts their own activities in a favourable light. The dialectical nature of boundary work is nicely revealed here, as both critics and defenders of string theory have responded to one another in changing ways.

Two books, critical of string theory, were published in 2006 in quick succession. Peter Woit, who had previously engaged in sustained critique of string theory and the behaviour of string theorists on his blog Not Even Wrong (Woit, 2004 - Present), published a book with the same title (hereafter 'NEW'). The book was initially published in the United Kingdom in June by non-academic publishing house Johnathon Cape. The title was extended from the blog title to read: *Not Even Wrong: The Failure of String Theory and the Continuing Challenge to Unify the Laws of Physics* (Woit, 2006d). The US edition was published in September of the same year, with a slightly different title, *Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law*, by another non-academic publishing house, Basic Books (Woit, 2006e). Lee Smolin's book, *The Trouble with Physics: The Rise of String Theory, The Fall of a Science and What Comes Next*, was also published in the UK[37] and the US at the end of August in 2006 by Penguin and Houghton Mifflin publishing houses (hereafter 'TTWP') (Smolin, 2006c) (Smolin, 2006b). One consequence of the timing of the publications was that many commenters conflated the arguments contained within each book and while there were several similarities the books differ in many key respects. A second consequence was that, at the very least in the eyes of the press,[38] the idea of a 'crisis' in high energy physics gained considerable traction. Supporters of string theory reacted against this and both attempted to discredit Smolin and Woit's arguments and to provide positive arguments in support of string theory. In the years since, many of those who participated in the debates over string theory have described the

[37] The UK edition differs slightly in appearance as Smolin had the subtitle removed from the cover following the reaction of some string theorists.

[38] On October 20 2006 George Johnson, science journalist in residence at KITP, led a discussion on the 'string wars', which focused on the "loud media reaction" following Smolin and Woit's books in the press (G. Johnson, 2006). Some examples include (Vergano, 2006) (Brumfiel, 2006) (Lemonick, 2006) (McKie, 2006).

period of time from the summer of 2006 and into 2007 as being the most intense period of the ‘string wars’ (Alejandro, 2006) (G. Johnson, 2006) (Carroll, 2006b).

Both Smolin and Woit dedicate parts of their book to the argument that ‘the academy’ has allowed string theory to dominate (Smolin, 2006c) or, in Woit’s case, to become ‘the only game in town’ (Woit, 2006e). Smolin argues that string theory has been able to monopolise scarce resources and to manipulate professional opportunities, preventing resources from going to existing alternative approaches. Woit, who does not advocate for a rival program, takes it a step further and questions whether a competitive alternative will ever be able to develop in the academy whilst string theory is ‘the only game in town’. The books of Smolin and, to a lesser extent, Woit[39] were very effective in inciting debate over sociological effects within the academy, and a number of other participants joined the dialogue. The sociological critique of string theory evolved to take on a number of dimensions, including criticism of the dominance that has allowed string theorists to corner the grant market and restrict employment opportunities. There is also criticism of the use of popular media to distort the public image of string theory; this is also argued to further string theorists’ control of resources. Accusations of distorting the image of string theory extend to leaders within the field and accusations of arrogant, or even bullying, behaviour are levelled at the string theory community in general.

Not surprisingly, some string theorists have mounted a vigorous defence of string theory. Mike Duff argues that Smolin’s book represents “a venomous attack on string theory and its practitioners” (Smolin et al., 2007, p. 5). Smolin’s characterisation of string theory and his allegations of institutional bias and of self-serving hiring practices infuriated many string theorists, some of whom have simply refused to engage in debate. While Duff concedes that “some string theorists are arrogant, exclusive and unwilling to listen to unorthodox views”, he maintains that Smolin’s book gives a distorted and misleading account of the situation in string theory (Smolin et al., 2007, p. 5).

These are just some examples of the various norms that participants in the debates over ‘sociology’ and string theory debate allege have or have not been adhered to by the string theory community. Christine Dantas, in a comment on string theorist Clifford Johnson’s blog, characterised the situation in the following terms:

> “Given the state of affairs, that is, fundamental misunderstandings among debaters, some kind of convergence seem to be very far from obvious. But is it really a storm in a teacup? What is really happening? …
>
> Mostly, these misunderstandings are clearly related to the vision of science that people have.” (Dantas commenting on (C. Johnson, 2006f))

[39] Smolin’s book contained a significantly longer and more sustained critique of sociological forces in high energy physics. I do not mean to suggest here that Woit’s book was less influential; merely that his primary focus was not on sociological considerations and consequently the book incited less debate over ‘sociology’.

String theorists like Polchinski, Gross, Susskind, Johnson and Duff have in their responses provided alternative sociological views of string theory. Critics and supporters alike argue that the social organisation of the string theory community is significant for the epistemic appraisal of string theory, as well as for the projective appraisal of the 'promise' of string theory.

The debates about string theory may also be said to be 'ideological' in the sense that the protagonists on both sides attempt to set out their views on the normative structure of science, which they hope may shape the direction of physics in the future. As Jasanoff has argued:

> "The questions that loom as interesting, then, have to do with: the nature of categories and classifications … with the agents, instruments and processes that produce these classifications; with patterns of inclusion and exclusion on either side of the line of expertise; and with the influence of history and culture on the drawing and redrawing of these kinds of boundaries. The project of looking at the place of expertise in the public domain appears in this light as a project in political (more particularly democratic) theory, with epistemological questions embedded in it, but not wholly reducible to epistemology." (Jasanoff, 2003, p. 394)

In this regard, string theory critic, Woit has said that he would like his book to be thought of as useful reading for those interested in entering the field so that they can make better-informed decisions. Smolin describes his work as "a serious book", which attempts to deal with the current crisis in physics, "not a popularisation". Indeed, Smolin explains that his decision to write TTWP was motivated primarily by philosophical and sociological concerns. His aim was to present "a view of what science is and how science works" (Smolin, Duff & Cartwright 2007).[40] The responses by string theorists Greene, Susskind, Polchinski, Johnson and Duff also take up this challenge. These works, in contrasting ways, each establish notions of 'dominance' and of 'expertise'.

As was argued in the previous chapter, there is an important sense in which the string theory controversy differs in certain crucial respects from most of the cases typically studied by sociologists of science. In most scientific controversies in which we find scientists engaging in boundary work, the boundary dispute is generally over whether an unorthodox or minority view or approach should be regarded as 'science', 'pseudoscience', or 'pathological science'. UFOology, parapsychology, intelligent design and cold fusion all represent cases of this sort. The "ideological attempts to define science", as Gieryn explains, are largely motivated by the desire "to justify and protect the authority of science by offering principled demarcations from poachers or impostors" (Gieryn, 1999, p. 26). However, in the case of string theory, there is consensus between those who seek to criticise and those who defend string theory that it is the dominant research program in a well-established field.

[40] Woit claimed that his primary motivation for writing *Not Even Wrong*, the book (Woit, 2006e), and Not Even Wrong, the blog (Woit, 2004 - Present), was "the environment for people just starting out. Can something be done to encourage them to try really new things, not just follow down the same well-worn path that does not clearly seem to be leading anywhere?"

This presents an intriguing departure from most studied episodes of boundary work. During the 'string wars' string theory was considered dominant within quantum gravity research by both those that sought to defend and to criticise the research program. Yet string theorists were forced to defend their authority as the dominant research program against accusations of groupthink behaviour, of monopolising funding, of distorting the public image of string theory and of a lack of honesty regarding the problems within the discipline. This appears to be the reverse of the usual practice in such disputes, in which the prevailing scientific orthodoxy attempts to impose more stringent criteria on a minority view or approach in an effort to exclude certain intellectual activities they deem 'pseudoscientific' (Taylor 1996, p. 91). In this way, the string theory debates serve to enrich our understanding of specific historical contexts in which scientists engage in the ideological discourse over what legitimately counts as scientific activity.

The uniqueness of this example and the examination of the debates over how to interpret the dominance of string theory in appraisal of string theory will enrich arguments made by philosophers of science, such as Dawid, who draw upon the descriptive claim that string theorists argue that there are "no alternatives" (Dawid, 2013a, 2013b). It will also provide qualified support that the argument of no alternatives increases trust in string theory as a final theory (Dawid, 2013a, p. 88) (Dawid, 2013b, p. 51). These arguments are discussed further, with reference to the philosophical literature, in section 1.4 of chapter one.

This chapter will not consider the question: was (or is) string theory a dominant research program? Instead this chapter examines how dominance is constituted and reconstituted over the course of the debates. This is because the chapter attempts to provide a description of the 'sociological' debates over string theory and an analysis of those debates (as opposed to analysis of the string theory research program as dominant or otherwise). Indeed, neither the supporters nor critics of string theory question the dominant nature of string theory in their normative appraisals. This agreement on the dominance of string theory is typical of debates over string theory in which critics and supporters alike unite as to the cogency of particular categories, such as dominance, and then dispute how a category should be constrained. In particular, for the participants in these debates, there is unanimous agreement of the categories of 'dominance', 'popular' or 'public' and 'expertise', and also significant dispute as to how these categories should be understood and applied in normative judgements of good and bad science.

The critique of string theory that identifies 'sociological' issues does not originate with the books written by Smolin and Woit. Woit's own blog, also titled Not Even Wrong, preceded the book and several posts contained many of the arguments he presented in the book version.[41] There is evidence

[41] See (C. Johnson, 2005) (Trodden & Krauss, 2005) for some examples. Johnson claims the discussions began in the summer of 2005: "since the Summer of 2005 we've had many a detailed public discussion about several things about research in string theory, particle physics, and the like -scientific, sociological, and otherwise,

of sociological critiques of string theory as early as 1986 (J. Ellis, 1986, pp. 595, 597), 1988 (Davies & Brown, 1988)[42] and 1995 (Horgan, 1997). And while a considerable number of the participants in the debate were based in North America, there were also critiques that came out of Japan (Nakanishi, 1986a, 1986b, 1993, 2006a). This chapter, however, focuses on the sociological critiques that were developed separately by Smolin and Woit because of the significant response with which those critiques were met. Many string theorists and other members of the high energy physics community attempted to engage with the critiques, resulting in several extended debates. The responses and ensuing dialogues make up most of the source material for this chapter, including in-person debates, book reviews, blog posts, journal papers, blog commentary sections and interviews.

The sociological literature on string theory is in an intriguing position. Despite the, sometimes high profile, debate in the theoretical physics community, there currently does not exist a detailed sociological study of the string theory research program or of string theorists.[43] This may be because sociologists and historians are reluctant to take on the string theory debates as a subject until some kind of consensus position has formed. However there is an advantage in analysis prior to consensus in the theoretical physics community, as both the ‘winner’ of this debate and how resolution of the debate will occur are far from obvious. The threat of ‘whiggishness’, whilst not absent, is minimised. When we consider controversies where the ‘science is settled’, such as vaccines and climate change, a normative application of dominance and expertise are often considered to be unproblematic. Any attempt to balance out an expert majority with an alternative view point is met with howls of outrage. This chapter attempts to describe how norms of science are constituted concurrently with consensus formation, without knowledge as to how the story might end.

The dominant nature of string theory is often assumed in the philosophical literature, such as in the opening line of Johansson and Matsubara’s paper: “string theory has evolved into a dominating field of research, perhaps the dominating one, in fundamental theoretical physics” (Johansson & Matsubara, 2011, p. 199). Later in the paper, Johansson and Matsubara comment that “string theory has a very dominant status, which is a criterion for being normal science” (Johansson & Matsubara, 2011, p. 203), and then, in the conclusion, state that they considered the dominance of string theory be a “matter of fact” (Johansson & Matsubara, 2011, p. 208). Ritson and Camilleri (Ritson & Camilleri, 2015) dedicated a section of their paper to the dispute over the boundary debate between good science and pathological science. This chapter seeks to build on this work to go beyond identification of the

starting at Cosmic Variance (e.g., the post above) and also here at Asymptotia. Not just myself, but several senior people have been involved at various points” (C. Johnson, 2007).

[42] In particular, see interviews with John Ellis (Davies & Brown, 1988, pp. 151-170) and Richard Feynman (Davies & Brown, 1988, pp. 192-211).

[43] The features editor of *Physics World*, in response to the ‘string wars’, attempted to prepare a short history of string theory and to map out the then-recent debates (Chalmers, 2007).

two sides of the debates, and to examine in detail the ways in which good science and pathological/bad science are constituted and the many points of conflict.

Methodology

Any kind of linear historical reconstruction of the debates would fail to capture how on different media several parts of the debates would be occurring at once, with different foci flaring and disappearing in different locations. Any kind of cartography in which each of the relevant issues was mapped with reference to related issues within the debate would also imply a permanence. This permanence is not an accurate portrayal of the debates as it does not reflect the ephemeral nature of some of the disputes occurring on blogs and their corresponding commentary sections.[44] Furthermore it would not capture the series of critiques and responses and responses to critiques and so on. A taxonomy of the debates would suffer from some of the same issues as a cartography due the lack of permanence of the constitution of the categories.

Despite the problems that a taxonomy of the debates undoubtedly causes, this chapter is organised by centres of conflict within the debates. These centres may be best thought of as cluster concepts. By separating out the debate into clusters around which a critique and a response occurred, the aim is to convey a sense of back and forth over isolated issues. This sense is not perfectly representative as the clusters emerged through sustained critique over many years. I argue that what is presented in this chapter is representative of certain points within a very tangled web of critique. However what this structure does achieve is to foreground certain concepts considered pertinent to the rhetorical construction of scientific norms. The three clusters described are ‘dominance’ and ‘expertise’. Each is constituted and reconstituted by protagonists within the debate. Whilst this strategy could be taken to imply that the categories of ‘dominance’ and ‘expertise’ have permanence, instead all that remains across each debate is a persistent belief by those who participate that the words ‘dominance’ and ‘expertise’ have traction, despite the slippery nature of the concepts within the debates.

To highlight the difficulties, some basic information with regards to a series of blog posts, some of which become sites of the string wars, is illustrative. Clifford Johnson, a string theorist at University of Southern California, wrote a post on his blog that was intended to be part one of two posts dedicated to the books written by Smolin and Woit and the resulting debates. He titled this post ‘More Scenes From the Storm in a Teacup, I’ (C. Johnson, 2006a), which was posted on August 21 2006. This two-part blog post evolved into a seven part series (C. Johnson, 2006a, 2006b, 2006c, 2006d, 2006e, 2006f, 2007), with the final instalment posted on 13 March 2007. Combined, these posts generated 787 comments, not deleted at the time of writing (4, 18, 8, 165, 91, 326, 175 comments respectively). The last comment was left on August 22 2007. Part VI is over 70 000 words and part

[44] For an exploration of the tension between the ephemerality and permanence of blog posts, see section 5.9 in chapter four.

VII is over 44 000 words in length. The length of the comments varies from singular short sentences to comments that are over 3000 words.

1. 'Sociology' and the 'academy'

1.1 Smolin and Polchinski's use of the word 'sociology'

Smolin's use of the word sociology is idiosyncratic. The clearest examples of his use of the word come from TTWP and in particular his chapter 'How do you fight sociology' (Smolin, 2006c). Also illustrative is the review of TTWP and NEW, written by Polchinski, published both in *Scientific American* (Polchinski, 2007a) and an extended version at Cosmic Variance (Polchinski, 2006). The publication of Polchinski's review on Cosmic Variance resulted in a vigorous debate in the commentary section and a response from Smolin (Smolin, 2007a). Also published on Cosmic Variance, Polchinski authored a response to Smolin's response to Polchinski titled 'Science or Sociology' (Polchinski, 2007b). 'Science or Sociology' also resulted in vigorous debate in the commentary section. Through this exchange we may see the ways in which each contributor to the debates demarcates 'sociology'.

One sense in which Smolin uses the word 'sociology' is as an attempt at describing how the term is used within the string theory community:

> "It is worth nothing that the word "sociology" comes up more nowadays among string theorists than among any other group of scientists I know. It seems to be shorthand for "the view of the community." In discussing the current state of affairs with young string theorists, you often hear them say things like "I believe the theory, but I hate the sociology." If you comment on the narrowness of viewpoints represented at string theory conferences or on the rapid succession of fashionable research topics from one year to the next, a string theorist will agree and add, 'I don't like it, but it's the sociology'." (Smolin, 2006c, p. 267)

Here Smolin claims that 'sociology' is an oft used phrase to describe both the attitudes and behaviours of members of the string theory community. By writing passively Smolin seems to be attempting to separate himself from critique of string theory. Instead he claims to be reporting on "a set of phenomena" that a collective refers to as 'sociology' (Smolin, 2006c, p. 267). However on each occasion he uses the term, Smolin draws connections between 'sociology' and 'view', be it "views", "viewpoint" or "fashionable". In this way Smolin sets up an understanding of 'sociology' as opinion, assessment or attitude.

The argument for interpreting Smolin in this way may be extended by examining how Smolin further constitutes his use of the term in a way that is intrinsically tied up in appraisals of string theory. Smolin continues: "More than one friend has advised me that, "the community has decided string theory is right and there is nothing you can do about it. You can't fight sociology" (Smolin, 2006c, p.

267). Smolin also makes a similar and stronger claim: “There is good evidence that the progress of string theory itself has been slowed by a sociology that restricts the set of questions investigated and excludes the kind of imaginative and independent-minded scientists that progress requires” (Smolin, 2006c, p. 270). Here we see Smolin construct his normative argument that a ‘sociology’ or a community opinion, assessment or attitude that lacks diversity with have a negative impact on the progress of science. This appraisal is projective, focused on the future, that is to say it is one which examines the ‘promise’ of string theory.

In his review of Smolin and Woit’s books, Polchinski is both in agreement and disagreement with Smolin and Woit’s views. Polchinski is in seeming agreement (or at least does not contest) that ‘sociology’ should be understood as meaning opinion, assessment or attitude: “A central question for both Smolin and Woit is why so many very good scientists continue to work on an idea that has allegedly failed so badly. Both books offer explanations in terms of the sociology of science and the psychology of scientists” (Polchinski, 2006). Disagreement lies in how the positive opinion, assessment or attitudes towards string theory should be understood. Polchinski claims that “much of what Smolin and Woit attribute to sociology is really a difference of scientific judgment” (Polchinski, 2006). The sections that follow will take up this claim, made here by Polchinski and elsewhere by others. However, it is relevant to see how the category of ‘sociological’ is set up here to be dichotomous to science. Here, we see Polchinski sets up a sociology versus science dialectic.

1.2 ‘The academy’ or ‘toy sociology’

Smolin and Woit both begin with claims that string theory, as an institutionally dominant research program, has monopolised resources and the public’s attention. Both authors support these arguments with a description of the ‘academy’. Smolin argues that the ‘groupthink’ mentality of string theorists is only part of the picture; the other important consideration is the role the academy plays. In a chapter titled ‘How Science Really Works’, Smolin claims “we need to inspect the dark underbelly of academic life. Because, as the sociologists tell us, it is not just about wisdom, it is about power: who has it, and how it is used” (Smolin, 2006c, p. 332). Woit also dedicates a chapter to describing the institutional forces. In particular, Woit argues that “one needs to look at what the standard career path is for ambitious, talented young physicians” (Woit, 2006e, p. 233). This chapter argues that each author builds a picture of the various forces at play within institutional physics, focusing in particular on outcomes faced by those that conform versus nonconformists.

1.3 Smolin’s academy

Smolin’s chapters ‘How Do You Fight Sociology’, ‘What Is Science’, ‘How Science Really Works’ and ‘What Can We Do For Science’ are contained in the fourth and final section of his book. It is in this section, titled 'Learning From Experience',[45] where Smolin outlines and develops his discussion of 'sociology' (Smolin, 2006c). Smolin's focus lies in describing the 'inner workings' of institutional hiring practices. Hiring practices are argued to be of significant importance because affiliation to an institution is fundamental to membership of a scientific community. Smolin argues that: "the system is set up so that we older scientists can reward those we judge worthy with good careers and punish those we judge unworthy with banishment from the community of science" (Smolin, 2006c, p. 333). This may be achieved through several mechanisms inherent in the hiring processes. One example Smolin offers is the custom of candidates for a faculty position requiring letters of recommendation. Smolin argues that the system is set up to enable the orthodoxy to prevail as "a professor will shamelessly write letters slanted toward his or her own students, or for people who are following his or her particular research program, or even for people of his or her own nationality" (Smolin, 2006c, p. 334).

A second impacting factor, which also favours hiring those who conform to the prevailing position, is funding considerations:

> "The goal is not only to hire good scientists. Hiring committees, chairs, and deans often have another goal in mind …it is, first of all, important to hire people who are likely to win generous grant support. This immediately favours members if large established research programs over initiators of new programs." (Smolin, 2006c, p. 337)

Furthermore, Smolin claims that these practices are both well-known and entrenched within the system (Smolin, 2006c, p. 334). Smolin, unsurprisingly, draws a picture of 'the academy' where those who think differently hold little power and are prevented from joining the community of science. These claims are reinforced by Smolin when he describes his prior experience of trying to publish an article in *The Chronicle of Higher Education* on "the threats to academic freedom coming from the dominance of popular research programs" (Smolin, 2006c, p. 345). The editors declined to publish the article as the claims were well-known and unoriginal. Smolin's strategy appears to be one of drawing upon allies so as to support the accuracy of his claims (as already 'well-known'). Smolin's decision to include this anecdote seems motivated by a desire to paint these problems as well-known and universal.

1.4 Woit's academy

Like Smolin, Woit also dedicates a chapter to discussion of sociological forces at work within institutionalised physics. Woit also covers much of the same ground in the chapter titled 'The Only Game in Town: The Power and The Glory of String Theory' (Woit, 2006e, pp. 224-239). Woit concentrates on the academic job market in the United States as that forms his personal experience

[45] Part one is titled 'The Unfinished Revolution', part two is titled 'A Brief History of String Theory' and part three is titled 'Beyond String Theory' (Smolin, 2006c).

and as most of the leaders of the string theory research program are located in the US. Noting how the academic job market changed after the 1970s, Woit claims that for the past 30 years job prospects in particle theory have been "grim" (Woit, 2006e, p. 233). Furthermore, the professionalization of the discipline prevents a modern day Einstein, or someone who works in his or her spare time, to contribute to particle physics unless he or she is independently wealthy (Woit, 2006e, p. 235). Woit describes the "well understood"[46] requirements of the obtaining an academic job: letters of recommendation from high profile physicists; publications in well-respected journals; and publications on the "latest and hottest topic" (Woit, 2006e, p. 236). Woit further details how those who are appointed to positions are forced to compete for scarce but necessary grant funding, and he quotes string theorist Mike Duff's description of competition in academia in the US: "the ethical standards are lower as a consequence" (Duff quoted in (Woit, 2006e, p. 239)).

Both Smolin and Woit set up toy models of the sociology of science that do not engage with the existing vast literature on institutional practices, dominance, popularisation and expertise. Smolin invokes existing sociology only in order to universalise his claims (with the story of his rejected *Times Higher Education* piece), whereas Woit makes no mention of existing sociology of science. Woit and Smolin can be read as not aiming to contribute to a sociological understanding of the practices of high energy physicists but instead to support rhetorically their normative view of science and critique of string theory. This particular interpretation is strengthened when we consider that neither author is setting the stage, as each has already presented his ultimate arguments. Instead, each author is attempting to further convince his readers of a vision of the way good and bad science progresses. Smolin, who argues that science should be organised so as to encourage diversity, assembles a model in which string theory will continue to drown out alternative perspectives and delay progress. Woit, who argues that the behaviour of string theorists has become pathological such that progress cannot occur, assembles a model in which dissenting voices will not be heard.

[46] Here, Woit invokes a similar strategy to Smolin's strategy to universalise his claims.

2. Dominance

As I have shown, Smolin and Woit take great pains to describe the system in which they claim that string theory has become dangerously institutionally dominant. They claim the danger here is in stagnating a field in which little or no progress will be made. These critiques set up an understanding of the dominance of string theory as socially-corrupted. In response to these arguments, supporters of string theory defend the dominance of string theory as evidence of the consensus of experts. I will examine the various arguments made by critics of string theory that attempt to undermine the legitimacy of string theory. Smolin and Woit, in different ways, assert that an unhealthy reliance on leaders within the field and arrogant and groupthink behaviour constitute the dominance of string theory (and the institutional rewards such as grants and faculty positions). By contrast, Polchinski, Duff and Johnson disagree with this constitution of the dominance of string theory and argue that the dominance of string theory should instead be understood as indicative of good scientific judgement and that a positive epistemic appraisal of string theory follows.

2.1 Unhealthy reliance on the vision of leaders

Both Smolin and Woit identify a number of other psychological and sociological factors that in their view have contributed to the dominance of string theory. The first is that string theorists must invest an enormous amount of time and intellectual effort mastering the subject before they can hope to make a worthwhile contribution. As Woit explains, "the huge degree of complexity at the heart of current research into superstring theory … means that a huge investment in time and effort is required to master the subject well enough to begin such research" (Woit, 2006, p. 205). In order to grasp superstring theory, young researchers must first master quantum field theory – which is itself a very demanding subject. Here Woit suggests that the immense intellectual investment required to enter the field makes it "psychologically and professionally very difficult to give up" (Woit, 2006, p. 206). Put simply, "the difficulty of superstring theory … makes it hard for researchers to leave" (Woit, 2006, p. 206).

The difficulties of mastering current work in string theory carry further important consequences. One of these is a perceived over-reliance on the judgement of leaders in the field. Both Woit and Smolin stress the enormous weight that Edward Witten's views carry within the physics community. As Woit explains, because of the immense difficulty and complexity of the theory involved, physicists:

> "often rely to an unusual extent not on their own understanding of the subject, but on what others say about it. The fact that Witten took up string theory with such enthusiasm in 1984 had a lot to do with it becoming so popular, and his continuing belief that it remains the most promising idea to work on has a huge influence" (Woit, 2006, pp. 205-206).

Critics argue that this has reached the level of hero-worship within the string community. As Smolin puts it, string theorists "typically want to know what senior people in the field, such as Edward

Witten[47] think before expressing their views" (Smolin 2008, p. 274). Some, like Magueijo, have argued that Witten's genius has made him something of a "guru" within the string theory community (Magueijo, 2011, p. 239). In Smolin's view, "the string community's huge regard for the views of a few individuals" has produced an "unusually monolithic community" (Smolin 2008, p. 284). Woit presents a similar view: "based on my experience, I'm pretty sure that if you sample non-string theorist physicists, you're going to find many people who would describe the behaviour of string theorists as "cult-like" (Woit 2006).

In Smolin's view, this unhealthy reliance on the professional judgement of leaders has led to an increasing "narrowness of the research agenda" (Smolin 2008, p. 284). Which problems are deemed worth working on at any given time is dictated to a large extent by trends driven by leaders in the field. Other physicists have offered similar accounts. Mikhail Shifman has argued that in the post-empiricist era of theoretical physics, novel ideas capture the attention of researchers, only to be abandoned just as quickly, meaning that "alternative lines of thought by and large dry out" (M. Shifman, 2012, p. 2).[48] Both Smolin and Woit see this trend as cause for deep concern. As Woit puts it: "Without any new experimental data to provide clues as to which direction to go in order to make further progress", research on string theory has become too dependent on the views of a few individuals, and consequently it has "stagnated and worked itself a long way into a blind alley" (Woit 2007, p. 258).

2.2 Arrogance

A *New York Times* editorial by Laurence Krauss, written on November 8 2005 (Krauss, November 8 2008), that criticised string theory through comparisons to intelligent design ignited a storm of controversy across several blogs. JoAnne Hewitt argued that the discussion was missing the perspective of a particle physicist and detailed her criticisms of string theory in a blog post on Cosmic Variance (Hewett, 2005). In particular she identified and criticised three expressions of arrogance within the string theory community:

> "Arrogance #1: I find the arrogance of some string theorists astounding, even by physicist's standards. Some truly believe that all non-stringy theorists are inferior scientists. It's all over their letters of recommendation for each other…

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[47] In 2005, J. E. Hirsch, a physicist at the University of California at San Diego, proposed a metric to "quantify the cumulative impact and relevance of an individual's scientific research output" (Hirsch, 2005, p. 16569). Via this metric Hirsch argued that Witten had the highest number of high impact papers of any living physicist (Hirsch, 2005, p. 16569). The metric, known as the h-index, is now well-established.

[48] According to Shifman: "In this mode each novel idea, once it appears, spreads in an explosive manner in the theoretical community, sucking into itself a majority of active theorists, especially young theorists. Naturally alternative lines of thought by and large dry out. Then before the idea brings fruit in understanding of phenomena occurring in nature (both, due to the lack of experimental data and due to the fact that on the theory side crucial difficult problems are left behind unsolved), a new novel idea arrives, the old one is abandoned, and a new majority jumps onto the new train. Note that I do not say here that this is good or bad. This is just a fact of life of the present day theoretical community" (M. Shifman, 2012, p. 2).

> Arrogance #2: I personally find the attitude that people actually think we know enough at this point to define the 'theory of everything' to be quite conceited. To me, it is equivalent to claiming that the universe rotates around the earth….
>
> Arrogance #3: String theory is so important that it must be practised at the expense of all other theory. There are two manifestations of this: string theorists have been hired into faculty positions at a disproportionally high level not necessarily commensurate with ability in all cases … these manifestations are worrying for the long-term future of our field." (Hewett, 2005)

Hewitt clarified that her criticisms were not to be read as an argument for the abandonment of string theory; instead, she was arguing for string theory "to be practised in reasonable proportion to other endeavors, sans the grandiose claims (which have yet to be realized)" (Hewett, 2005). There are obvious parallels between Hewitt's argument and Smolin's argument, who also argues for the advantages of pursuing a plurality of approaches. Indeed Smolin quotes Hewitt's blog post at length (Hewitt quoted in Smolin, 2006, p.268-7) in support of his claim that the string theory community is exhibiting groupthink behaviour (see next section).

Writing in response to Hewitt's blog post, Woit also dedicated a blog post in support of many of Hewitt's claims, adding: "it has certainly been my experience that [string theorists] display a degree of arrogance that is pretty astounding. This includes some of the earliest and most prominent string theory bloggers, where the phenomenon is pretty much off-scale" (Woit, 2005d). Woit argues that the position outlined by Hewitt is one that is "widely" held by in the physics community but rarely openly expressed. He also repeated these criticisms in his book, arguing that often critics of string theory are arrogantly dismissed prima facie as "ignorant and incompetent" (Woit, 2006, p. 227). In support of this claim, Woit quoted from an interview of string theorists from the Princeton Institute for Advanced Study, Edward Witten and Nathan Seiberg: "Most string theorists are very arrogant, says Seiberg with a smile. If there is something [beyond string theory], we will call it string theory" (Jha, 2005) quoted by Woit several times (Woit, 2005e, 2006e, 2011a).[49]

2.3 Groupthink

Smolin diagnoses the problem with the 'sociology of string theory' as occurring at the community level rather than at the individual level. He argues that in his experience string theorists as individuals are 'less dogmatic' than string theory collectives. Smolin claims that this behaviour may be best understood by comparison with the phenomenon of 'groupthink': "Sociologists have no problem recognising this phenomenon. It afflicts communities of highly credentialed experts, who by choice or

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[49] Woit took up what he perceived as arrogance in the string theory community in one of his very first blog posts: "The arrogance of people in the particle theory community never ceases to amaze me. Assuming that anyone who dares to criticize what is going on in the subject must be ignorant is all too common behavior" (Woit, 2004a).

circumstance communicate only among themselves … it is called *groupthink*" (Smolin, 2006b, p. 286) (italics author's own). Quoting from an online resource from Oregon State University, Smolin provides a diagnostic list for groupthink behaviour.[50] Following this, Smolin steps away from full blown accusations of groupthink behaviour, professing: "this does not match up one-to-one with my characterisation of the culture of string theory, but it's close enough to be worrying" (Smolin, 2006b, p. 287). Whilst Smolin does not specify the degree to which he believes the groupthink phenomenon has taken hold in the string theory community, he certainly commits in some measure to accusations of its occurrence and corrupting influence. In the final pages of the book, in a section titled 'What can we do for science', Smolin returns to the concept of groupthink: "What we are dealing with is a sociological phenomenon in the world of academic science. I do think the ethics of science have been to some degree corrupted by the kind of groupthink explored" (Smolin, 2006b, p. 350). Groupthink is set up as the foe against which Smolin is attempting to fight.[51]

Key to the critic's accusations of an unhealthy reliance upon the visions of leaders and arrogant and groupthink behaviour is the idea that this behaviour has taken on the function of perpetuating the dominance of the string theory program. Typically when dominance is invoked in a normative way in debates between or over scientific research programs, it is done is such a way as to claim that dominance is evidence of the value of a particular approach. Classic examples are climate change and evolutionary biology. If the dominance of string theory can be reduced to arrogance, critics are able to undercut a normative claim as to the value of string theory. Here critics recast dominance as being constituted by 'unhealthy' behaviours like arrogance and groupthink, resulting in the acquisition of power. A particular consequence of this, also taken as further evidence that the dominance of string theory is self-perpetuating, is the monopolisation of resources and the job market.

2.4 Monopolisation of resources and the job market

For Smolin, the "issue is not whether string theory is worth doing or should be supported"; instead, he argues that the most important question to be levelled is "why string theory, in spite of a dearth of experimental predictions, has monopolized the resources available to advance fundamental physics, thus choking off investigation of equally promising alternatives" (Smolin, 2006, p. 267). The answer

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[50] "Groupthink members see themselves as part of an in-group working against an out group opposed to their goals. You can tell if a group suffers from groupthink if it:

1. overestimates its invulnerability or high moral stance,
2. collectively rationalizes the decisions it makes,
3. demonizes or stereotypes outgroups and their leaders,
4. has a culture of uniformity where individuals censor themselves and others so that the facade of group unanimity is maintained, and
5. contains members who take it upon themselves to protect the group leader by keeping information, theirs or other group members', from the leader" ("Group Think,") quoted by (Smolin, 2006a, p. 287).

[51] Chad Orzel has also explored similar content in a blog post critiquing string theory through sarcasm in which Orzel attempts a humorous characterisation of string theory as cult-like: "Let go of your petty objections, drink this Kool-Aid, and revel in the eleven-dimensional glory of what is undoubtedly the greatest creation in the history of human culture" (Orzel, 2007).

to his question, Smolin contends, is that the institutional dominance of string theory has allowed the string theory community to do so: “there is no doubt that this system has benefitted string theory and made it more difficult for people who pursue alternative research programs” (Smolin, 2006, p. 338). This position was supported by the author of the *Angry Physics Blog* who wrote a post in support of Smolin in which the author claimed:

> “The string theorists set the entire stage of deciding what and who is important. The program officers aren’t off in an ivory tower deciding what to fund. They’re listening to their respective community members and when the dominant voice is strings, then guess what happens?” (Anonymous, 2006a)[52]

Smolin sees these practices and mindsets as detrimental to the field. Smolin argues that this is harmful to physics, “because it chokes off the investigation of alternative directions, some of them very promising” (Smolin 2008, p. xxii). It is difficult to believe that Smolin is not referring to his own research program, loop quantum gravity, here as a ‘promising alternate direction’.

Smolin defends the right of the individual researcher “to pursue the research they think is the most promising”, but argues that string theory has acquired too much institutional power and this is reflected in two places: in the limited career options for aspiring theoretical physicists; and in the tenured positions offered. In an atmosphere of intense competition for research positions, those who seek to join the field of theoretical physics are only presented with one professionally realistic option if they want to pursue research on a unified theory – “string theory now has such a dominant position in the academy that it is practically career suicide for young theoretical physicists not to join the field” (Smolin, 2006, p. xx). Furthermore:

> “Some young string theorists have told me that they feel constrained to work on string theory whether or not they believe in it, because it is perceived as the ticket to a professorship at a university. And they are right: In the last fifteen years, theorists who pursue approaches to fundamental physics other than string theory have almost no career opportunities … Even as string theory struggles on the scientific side, it has triumphed within the academy.” (Smolin, 2006, p. xxii)

Weaving together anecdotal evidence and broad claims, Smolin argues that the dominance of string theory is self-perpetuating.

Woit also claims that the academy has played a role in the dominance of string theory: “there are other reasons why there is only one game in town, but the social and financial structures within which

[52] Written as a response to Carroll’s review of TTWP. The Angry Physics blog: “Presenting the “other” side of academic physics, where people backstab and give lousy talks. Where people are sometimes lazy or incompetent, and the best don’t get the credit or the job. From the perspective of someone lucky enough to have landed a tenure-track professorship” (Anonymous, 2006a).

people are working are an important part of this situation" (Woit, 2006, p. 239). Woit argues that "the most common justification I have heard is some version of 'Look, it's the only game in town. Until someone comes up with something more promising, this is where the action is'" has been made since 1984 (Woit, 2006, p. 224), and that part of the reason why a competitive alternative has not been able to develop is the self-perpetuating dominance of the string theory research program within the academy (Woit, 2006, p. 233). In particular, Woit is critical of the negative reception his blog has received: "more recently, I have found the continuing dominance of superstring theory in particle physics taking on an increasingly disturbing aspect" (Woit, 2006, p.227). Woit claims that he has been subject to death threats from Harvard string theorist from Lubos Motl[53] (Woit, 2006, p. 227). He also levels accusations of censorship, achieved through the peer review process, and gives a detailed account of attempting to publish NEW through a Cambridge or an alternative university press. Woit takes pains to note that the peer review that came from sting theorists did not contain "any scientific argument"; instead, that the review was ideologically biased (Woit, 2006, p. 229).

To argue that dominance results from the institutions of the academy is almost trivial, and certainly no surprise to the sociologist of science. However the force of the argument, delivered by the toy sociological models that the critics employ, is one that seeks to understand the dominance of string theory as resultant from certain ideologically corrupt practices. It is unusual in disputes over scientific credibility, where we see an appeal to a similar concept, consensus, to point to the dominance of a research program in a negative appraisal of a research program. This is explored further in chapter two. The following sections will explore how string theorists were forced to defend both themselves and the dominance of string theory against accusations of pathological science. The uniqueness of these debates provides insight into ways in which protagonists constitute norms of inquiry and the slipperiness of the categories concerned.

2.5 Motivations

One of the responses to Smolin and Woit's critiques was to question the motivations of the authors. Smolin and Woit were presented as frustrated failed academics who, instead of positively contributing to the field, had taken the unethical approach of detailing criticism in order to raise their own profiles or to advance their own careers. In a combined review of Smolin and Woit's book, string theorist Leonard Susskind asked: "are the critics a bunch of disgruntled conspiracy theorists, angry at being ignored? And might there be a bit of opportunism at work, an opportunity for gaining 15 minutes of scientific fame – without the real work?" (Susskind, 2006). In particular, those who supported string theory, focused on the motivations of Smolin. They accused him of concealing his true motivation: an attempt to gain more funding for loop quantum gravity. Clifford Johnson commented on his blog: "shall we call it what it is, the 'I want more money for my approach', bit from Lee (my words not

[53] Motl has also claimed he received anonymous death threats (Motl, 2005a).

his)” (C. Johnson, 2006c). By suggesting that ulterior motives are at play, the critics attempt to challenge the accuracy of the descriptions and weight of the judgements offered by Smolin and Woit.

2.6 The ‘scientific judgement’ response

As Polchinski puts it, “much of what Smolin and Woit attribute to sociology is really a difference of scientific judgment” (Polchinski, 2006). The reason that theoretical physicists have worked on string theory is that it has made genuine progress in solving many outstanding theoretical problems, and represents by far the most promising – indeed for many physicists, the *only* viable – approach to realising the goal of a unified theory of quantum gravity. Rather than contest the assertion of the dominance of string theory and the monopolisation of resources, the ‘scientific judgement’ response seeks to reconstitute how dominance should be understood in both the epistemic appraisal of string theory and the projective appraisal of the future prospects or promise of string theory.

In response to Smolin’s characterisation of the string theory community as “unusually monolithic”, Polchinski argues:

> “Overwhelmingly the concentration on string theory is a scientific judgment, made by a very diverse group of theorists. Look at any of the several dozen most well-known string theorists: my own scientific experiences and tastes, both inside and outside string theory, are very different from any of theirs, just as they are from each other... String theorists can be rather focused, but they are not as closed to new ideas as you portray. For example, such ideas as holography and eternal inflation were developed outside of string theory, and might have become ‘alternative ideas.’ Instead they were recognized as likely parts of the big picture.” (Polchinski, 2007b)

Here Polchinski offers both a sociological description of the string theory community and a normative account of science. In addressing the description of the string theory community, Polchinski draws attention to the diversity of approaches within string theory, as well as what he argues to be the fruitful interconnections that have emerged in recent years between string theory and other areas of research in contemporary physics, such as inflationary cosmology. The emergence of such interconnections and the openness to new ideas explains why string theory occupies the prominent place that it currently does in fundamental physics. This descriptive account stands in sharp contrast to Smolin’s view of the string theory community as “monolithic”, but Polchinski’s characterisation of the ethos of science is in some respects similar to Smolin’s. Here Polchinski demands that “scientists take responsibility for what they say” (Polchinski, 2007b). Scientists have a responsibility to present their ideas as clearly and precisely as possible, and to engage in a process of transformative criticism. When counterarguments are presented, they must be responded to, “and the original assertion modified if necessary”. In view of this, Polchinski sees it as “ironic” that Smolin attempts to take the moral high ground (Polchinski, 2007b).

In a similar vein, Duff has argued that the dominant nature of string theory is the result of the majority of the community judging it to be the most promising approach: "most of the bright young people, as Smolin concedes, are voting with their feet and opting for string theory" (Smolin in (Smolin et al., 2007)). Likewise, Sean Carroll has maintained that string theory has become the dominant paradigm of theoretical physics "for intellectual reasons, not socio-psycho-political ones" (Carroll, 2006d). Indeed, one should defer to the judgment of "trained experts who think that this is the best way to go, based on the results they have seen thus far" (Carroll, 2006d). Like Duff, Carroll characterises the dominance of string theory community as a consequence of critical consensus:

> "The reason why string theory is so popular in physics departments is because, in the considered judgment of a large number of smart people, it is the most promising route to quantizing gravity and moving physics beyond the Standard Model ..." (Carroll, 2006d)[54]

Rather than direct their attacks at the scientific community at large, defenders of string theory cast the dominance of string theory as a critical consensus that has emerged in the quantum gravity community. The dominance of string theory is reconstituted as a positive epistemic judgement.

2.7 The 'no alternatives' response

There is also evidence that supporters of string theory argue the 'no alternatives response' in an attempt to reconstitute the dominance of string theory as a positive epistemic judgement. Here defenders of string theory shift the onus back onto their critics. "The most effective way for critics of M-theory to win their case", Duff contends, "would be to come up with a better alternative. So far nobody has" (Duff, 2011b, p. viii). Here the claim is clear: 'put up or shut up'. In a similar manner, Carroll argues: "The way to garner support for alternative approaches is not to complain about the dominance of string theory; it's to make the substantive case that some specific alternative is more promising" (Carroll, 2006d). Clifford Johnson extended this argument to claim that Smolin and Woit have misunderstood how science progresses in a criticism reminiscent of Lakatos' criticism of Popper's Falsificationism. Clifford argued that:

> "If you want people to work on other ideas, please present those other ideas and convince your peers about the merits of those ideas, what promise you see for them, and people will work on them if they agree with you. This is the way science proceeds. People work on the best and most promising things they see. They don't just stop working on a huge body of ideas, clear their desks, and then sit there hoping new ones will come to fill the void. It does not work that way." (C. Johnson, 2006d)

[54] Carroll repeated this claim in response to the critique of string theory the following year in an op-ed piece for *New Scientist*: "so is the jig up? Is string theory in its last throes? No, not at all. At least, not if we measure the health of the field by more strictly academic criteria. String theorists are still being hired by universities in substantial numbers; new graduate students are still flocking to string theory to do their Ph.D. work" (Reprint accessed through Carroll's blog at (Carroll, 2007)).

Johnson argues that Smolin and Woit have failed to understand how scientists behave in the situation where there is no an alternative promising approach.[55]

This defence attempts to condemn Smolin and Woit's critique in different ways. Woit argues that the string theory research program has become too dominant however he does not advocate for an alternative approach. Johnson argues that Woit's position is ill-founded as scientists do not, and should not, abandon a theory without a promising alternative. Johnson's position is considerably more critical of Smolin, given that Smolin advocates for a rival position. Here we see an argument similar to that discussed in the previous section that focuses on the notion of 'promise'. In Johnson's view promise it is a matter of scientific judgement and the fact that "people" are not working on loop quantum gravity is an indication it is not "the best and most promising things they see" (Johnson, 2006d).[56]

The dispute over how the dominance of string theory can be understood as foregrounding a central issue at stake in the debates over string theory: that issue is what factors should be taken in to consideration in the appraisal of a research program, especially where the appraisal concerns the potential of a research program? This issue, of the potential of a research program to progress, is key to arguments offered by Johnson and Polchinski in which they argue that to characterise the dominance of string theory as resultant from pathological science is to misunderstand theory appraisal. Instead, they argue, the dominance should be taken as evidence that string theory is the most promising approach according to scientific judgement. In this case there are two elements to this issue: how should we appraise of the current state of a research program; and how we evaluate its potential to solve problems in the future.

[55] In a reply to the reply (to the reply) Smolin pointed to the response his book has received. Smolin took the backlash against himself and Woit as further evidence for groupthink behaviour and pathological science. In particular, Smolin argued that the resistance to criticism indicated that the string theory community's defence was ideological and deviated from the norms of "openness to criticism from experts and its welcoming of a diversity of approaches" (Smolin, 2007). Polchinski's response to the reply to the reply was to yet again recast the debate such that the criticisms of string theory were ideological (Polchinski, 2007b).

[56] Loop quantum gravity theorists, like Rovelli and Smolin, argue that they *have* made progress in developing a fully background independent formulation of quantum gravity, which has thus far eluded string theory (Rovelli 2003). String theorists, on the other hand, maintain that it is far from obvious that 'background independence', as it is defined by loop quantum gravity, is an essential prerequisite of the theory. Here we may characterise the situation in the terms set out by Larry Laudan. Different judgements about the progress of string theory rest on "divergent views about the attributes our theories should possess" (Laudan, 1984, p. 42). For an examination of the debate over what constitutes a 'background independent' theory, and path theory construction should take see chapter five, section 1.2.

3. Expertise

3.1 Contested domains of science: disputes over popularisation

Just as Smolin and Woit each employed a notion of the academy, they also employed a notion of ‘the public’ as an important domain, populated by non-expert discriminators responsible for funding decisions. As is considered below, certain supporters of string theory have responded by accusing their critics of misinformed and damaging distortions that would gain no traction in scientific publications. In doing so, they too have employed a notion of a ‘public domain’ as distinct from a ‘scientific domain’. At the core of this dispute is a conflict over differing co-constitutions of ‘public’ and ‘expert’.

3.1.1 Creating scientific facts

Smolin has been especially critical of many recent ‘popularisations’ of string theory, in which the authors have tended to overstate claims that string theory has definitively solved a range of crucial problems such as quantum gravity, black hole entropy, moduli-stabilisation, and background independence by presenting a misleading image of string theory as triumphantly marching towards a ‘theory of everything’. The problem, Smolin argues, is that:

> “Many presentations of string theory, for the public as well as for colleagues, seem to have been misleading on this issue. Many people I spoke to were under the impression that perturbative finiteness was an established fact. Most review papers for physicists and popular books gave the impression that perturbative finiteness of string theory is a fact. Only a few characterize the situation correctly.” Smolin commenting on (C. Johnson, 2006f)

Woit makes a similar complaint in several blog posts written since 2007 grouped together under the category titled ‘This week’s hype’, where he provides links to various popular pieces that he argues engage in hubris about string theory without making important qualifications (Woit, 2007 - 2015). Jim Baggott, a self-described physicist-turned-writer, detailed a of variety criticisms of string theory and string theorists in his book *Farewell to Reality: How Fairytale Physics Betrays the Search for Scientific Truth*[57] (Baggott, 2013). Following the publication of the book, he had a short debate with string theorist Mike Duff in which Baggott accused string theorists of misrepresentations in ‘popular literature’:

> “A string of recent bestselling popular science books, supported by press articles, radio and television documentaries, have helped to create the impression that this is all accepted scientific fact. Physics has gone too far” Baggott in (Baggott & Duff, 2013).

[57] Criticisms of string theory and theorists are concentrated in the chapters ‘In the cemetery of disappointed hopes: Superstrings, M-theory and the search for the theory of everything’ and ‘Gardeners of the Cosmic Landscape: Many worlds and the multiverse’ (Baggott, 2013).

Smolin and Woit see it as their obligation as scientists to 'set the record straight' in countering what they see as misleading and exaggerated claims made by string theorists.

3.1.2 Not acknowledging failure

Woit sees the refusal of the theoretical physics community to acknowledge the failures of string theory as perhaps the most disturbing trend in recent years. Woit argues that string theory has failed to deliver on its original promise of unifying a quantum theory of gravitation with elementary particle physics: "As years go by and it becomes increasingly clear that superstring theory has failed as a viable idea about unification, the refusal to acknowledge this begins to take on ever more worrying connotations" (Woit 2007, p. 216). Similarly Smolin argues:

> "We must be careful to present the failures along with the successes. … In recent years, many books and magazine articles for the general public have described the amazing new ideas that theoretical physicists have been working on. Some of these chronicles have been less than careful about explaining just how far the new ideas are from both experimental test and mathematical proof." (Smolin, 2006c, p. xxi)

Another critic of string theory, Dan Friedan, has stressed the importance of recognising failure as an integral "part of the scientific strategy". Scientists, according to Friedan, have "a responsibility to recognize failure" (Friedan, 2003, p. 8). This offers yet another characterisation of the values that underpin science. Friedan argues that the refusal to recognise failure is detrimental to scientific progress.

Friedan's view can be contrasted with that articulated at the 'Strings 2003' conference by David Gross, who closed his lecture by quoting Winton Churchill. Gross appealed to his fellow string theorists to "never, never, never, never, never give up" on searching for uniqueness[58] (David Gross, 2003a). Gross identified persistence, not a readiness to acknowledge failure, as the virtue most befitting the theoretical physicist.

3.1.3 Public as important, non-expert, decision makers (arbiters)

Honest 'popular' or 'public' communication is argued by Smolin, Woit, Friedan and Baggott to be important because of the role they argue the public plays in determining funding allocation. Smolin reminds the reader that "we physicists require significant resources, which are provided largely by our fellow citizens" and that, to this extent "physicists, who communicate with the public, whether through writing, public speaking, television or the internet, have a responsibility to tell the story straight" (Smolin, 2006c, p. xxi). Smolin makes explicit that he considers TTWP to be popular literature or a 'public' book:

[58] For more on the debates over uniqueness see section 2.1 of chapter five.

> "It may seem strange to be discussing academic politics in a book for the general public, but you, the public, individually and collectively, are our patrons. If the science you pay for is not getting done, it is up to you to hold our feet to the fire and make us do our job." (Smolin, 2006c, p. 353)

If I temporally adopt my actor's category and assume that string theory is dominant, Smolin here may be characterised as a 'dissenter'. As Delborne has noted, if dissent is examined as practice, rather than a position, that practice of dissidence is sometimes to argue that the public may be a correcting participant in disputes over the norms of science (Delborne, 2008). Here we see how Smolin co-constitutes the notions of 'public' and 'expertise'. The public is understood to be comprised of important decision makers who have some measure of control over what physics should be funded. However they are unable to determine honest physics from dishonest physics, lacking the expertise to discriminate between these two categories (as constructed here). Expertise is negatively constituted by this argument.

3.1.4 The 'you are distorting the facts' response

In an exchange with Smolin following the publication of TTWP in 2007, Polchinski engaged in a sustained critique of what he saw as Smolin's deeply flawed account of the developments in theoretical physics over the past two decades. Many problems that Smolin had claimed were ignored or remained unsolved, such as the moduli-stabilisation problem, were, in fact, successfully solved once the appropriate tools became available. In his reply to Smolin, Polchinski writes that this "is an example of something that that happens all too often in your book: you have a story that you believe, or want to believe, and you ignore the facts… You are portraying a crisis where there is actually a major success, and you are creating an ethical issue where there is none" (Polchinski, 2007b).

In the end, Polchinski acknowledges that "sociological effects exist; they must, since science is a human activity", but he finds little evidence to support Smolin's claim that the behaviour of string theorists has ultimately been harmful to the progress of physics. "To make the case for a strong sociological effect, at each turn you are forced to stretch the facts beyond recognition" (Polchinski, 2007b).

Not only does Polchinski argue that Smolin's argument is fatally flawed, he also argues that instead it is Smolin who has violated the norms of the scientific community. Arguing that successful science does indeed rely upon on a commitment to a shared scientific ethic, Polchinski argues that "the principal scientific ethic is that scientists take responsibility for what they say" (Polchinski, 2007b). For Polchinski the norms that define the scientific ethos are also honesty and responsibility:

> "Coming back to ethics, When [sic] a statement is made, to what extent has it been thought through, and appropriate checks and counterarguments considered (and, yes, the appropriate calculations done)? To what extent are known difficulties acknowledged? When a new

counterargument is given, is it addressed, and the original assertion modified if necessary? Are facts presented in a clear and direct manner? This is howscientists [sic] judge one another. It is clear why this is necessary: science works by the parallel activity of many minds, and it is necessary that information be exchanged in as accurate a way as possible. Given the above discussion, I find [Smolin's] claim to the ethical high ground to be ironic." (Polchinski, 2007b)

Here it is clear that, for Polchinski, it is Smolin, not string theory community, who is guilty of an ethical failing.

In this context, it is instructive to see how Susskind attempts to turn the tables on critics like Smolin and Woit:

> "What in the multiverse is going on? Could it really be that a secret cabal of scientific priests have plotted to overthrow the rules of good scientific method and have absconded with the nation's scientific funding? Have America's greatest universities – Harvard, Princeton, Stanford, Berkeley, Massachusetts Institute of Technology, California Institute of Technology - all become infected with the same cancer of meaningless metaphysical speculation? Has serious science been driven out by string theorists bent on world domination? Or are the critics a bunch of disgruntled conspiracy theorists, angry at being ignored? And might there be a bit of opportunism at work, an opportunity for gaining 15 minutes of scientific fame - without the real work?" (Susskind, 2006)

Susskind here offers a different assessment and, in order to do so, makes an appeal to the credibility and reputation of America's leading research institutions to defending the legitimacy of string theory. Critics are considered outsiders and are here compared to "disgruntled", "angry" "conspiracy theorists". Susskind here implies that there are ulterior motives at play, thus rendering the critics' judgements suspect. Geoff Brumfiel has similarly claimed that the recent criticisms of string theory are "written by outsiders" and "have stirred deep resentment in the tight knit community" (Brumfiel, 2006, p. 491). In his review of the both Smolin and Woit's books Brumfiel, as is typical, portrays Smolin as an outsider. Brumfiel also mentions in the same review that Smolin has published within string theory. From this we can see how the strategy of 'othering' is employed. Here the debate becomes a debate over credibility.

Polchinski attempts to attack the credibility of both the claims made by Smolin and Woit, whereas Susskind personally attacks the credibility of Smolin and Woit:

> "Basically, I believe Woit is simply excessively negative: in any human endeavor one could assume the worst at every turn, but this is not what moves the world forward. Smolin seriously distorts both the science and history, manufactures ethical issues where they do not

> exist, and miscasts questions of science as issues of sociology. Some of his points bear thought, but as presented they are wrapped in demagoguery and distortion." (Polchinski, 2007c)[59]

The position held by Polchinski may be characterised as follows: the debate about the merits and legitimacy of string theory as a science is not primarily a sociological or philosophical one, as some critics would have us believe, but rather it is essentially a *scientific* debate and as such it is only those with scientific expertise who are qualified to make a scientific appraisal of string theory. That is, there is a class of experts with the epistemic authority to comment. This practice of 'othering' is very common to the string wars (and to scientific controversies in general). In attacking the credibility of Smolin and Woit, it is Smolin and Woit who become boundary objects. The notion of contested boundaries of expertise will be explored in the final section of this chapter. The next section will examine the ways in which the boundary between domains of scientific discourse is contested, examining the controversy over public/popular discourse.

3.1.5 The 'you have used an illegitimate site, the public arena, to criticise' response

The strategy of 'othering' is further employed where a number of defenders of string theory have responded to the critique outlined by Smolin and Woit by labelling it as a popularisation. Just as Smolin construed the public as non-expert so too did Johnson when he argued:

> "This is why I do not agree with the practice of writing books presenting a distorted picture of a field of an entire field of research, the sole purpose being to devalue and undermine the work of very many people, and represent them as marching in lock step to some religious or sociological ideology. All presented to a readership who is not in a position to determine whether this is true or not. That is not the way to proceed." (Johnson commenting (C. Johnson, 2006f))

On this approach, Johnson argued that the public arena is an illegitimate site to criticise string theory. Similarly Woit's criticisms of string theory have been dismissed by some because his primary medium is a blog, and that makes those criticisms science journalism rather than science 'proper' (Woit, 2006c).[60] During a debate with Smolin and Nancy Cartwright, Duff quite explicitly separated the notions of public and science, claiming that: "Sadly many critics of string theory, having lost their case in the court of Science are now trying to win it in the court of Popular Opinion" (Smolin et al., 2007). Use of the internet-based media, and in particular the use of blogs, is argued by Duff to be populated by non-experts: "The internet, where everyone is an expert, now provides their ideal forum.

[59] According to Carroll: "Smolin stacks the deck by contrasting the "craftsmen" who toil within string theory to the "seers" who pursue alternatives, and it's pretty obvious which is the more romantic role. Many physicists are more likely to see the distinction as one between "doers" and "dreamers," or even (among our less politic colleagues) between "scientists" and "crackpots" (Carroll, 2006e).

[60] See chapter four for a detailed examination of some of issues surrounding blogs.

Attempts at sensible commentary or discussion on the blogosphere are usually quickly overwhelmed by a cacophony (the collective noun?) of crackpots" (Duff, 2013, p. 192). Here string theorists attempt to construe the debate on their own terms such that online or blog based critics and criticisms of string theory are deemed 'popular' and thus part of the public, non-expert domain. For Johnson, misinformed criticism in the "popular domain" is "not the method by which physics gets done faster. If anything, it has the opposite effect" (C. Johnson, 2006e). Just as Smolin argued, misinformed positions directed at non-experts have the consequence of slowing progress.

Duff's position was not limited to online contributions: he also sought to delegitimise each of the books written by Smolin and Woit as non-expert popularisations:

> "The battle for the correct theory will not be won on the bookshelves of Barnes and Noble, nor in the debating chamber of the Royal Society for the Arts. The batter will be won in the pages of scholarly scientific journals or, in their modern guise, on the electronic archives on the internet (http://arxiv.org/). The way to persuade your scientific colleagues that you have a good theory that is worthy of support is to publish your findings and make the most convincing case that you can."[61] (Duff, 2013, 188)

Duff and Johnson seek to delegitimise the position Smolin and Woit strike by placing it outside of science. Johnson further pursued this strategy, explicitly labelling Woit a non-expert, coupled with accusations of "deliberately distorting the public's view" (C. Johnson, 2006d):

> "You [Woit] take something that you are not an expert in, and take signs that there is something yet to be shown or understood with more research and turn that into a proof that string theory is wrong, doomed, a sham, has failed, etc, etc. It is just a dishonest and downright underhand practice… and you use it to successfully manipulate journalists, non-experts, and a general audience." (C. Johnson, 2006f)

Witten too briefly joined in the debate to caution against what he saw were potential negative outcomes of Smolin and Woit's critique: "they bring into the public arena technical claims that few can properly evaluate. They are sometimes able to generate astonishing amounts of publicity" (Witten, 2006). Here we see the ways in which the categories of 'public' and 'expert' are co-constituted as in opposition to each other: the public domain is populated by non-experts outsiders.

3.2 Contested domains of science: disputes over organisation

In order to deny the legitimacy of the critique of string theory, several of those active in the debates over string theory presented arguments as to who has expertise to judge the success/failure of string

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[61] Several years later, Duff reiterated this claim in a debate generated by fresh criticisms of string theory: "The battle for the correct theory will not be won on Amazon or on the blogosphere, however. It will be won in the pages of scholarly scientific journals. Sadly, many critics of string theory, having lost their case in the court of science, try to win it in the court of popular opinion" (Duff in (Baggott & Duff, 2013).

theory. Just as the preceding section examined how the expertise was assembled negatively, i.e. the characteristics of the non-expert, this section will examine how expertise is characterised positively. This section will focus on two exchanges between Woit and Johnson and Smolin and Johnson debating specialised experience against wider, not singularly specialised experience. Woit and Johnson are united in their argument that it is the string theory community that is able to appraise the condition of string theory, however, unsurprisingly, they are divided as to their appraisal of the prospects for string theory. The position advocated by Johnson bears similarities to Collins' recent construal of 'core-sets'. Johnson continues to defend this position against Smolin's contention that it is wider, not singularly specialised, experience that is necessary to judge the success/failure of string theory (i.e. that it is necessary to include voices from outside the string theory community).

3.2.1 Homogenous experience is necessary (but probably not sufficient) in order to judge success

The exchange between Woit and Johnson began with a claim from Woit regarding citation practices: "the reason that string theory paper like the one you mention[62] are not getting cited is that they're not that significant. The collapse is not in the "citation market", but in the intellectual health of string theory" (Woit commenting on (C. Johnson, 2006f). Woit's negative appraisal of string theory appeals to the authority of the members of the string theory community. A sarcastic comment ("As measured by you – An active and highly informed member of the string community, right?") from Johnson (C. Johnson, 2006f) prompted further explication of his position: "I think this is the most objective measure available of string theorist's evaluation of the significance of each other's work … What I'm quoting to you here is not my judgment, but that of the community of string theorists" (Woit commenting on (C. Johnson, 2006f).

Johnson responded by questioning Woit's ability to interpret the significance of citation practices.[63] Johnson's argument also appeals to the authority of those within the string theory community:

> "Even as someone who has been active in string theory for ~~14~~ 16 years, I would not have the gall to claim that the papers currently being written are not significant ... It is very well known that the significance of a huge number of papers is not often not recognized until very many years later. Sometimes decades … Why would you -especially as someone who is outside the field- feel able to say such a bizarrely unsubstantiated sweeping statement? It would be like me claiming that about the analogous papers in theoretical condensed matter physics!" (C. Johnson, 2006f)

[62] This refers to an earlier comment from 'Holmes'. The paper in question is 'New Instanton Effects in String Theory' (Beasley & Witten, 2006).

[63] Johnson was not alone in his critique of Woit. One such example is found in an anonymous commentator who claimed: "you [Woit] are not an expert on string theory. Despite your academic credentials giving you authority in your own field it would be wrong to assume that this authority is transferred to another field such as string theory" ('Charles' commenting on (C. Johnson, 2006f)).

Absent from this argument is a positive appraisal of string theory that appeals to Johnson's 16 years of experience in string theory. Johnson argues that 'insider' experience is necessary, although not sufficient (his 16 years of experience do not qualify him) to conclusively interpret citation practices within the string theory community. There are two layers here: Woit and Johnson agree that the judgement of those internal to the string theory community is significant to appraisals of string theory. The second layer, and where the disagreement lies, is that Woit contends that the judgement of a community is evident from citation practices, whereas Johnson questions Woit's authority, as an outsider to the community, to evaluate complex practices within the community.

Johnson's position is similar to Collins' and Collins and Evan's recent characterisation of the relationship between expertise and 'core-sets'[64] (Collins, 2014a, 2014b; Collins & Evans, 2002, 2007; Collins, Weinel, & Evans, 2010). Collins and Evans define a core-set as "being made up of those scientists deeply involved in experimentation or theorization" and correspondingly define those scientists as "core-scientists" (Collins & Evans, 2002, p. 242). Collins' position rests on a distinction between the resources that are available to core scientists and non-core scientists. Collins argues that experts have access to "specialist oral culture" and that both experts and non-experts have access to "written sources" (Collins, 2014b, p. 724). The interaction between members of a core-set gives them the expertise to interpret practices of members (and to interpret the behaviours of those that aspire to be members of their core-set) of a particular scientific community. This expertise, argues Collins, cannot be obtained from "Primary Source Knowledge" that is reading the scientific literature of an area of inquiry and as such is only available to core scientists (Collins, 2014b, p. 725). Both Johnson and Collin invoke a concept of 'insiders' and 'outsiders'. While Johnson does not give us specifics as to membership characteristics of insiders, both he and Collins normatively argue that it is experts/insiders who should interpret scientific practices (C. Johnson, 2006f) (Collins, 2014a, p. 132) (Collins, 2014b, p. 724). For Collins, this is because peer review alone is not sufficient to determine the significance of the content of a scientific paper. For Johnson, this is because the number of citation a paper receives is not sufficient to determine the significance. For both, what is important is how insiders interact with the content of a paper over time. Johnson implicitly and Collins explicitly invoke a 'relational theory of expertise' (Collins, 2014a).

For Collins, the core-set is formed when some kind of consensus occurs and it continues to evolve and its borders renegotiated. One difficulty raised by the string wars with the normative argument offered by Collins, that outsiders (such a policy makers) should trust the core-set, is that the constitution of expertise is a point of contention and is playing a role in consensus formation. It is not clear *a priori* which understanding, or combination of understandings, will become part of the consensus. It is not even clear if a consensus will form. It would also be possible to construe Woit's position as consistent with Collins' position. Woit does not think that publication is sufficient to determine success of the

[64] For the earlier work on core-sets, see (Collins, 1985, 1999).

content of a paper. He does not even think that a citation is sufficient. He argues for a threshold of citation level which could be interpreted as significant (Woit commenting on (C. Johnson, 2006f)).

Johnson's characterisation of Woit as an outsider is also consistent with Collins' position that members of the core-set, or those who have expertise, are characterised by less certainty than those inside who have a more nuanced view:

> "What it means to be a 'specialist' is to be in there … twenty four hours a day. To be a non-specialist is not to be in there, If you are outside, things become inevitably simplified. The 'bandwidth' of the channel to the outside is too narrow to carry all the nuanced information about what is happening inside the core-set and it would be a full-time occupation to absorb it: to swim in the water you would have to become a professional or semi-professional yourself … What is nuanced and unclear to those inside the core-set becomes, paradoxically sharp and clear to those outside it." (Collins, 2014a, p. 85)

Compared with Johnson's appraisal of Woit's position against himself:

> "Peter,
>
> Let's just set this out clearly:
>
> (1) Your position is that you -having never worked in the field- know absolutely for sure, before the research is done (even though the research program is remarkably healthy…. despite your awfully shaky and naïve argument about citations to the contrary) and so research should stop.
>
> (2) My position is that I do not know one way or the other (and I actually *work* in the field)… I'd dearly love to know the answer ... I'm letting people get on with their research". (Johnson commenting on (C. Johnson, 2006f))

According to Johnson, Woit does not have the expertise to understand the nuances of interpreting citation levels; instead, he has reached an overly simple conclusion. As was commented on earlier, Johnson does not provide a ringing endorsement of string theory research; instead, he argues that judging the success or failure of string theory is a complex process and that, for those with expertise in the field, it is too soon to pronounce simplistic judgements.

However, this assertion that it is 'too soon to pronounce judgement' is also disputed by some critics of string theory who argue that one of the many resources that string theory has monopolised is time. This issue is also deeply tied to assessments of progress in string theory, where some question whether sufficient progress has been made to justify continuing work on string theory. Woit has been particularly strong on this issue, where he contradicted Duff's argument that more time is needed: "One can argue that string theorists just need more time (Duff points to the idea of atoms arising back in 400BC, taking more than two millennia to come to fruition), but the problem with string theory is not that progress is slow but that it is negative" (Woit, 2013a). The question of the amount of time that should be devoted to a research project is one that faces policy makers and those who fund scientific endeavours. This raises a further difficulty for Collins: is it not clear for how long funding bodies should 'trust' in the 'core-set'.

3.2.2 Heterogeneous experience is necessary to judge success

In the introduction to TTWP, Smolin outlines how he believes an expert community should be organised. Although not, in this instance, explicitly articulated as an opposing position to those outlined above (his position predates the exchange between Woit and Johnson[65] and his exchange with Johnson that followed), it is instrumental to examine his initial articulation of norms of an expert community:

> "Science requires a delicate balance between conformity and variety. Because it's so easy to fool ourselves, because the answers are unknown, experts, no matter how well trained or smart, will disagree about which approach is most likely to yield fruit. Therefore if science is to move forward, the scientific community must support a variety of approaches to any one problem." (Smolin, 2006c, p. xxii)

Instead of relying on the judgement of those within an avenue of inquiry, the position that Woit and Johnson support, Smolin advocates for the value in an assemblage of diverse experience within a scientific community.

Smolin's position evolved in response to the criticisms of *TTWP* that questioned Smolin's authority to speak to string theory. Smolin took to Johnson's blog to defend his credibility and authority:

> "Part of the problem, in my view, is your assertion that I am trespassing into a domain that is not mine (â€oea body of work by othersâ€) [sic]. How could this be? I have worked my whole life on the problem of quantum gravity, including years of work and 18 technical papers, plus reviews, and books, on string theory itself." (Smolin commenting on (C. Johnson, 2006e))

Perhaps unsurprisingly, Smolin positions himself as having a wide experience and as such he argues that he has the unique authority to comment on string theory:

> "I am one of the few people who has actually switched my interest and activity among the different approaches, so unlike most people I can talk about what is involved from personal experience. I can report it is not that hard technically to switch between string theory and

[65] It is often difficult to precisely time stamp the introduction of a point of conflict in the string wars, especially given the ephemeral nature of blog discourse. Points of conflict are repeated over and over in the general milieu of crankiness.

> LQG [loop quantum gravity] and back again, but it does take a fair amount of honest soul searching." (Smolin commenting on (C. Johnson, 2006f))

Clifford's response and the response it elicited from Smolin, led the discussion away from Smolin in particular to discussion of the norms of the community as a whole:

> "a hop-and-skip-here-and-there approach one's to research can lead to being a 'Jack of All Trades, Master of None'. (I am not saying that this is the case for you, Lee.) Does this produce better results than dedicating a research program to really understanding a problem for several years? This is not clear to me, so I would say beware… it is not for everybody… in fact, it is probably not for most people, given the complexities of the problems involved – definitely not early in one's career." (C. Johnson, 2006f)

Smolin responded:

> "The point is that the measure of success of any scientific research program is not the degree of agreement there may be among those who work exclusively on that program. There is, in each general research field, a larger community of people with the expertise to evaluate claims made by different research programs in that field. With respect to string theory that larger field includes all those trained in theoretical and experimental physics who work in quantum gravity, cosmology and/or elementary particle physics. To succeed, each of us who has a research program in the area has to amass enough evidence to convince all those that do not work on our research program that our approach is correct.
>
> If there are proponents of any particular approach, string theory or something else, who insist that they alone should be the judges of progress in their research program, they misunderstand how science works … the ethics of science requires that the community of people with this larger expertise are the court in which the claims of any research program are to be judged." (Smolin commenting on (C. Johnson, 2006f))

This exchange exhibits nuances of Johnson's position. He is not arguing that a homogenous focus on string theory is sufficient to conclude the success of string theory. Rather his position is that a homogenous focus is required to have the expertise to appraise the success or failure of string theory. In contrast for Smolin the net must be cast wider so as to include the quantum gravity community, and heterogeneous expertise is necessary to conduct an appraisal of any theory of quantum gravity. These positions are divergent in their positive characterisation of expertise.

This argument is reminiscent of the normative feminist epistemologies as argued by (Longino, 1990, 1999) and (Okruhlik, 1994), and the normative social epistemology argued by (Kitcher, 1990). Certain protagonists in the string wars, I argue, are attempting the "revamped" classical epistemology rather than the "radical" epistemology, as described by Goldman below:

> "According to some writers, social epistemology should retain the same general mission as classical epistemology, revamped in the recognition that classical epistemology was too individualistic. According to other writers, social epistemology should be a more radical departure from classical epistemology, a successor discipline that would replace epistemology as traditionally conceived." (Goldman, 2010)

The positions examined throughout this chapter are arguments within the aims of classical social epistemology as they do not reject the concepts of truth and justification, and understand knowledge to be more than "simply what is believed, or what beliefs are 'institutionalized' in this or that community, culture, or context" (Goldman, 2010).

In particular, Smolin makes the normative argument for a scientific ethos whereby the organisation of science is 'corrective' as opposed to 'constitutive'. Smolin retains a place for rationality in consensus formation, and thus maintains a role for knowledge that is not exclusively what is believed. He further argues that when consensus via rational processes is not available, the social organisation of science should be such that it encourages a variety of perspectives (Smolin, 2006c, p. 301). This is in a sense similar, albeit more conservative, to the approach advocated for by (Longino, 1990) and (Okruhlik, 1994). Okruhlik takes the stance that "only the inclusion of diverse standpoints will bring about the conditions under which change is possible" (Okruhlik, 1994, p. 40). This comparison serves to highlight a consequence of Smolin's position that is the epistemic significance of the social organisation of science. As Okruhlik argues, "Once we recognize that the content of science is affected by the social arrangements that govern its practice and production, then those social arrangements acquire epistemic significance as do the affirmative action programs and other interventions undertaken to alter those social arrangements" (Okruhlik, 1994, p. 39). Kitcher has also argued that progress in science will be optimised when members of the scientific community are encouraged to pursue a variety of strategies to solve a problem (Kitcher, 1990, 1993). The debates over the course of this chapter reveal that, for those engaging with the points of conflict, the social organisation of science has epistemic and heuristic consequences, namely that the social organisation of science informs scientific judgements of the success/failure of string theory and its likelihood to succeed/fail in the future.

4. Epistemic appraisal and the appraisal of promise

In the previous chapter, the dispute over the criteria by which a theory maybe judged as scientific was analysed as an example of boundary work. In this chapter, the 'sociological' debates have been also been shown to be debates concerning criteria by which a research program may be judged. Instead of invoking philosophical models such as naïve Popperianism, toy sociological models of the academy were employed so as to normatively argue for and against the dominance of string theory, for divisions between expert and non-expert populations of scientific communities and for heterogeneous versus homogenous expert scientific populations. These debates reveal contrasting epistemologies where it becomes apparent that, for some, the social organisation of science has epistemic consequences, both positive and negative.

In a chapter devoted to 'What is science?', Smolin argues that there is no single scientific method on which the progress of science fundamentally depends (Smolin, 2006c, pp. 289-307). Later, he insisted that "the success of science is due to the formation of communities tied together by ethical principles" (Smolin, 2007a). This sociological conception of science, Smolin argues, is "the major theme of theme of the book" (Smolin, 2007a). Scientific success is contingent on a particular organisation of a scientific community:

> "Science has succeeded because scientists comprise a community that is defined and maintained by adherence to a shared ethic. It is adherence to an ethic, not adherence to any particular fact or theory that I believe serves as the fundamental corrective within the scientific community." (Smolin, 2006c, p. 301)

This view of science requires that in situations where there are no rational and empirical grounds to forge a scientific consensus, "we should encourage a wide diversity of viewpoints" (Smolin et al., 2007, p. 4). Smolin's articulation of the 'scientific ethos' represents an attempt to define certain sociological norms which are binding on the scientific community and which are essential to 'good science'. Deviation from these norms results in pathological science, in which progress grinds to a halt (Smolin comments on (C. Johnson, 2006f)). Smolin's commitment to the 'scientific ethos', rather than any one version of the 'scientific method', reveals that for Smolin the organisation of the social structures of science has consequences for future scientific knowledge.

Smolin is often interpreted as a 'critic of string theory' and there are certainly passages throughout TTWP that can be read in support of that interpretation. However to simply label Smolin as such is to misunderstand what is significant to him in TTWP: the organisation of science. In response to criticism, Smolin reasserted his position that:

> "It would be best for the progress of science if all those who work on these different approaches consider themselves as a single research community, within which we try to mix people doing different things, as well as vary our own research interests, because presently

> different questions are best approached with different approaches.” (Smolin commenting on (C. Johnson, 2006f))

This projective appraisal – or, in Johnsons’ terminology, “promise” in relation to which he argues that “you have to look at the promise of the subject. String theorists have demonstrated its manifest promise … and being able to explain very clearly too their peers from other fields what they are trying to do, and what they have done so far” (C. Johnson, 2006f), has been consistently been referred throughout the debates over sociology and quantum gravity.

The ‘sociological’ debates are interesting from a descriptive perspective as they deepen our understanding of disputes over the scientific status of research programs. This deeper understanding comes from the usual case of universal agreement that string theory is a dominant research program, and yet there is significant conflict over the normative appraisal of a dominant research program. The outcome of this dispute is still unclear and it is difficult to imagine how it will be resolved, particularly given that the amount of time and the resources spent on a research project are a point of conflict within the debates.

It would be easy to assume that all participants essentialise the category of science as a privileged route towards knowledge as distinct from a socially-corrupted route. This positions has some cogency: yes, it is often argued that sociology effects bad science in particular where participants are debating the existence of pathological, or socially-corrupted, science. However, in addition to these debates, there are also debates over sociology and ‘good science’ and what norms underpin successful scientific endeavours. There is a prevalent belief that bad organisation of science will impede progress, but there is also a belief that an optimal organisation of science will enhance progress.

It is further enlightening in terms of understanding the string wars. The social organisation of science into expert and non-expert communities and the organisation within expert communities into heterogeneous or homogenous communities are significant points of conflicts in the string wars. Here we see that the categories of popular and expert are being constituted and reconstituted over the course of the debates and remain slippery categories that fail to grip. The complexity of the string wars and the many points of conflict that see participants in the debates unite and divide highlight yet again that ideological and intellectual problems cannot be separated. The debates over ‘sociology’ are debates over the appraisal of string theory, which highlight that one aspect of appraisal is focused on the future rather than an appraisal of the present state of string theory (namely, is string theory science). This appraisal concerns the likelihood of progress, of future success and of the production of scientific knowledge. Chapter five will further examine the role of projective assessment the debates over methodology.

Chapter Four: Contested technologies

Introduction

Various controversies over string theory, collectively termed the ‘string wars’, intensified in 2005. The ‘string wars’ is a term employed to describe the, often public, debates over string theory that reached a climax in the northern hemisphere summer of 2006-07. These debates were played out in op-eds, blog posts, popular books, book reviews and recorded public debates. Many of the points of disagreement between critics and defenders of string theory turn on: complex, highly technical matters; prescriptions concerning the nature of scientific progress and the demarcation of science from non-science; and debates over the sociological norms of scientific inquiry and the scientific ethos.

Also in 2005 the open access preprint publisher, arXiv, instituted a new feature called a ‘trackback’. This new feature enabled authors of weblog (or blog) posts, discussing a paper on arXiv, to leave a trackback (a link) to the blog post on paper’s abstract page on arXiv. The institution of this new feature, and the determination of which specific bloggers would have access to the feature, generated a public controversy that was played out in the blogosphere. Noted string theory critic Peter Woit was denied access to the trackback feature for his blog ‘Not Even Wrong’. In response to Woit’s (and his supporters’) questions and criticisms, arXiv advisory board member and string theorist, Jacques Distler, revealed on his blog that arXiv was only allowing ‘active researchers’ to have access to the trackback feature. This generated significant disagreement within the high energy physics community. The disagreement centred on what were and what should be the defining features of an ‘active researcher’, as well as the norms to which peers of the high energy physics community did and should conform. Although the community was in almost unanimous agreement that so-called ‘crackpots’ should not have access to the trackback feature, it was unable to reach a consensus as to how to define a ‘crackpot’ or an ‘active researcher’.

I present the controversy over the trackback feature as a case study in which we find examples of blog discourse, each of which confounds categorisation as permanent or ephemeral communication. The case study reveals that the trackback feature was originally conceived to develop certain blog discourse as an alternative or complementary form of peer review. However, the ongoing function of the blog, as a form of scholarly communication, is questioned by the high energy physics community; both the function of the blog, as a form of peer review, and the identity of the ‘peer’ or active researcher are disputed.

1. Thematic concerns from the literature

There exists a growing body of literature, in the fields of science communications and information studies, that examines blogs and other web 2.0[66] applications as forms of scholarly communication. However, the interplay between blogs, arXiv and the trackback feature is yet to be examined in any academic literature.[67] The most comprehensive study of 'research blogs', with a particular focus on science blogs, was completed by Shema et al (Shema, Bar-Ilan, & Thelwall, 2012) in the field of information studies. Web 2.0 technologies have yet to be extensively addressed in history, philosophy or sociology of science literature[68] apart from a few small exceptions. A short piece by philosopher John Wilkins, who authors a philosophy of biology blog titled 'Evolving Thoughts', argues that blogs "contribute to the current practice and reputation of science" (Wilkins, 2008, p. 41) and consequently should be regarded as an important phenomenon to be understood. Meyer and Schroeder have argued that that "we must start to develop a sociology of online knowledge if we are to understand this emergent domain" (Meyer & Schroeder, 2009, p. 258). Bell (A. Bell, 2012) has argued, based on a small survey of people who blog about science relating to the brain, that further study is required to understand professional tensions existing between those who blog about science and scientist bloggers.

Broadly speaking, this paper argues that there are three themes that are beginning to be tentatively expressed by various authors: that blogs are an ephemeral form of scholarly communication; that blogs are a window into 'science in the making'; and that blogs are performing a function akin to peer review. This paper will explore each of these themes with reference to the case study presented, and will argue: that the trackback controversy is evidence for certain blogs being a window into science in the making; that there is a tension between ephemerality and permanence in blog discourse and that this tension is important to understanding the potential functionality of blogs as providing public peer review; and finally that the concept of the 'peer' is sufficiently contested so as to problematise the potential for blogs to be sites of peer review.

1.1 Ephemeral scholarly communication

Many authors have claimed that scientific blogs should be considered informal scholarly communication (Kouper, 2010, p. 8; Maron & Smith, 2008, p. 28; Pikas, 2008, p. 100; Puschmann &

[66] Web 2.0 refers to what is considered to be a second phase of the World Wide Web. This second phase is characterised by sites that tend be interactive with active participants who generate their own content.

[67] A small exception is a paper by Brian Trench in which he mentions, but does not explore, links between blogs and arXiv (Trench, 2012, p. 280). However Trench does not mention the trackback feature.

[68] Outside of the field sociology of science, researchers in the fields of journalism (Shanahan, 2011) and sociology, communications and geography (Riesch & Mendel, 2014) have both drawn on Gieryn's concept of 'boundary work' (Gieryn, 1983, 1999). Shananhan also extended Gieryn's conceptual framework to include 'boundary layers', as distinct from 'boundary objects', to her analysis of 'journalistic science blogs' (Shanahan, 2011). However Shanahan's study is naïve to many of the developments in the fields of history and philosophy of science and sociology of science.

Mahrt, 2012, p. 180; Smith, 2008, p. 15). This occurs explicitly: for example, Meyer and Schroeder argue that “online knowledge includes both formal outlets, such as journals and data archives, as well as informal outlets, such as blogs, webpages, and podcasts” (Meyer & Schroeder, 2009, p. 247). Warden also took note of the interactive nature of blogging, stating: “blogging is one Web 2.0 tool that is well suited to informal communication by researchers” (2010: 203), and Maron and Smith claim “blogs are clearly an informal method of scholarly communication” (Maron & Smith, 2008, p. 28).

An important element to the characterisation of blog discourse as informal is the percieved emphemerality of the form. The distinction between formal and informal scholarly communication may be traced back to the work of the sociologist William Garvey (Garvey, 1979). Garvey argued that scientific communication can be categorised into two domains: formal and informal. Informal communication is things such as conversations in hallways, with small audiences, drinks at the pub and comments on drafts from supervisors or colleagues. Garvey argued that informal communication incorporates ephemeral communication that is conducted between private networks (Garvey, 1979).

To describe science blogs as ephemeral is an intuitive response that ties in with the current folk understanding of more general blogging phenomena and of the use of social media platforms. It is also a commonplace assumption that exists in much of the literature surrounding blogs as part of the scientific discourse (Shema et al., 2012). Wilkins has argued that “blogs are a highly idiosyncratic, personal and ephemeral means of public expression” (Wilkins, 2008, p. 411). Trench (Trench, 2012, p. 278) also quotes Wilkins’ description of blogs as ephemeral communication). Whereas journal papers and academic books, once published, have an unchanging text, blog posts evolve. Blog posts evolve in two ways: the author of the blog post may edit the content of the post at any stage and, with time, comments are added and deleted. The author of the blog acts as a moderator of the discussion and may also choose to delete comments (and this frequently occurs). These comments have the potential the change how the blog post is read in a number of ways, as the blog post is situated in an evolving context. It appears, but it is not clear from the literature, that the perceived ephemerality comes from a perception of text that disappears without record.

1.2 A window into ‘science in the making’

It is often argued that blogs may provide a view into ‘science in the making’ (Carroll, 2006f; Griffiths, 2007; Shema et al., 2012; Trench, 2008; Wilkins, 2008). Carroll deemed this to be a positive thing: “it’s a great opportunity for physicists to exchange ideas more readily with each other, and to let the rest of the world share the thrill of the process by which science truly progresses” (Carroll, 2006f, p. 8). Wilkins also deemed the development to be positive: “blogging is also a way to demythologize science. Unlike laws and sausages, the public should see science during its manufacture” (Wilkins, 2008, p. 411). Griffiths was more measured in his appraisal of the situation: “for better or worse, blogs have opened up a new form of discourse in physics that can – as it is carried out in a public fashion – be propelled into a broader context in a way that a discussion at a conference, say, would not have been" (Griffiths, 2007, p. 25).

Trench also explored the idea that blogs are a window into science in the making, quoting a *Nature* journalist in 2008, as his "central argument": "blogs are windows into academic coffee room chatter of the sort the media is not normally privy to" (Tomlin, quoted in (Trench, 2008)). Trench both requoted and reiterated a similar argument four years later (Trench, 2012), but he did so in a much more limited way. In particular, he argued that the majority of blogs are not providing even brief opportunities to examine 'science in the making' (Trench, 2012, p. 280) with one exception from which the case study that forms the basis for this paper is drawn: particular physics blogs and their interaction with arXiv. Trench claimed that links between blogs and arXiv "offered relatively rare examples of blogs facilitating public view of 'science-in-the-making'" (Trench, 2012, p. 280). Trench did not, however, elaborate on that claim, as his primary investigation centred on climate change blogs.

1.3 Peer review

The claim that blogs may be providing a kind of public peer review has found recent expression. However, many authors proceed without providing a clear understanding of how they conceptualise peer review. Where the possibility that blogs may be performing a function akin to peer review is mentioned in the literature and some anecdotal evidence is offered, few examples are offered and sometimes repeated (Bonetta (Bonetta, 2007, p. 445) described the same example as Batts et al (Batts, Anthis, & Smith, 2008, p. 1837)). Batts et al. describe a post-doctoral researcher who ends up co-authoring a paper after his blog post critiquing the conclusions made in paper published in *Nature* is recognised as 'beating another researcher to the punch'. This example is argued to be evidence that "blogs can have a substantial impact on traditional academia by providing a quick forum for public peer review" (Batts et al., 2008, p. 1837). No further details are offered by either Batts et al. or Bonetta (indeed, the details slightly differ in each: Bonetta claims the blog post is authored by a PhD candidate (Bonetta, 2007, p. 445)).

In this general form, peer review is construed as having two functions: criticism and evaluation of significance. Recently, Riesch and Mendel, in their UK-focused study, have argued that "the ability of those within and outside the group to read, comment on and challenge blog posts has come to serve as a form of informal peer review" (Riesch & Mendel, 2014, p. 54). Schmidt has argued that blogs can make visible "informal second stage peer review" that occurs at conferences that would not have otherwise be made public or available to colleagues unable to attend (Schmidt, 2008, p. 208). Shema et al. have also claimed that "science blogs can add to the transparency of the scientific process by reviewing and discussing the science culture in general and scientific research in particular" (Shema et al., 2012, p. 1).

The second function is to identify papers of potential impact: Shema et al. have also proposed that research blogs may provide a potential source to determine the impact of scholarly material, arguing that "[science blogs] allow informal post publication peer-review" (Shema, 2014, p. e35869). They claim this proposal is backed by their data, which indicates that journal papers that have blog entries dedicated to them receive a higher number of citations. They conclude that blogs "will become part of future research evaluation metrics" (Shema et al., 2012, p. e35869). Shuai et al. have also noted this effect with reference to papers that receive a Twitter announcement (Shuai, Pepe, & Bollen, 2012). This is a democratic construal of peer review that attempts to harness what Minol et al. have referred to as 'the wisdom of the masses' or utilising a 'collective intelligence' (Minol, Spelsberg, Schulte, & Morris, 2007, p. 1132).

A significant difficulty here is that much of the literature discusses 'science blogging' as a general phenomenon, and does not acknowledge differing practices of particular communities. Only nine of the 913 references examined by Shema et al were to blog entries discussing papers on arXiv, and the authors did not allude to whether those arXiv papers had been through traditional peer review when uploaded onto arXiv (Shema et al., 2012, p. e35869). One exception is the aforementioned work of Trench (Trench, 2012) who examined the 'Climategate' affair as a detailed case study and also briefly drew on the interactions between astrophysicists and particle physicists, arXiv and blogs. Trench argued that he had found 'indications' for "possibilities for a kind of public peer review through blogs … in the contrasting domains of astrophysics and particle physics, on the one hand, and climate science on the other" (Trench, 2012). Like Minol et al (Minol et al., 2007), Trench saw evidence for blogs facilitating a critical appraisal from a wider public (Trench, 2012).

2. Methodological concerns with the scientific blogosphere

A number of difficulties are encountered when using blog posts as source material. The most significant difficulty, as discussed earlier, is that blog posts are not always a static medium. Each of the authors of the blogs used as source material in this case study meticulously archives each post so that it is relatively easy to retrieve posts dating back to the creation of the blog. The main text of a blog posts written by the authors examined remains, to a large extent, permanent, with authors adding time-stamped updates at the bottom of the post rather than editing the original text. However, if an author did choose to edit the text, without a previously archived copy of the blog post, it would be very difficult to determine whether or not an edit had taken place. Furthermore, links to blogs that are no longer active are often broken, and often material has been removed from those blogs.[69]

An additional difficulty is that each blog author moderates the commentary section of his or her own blog based on his or her own personal standards. Consequently, it is impossible to determine, particularly years later, if comments have been deleted. Similarly, blog authors have the ability to 'close comments'. Blog authors typically will publish that 'comments are now closed', but the timing of that closure is not always published. Each blog post that was used as primary source material in this case study was archived so that a permanent copy could be maintained as it was used in the study.

A further difficulty in using blog posts as source material concerns issues of identity and anonymity. The intuitive response to any question regarding controversies played out on internet forums may be to assume that many participants participate under the protective cloak of anonymity. However, very few participants in the controversy concerning access to the arXiv trackback feature participated anonymously. The authors of each blog used in this case study list their names and institutional affiliations on their blogs with one exception; one author chose to write anonymously and writes under the pseudonym 'Capitalist Imperialist Pig' (Anonymous, 2004 - Present). However the author writing under that pseudonym makes it clear that he has a position at a university in the United States. Among the individuals who comment on those blogs, there is a mix of known individuals, anonymous individuals and individuals writing with a pseudonym (often known by those in the community to correspond to a particular identity). If a commentator is also an author of a blog, his or her name will also act as a link to his or her blog, thus confirming his or her identity.

[69] In particular, the commentary section from the 'Cosmic Variance' blog (Various, 2005 - 2013), which had several contributing authors, is no longer visible. One of the authors, Sean Carroll, has replicated all the blog posts that he personally authored, as well as all comments, at his personal blog, 'The Preposterous Universe' (Carroll, 2012 - Present).

3. ArXiv

> "While each new generation thinks it's somehow unique, there are objective reasons to believe that the past two decades have witnessed an essential change in the way information is accessed, and how it is communicated to and from the general public, and among research professionals." (Ginsparg, 2011, p. 1)

Inspired by a string theorist, Joanne Cohn, Paul Ginsparg created hep-th@xxx.lanl.gov, an automated email server for a small community of high energy physicists, the majority of whom were string theorists (Taubes, 1993, p. 1246).[70] The server was expected to receive around 100 submissions a year (Ginsparg, 2011, p. 4). In 1992, Ginsparg installed WorldWideWeb.app transforming the e-print archive into a webserver located at xxx.lanl.gov (Ginsparg, 1994, p. 159). In that same year, David Mermin suggested that "this could well end up as [string theorists'] greatest contribution to science" (Mermin, 1992, p. 9).[71]

ArXiv evolved into an open access resource and publisher of over 945,000 preprints (or e-prints)[72] in physics, mathematics, computer science, quantitative biology, quantitative finance and statistics. In an attempt to restrict access to arXiv, a two-step filtering process was instituted in 2004. In order to upload a paper onto arXiv, the following requirements must be satisfied:

> "1. The author must be an approved submitter, usually through having been endorsed[73].
>
> 2. Each paper from an approved submitter must be accepted by the moderator for that section of the arXiv." (Distler, 2006)

Moderators check that content is 'on topic' (rather than checking for accuracy) and endorsement of an author may come from a record of previous submissions, institutional affiliation or via another author who has authored 'a certain number' of papers within the same field. Today arXiv is hosted by Cornell University and is the primary means by which high energy physicists (and several other disciplines within physics, mathematics) access papers (Gunnarsdóttir, 2005). Figure 4.1 below depicts the home page of arXiv, which lists the disciplines and sub-disciplines archived.

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[70] It was renamed arXiv in 1998 (Ginsparg, 2011).

[71] Rickles (Rickles, 2014) has noted that, throughout the history of string theory, string theorists have often been among the first to adopt new computer based technologies.

[72] Kling (Kling, 2004) has argued that, given that the rate of papers that were ultimately accepted for publication was not 100%, a better terminology would be 'manuscript' or 'e-print' for an online version.

[73] Hyperlink to: http://arxiv.org/help/endorsement.

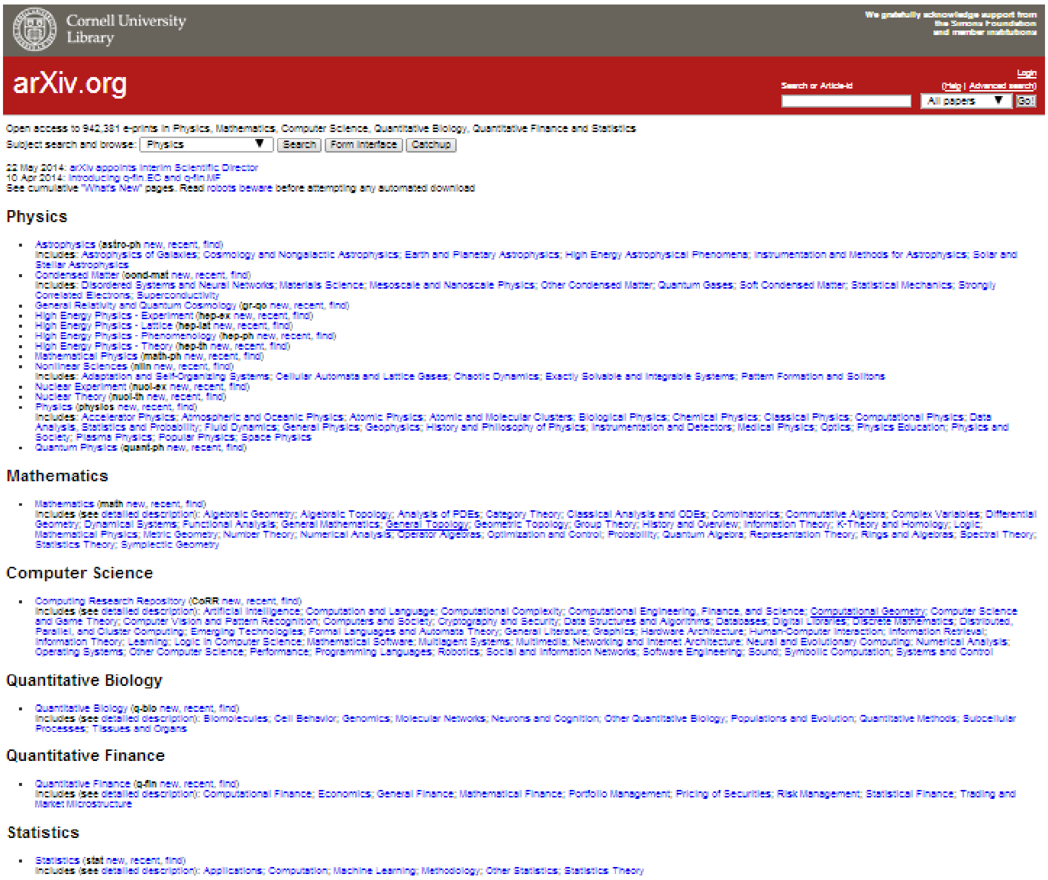


Figure 4.1 Front page of arXiv.org (Accessed 29.5.14)

4. Trackbacks

At the time of writing, the arXiv trackback feature involved a link located on the abstract page of a paper published on arXiv (see Figure 4.2 below with the location of the trackback feature circled). If clicked, the link took the reader to a list of links to blogs that featured a discussion of the paper published on arXiv.

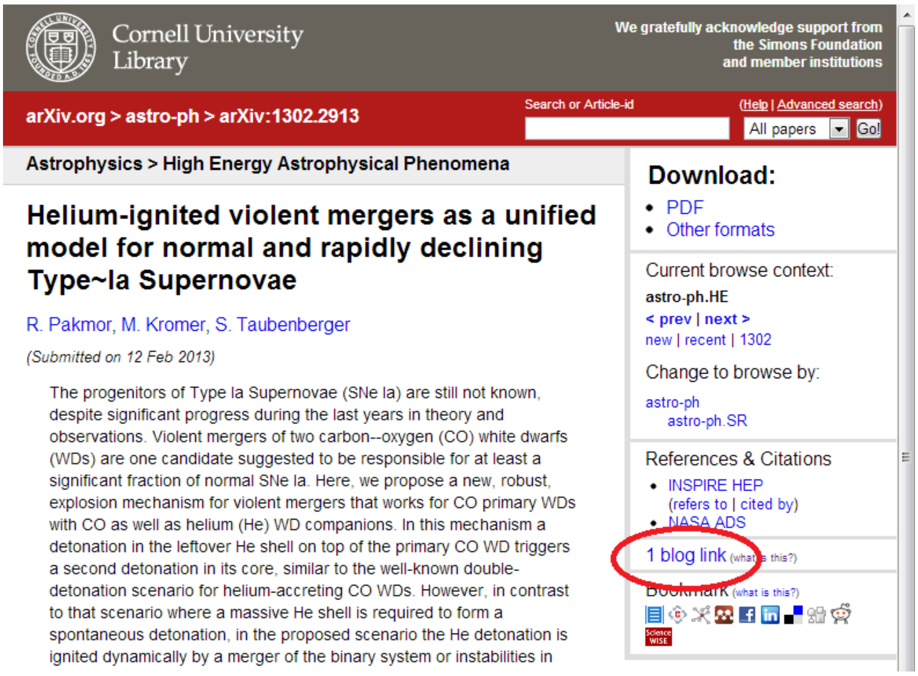


Figure 4.2 Example abstract page from arXiv.org

While not explicitly addressing trackbacks to arXiv (or a similar alternative), Luzón has claimed that trackbacks between blogs are used by participants to follow a conversation across more than one blog (Luzón, 2009). Luzón argues that trackbacks have the rhetorical function of community formation (Luzón, 2009, p. 86). For example, if a discovery is announced, an author may include links to each of the blog posts discussing the same discovery. This may be done for a variety of purposes, such as to endorse blog posts that an author considers to be offering a useful contribution, or to deliver criticism where an author links to the blog that he or she claims is making inaccurate statements.

5. Case study: controversy over the trackback feature

5.1 Beginnings

The precise origin of arXiv's trackback feature is difficult to ascertain given that, in its basic form, the trackback (in essence, a link) is not original and has taken on a variety of forms. Jacques Distler, a string theorist from the University of Texas and author of the blog 'Musings' (Distler, 2002 - Present), has claimed that arXiv trackback feature originated from his posting titled 'With enough eyeballs: a manifesto' (Distler, 2002b). In this posting, Distler argued that the weblog may be able provide a 'feedback loop' for e-print archives and, as a result, the weblog might perform the function of peer review.

Distler drew inspiration from what he referred to as an "oft-quoted maxim from the world of open access software" (Distler, 2002b). In a comment, Distler clarified to whom he attributed the authorship of the quote (in response to criticism from another commentator) and provided the context from which he drew the "maxim":

> "One of the core practices used in open-source software is peer review: Because everyone can see the code, everyone can see your work. One obvious benefit of peer review is that mistakes get caught sooner. I call this Linus's Law, after Linus Torvalds: '*With enough eyeballs, all bugs are shallow.*'" ((Raymond, 1999, p. 29) as cited in (Distler, 2002b); emphasis author's own)

The second source of inspiration is the Bogdanov hoax that featured two brothers, Igor and Grichka Bogdanov, who had two papers accepted for publication in a peer reviewed journal. After the papers were published, rumours circulated that the papers were part of a reverse Sokal hoax. Unlike the Sokal case, the brothers denied that the papers were a hoax and a controversy erupted, to a large extent in an online discussion group run by John Baez, as the string theory and high energy physics communities struggled to determine the legitimacy of the papers.

In the main body of the posting, Distler claimed that the weblog might provide a model for peer review due to three developments:

> "1. "Moveable type (weblogging software)
>
> 2. The Trackback
>
> 3. MathML 2.0 ("a nice way to do math")" (Distler, 2002b).

The trackback is described as a feature that provides an "automated way to link *back* to another site which references a given article" ((Distler, 2002b); emphasis author's own). It is claimed that the ability to connect a blog post to the abstract page via a trackback will overcome the difficulty of discovering the location of discussion on a given article, thus enabling active community members to follow the discussion wherever it might go.

5.2 An announcement of the adoption of the trackback feature

Sean Carroll, a cosmologist from Caltech,[74] announced the adoption of the trackback feature at arXiv in a blog posting on 24 August 2005 at 6.14pm (Carroll, 2005a). Carroll pointed to the dual practices of most physicists of submitting papers to arXiv before they submit to a journal and reading papers from arXiv almost exclusively. Carroll claimed:

> "Now your blog post can send trackbacks to the abstracts of papers at the arxiv!...Now, if you write a paper and people comment on it on their blogs, that fact will be recorded right there at the abstract on arxiv.org. Drawing us one step closer to the use of blogs as research tools." (Carroll, 2005a)

This was the first 'public announcement' of the new feature. The discussion quickly (within two and a half hours) moved to Distler's blog. However, in the initial comments on Carroll's post, some of the central concerns of the controversy are voiced. Concerns over noise, flame wars, crackpots and anonymous attacks are expressed.

5.3 An 'official' announcement of the adoption of the trackback feature

Twenty minutes after Carroll's posting, Distler posted to his blog an entry titled 'Trackbacks and the Arxivs' (Distler, 2005). He began that entry by claiming that a forum for critique for arXiv was an expressed interest of the high energy physics community: "ever since hep-th was founded in 1991 (or maybe a couple of years later, when it sprouted a web interface), people have talked about trying to overlay some sort of "discussion" or "commentary" layer" (Distler, 2005). Distler claimed that the advantage of the blog medium was that it provided an "interwoven, distributed conversation'; however, what was missing was the 'ability to plug into the conversation'" (Distler, 2005). Distler also revealed that he had been lobbying Ginsparg to feature trackbacks on the arXiv site. Distler was now able to publicly announce, and claim the credit for, arXiv's adoption of the trackback feature (Distler, 2005).

Finally, Distler added:

> "So does that mean that every crackpot and trackback spammer on the internet can now get linked-to from the arXivs? Well, … *no*. Just as you need to be a registered author[75] to submit papers, your weblog needs to be on an approved list, in order for your Trackbacks to appear.

[74] Throughout the string wars Carroll has at times written in defence of string theory, see for example (Carroll, 2005b, 2006d). Carroll has also argued that there is value in diversity of viewpoints; one such example (Carroll, 2006a) is presented in a later part of this case study.

[75] Here Distler refers to step one of the two-step moderation process outlined earlier.

> Going forward, the precise mechanism for getting on that list is yet to be determined. But, in the short term, the list of serious physicist-bloggers is short enough to handle by hand.
>
> You know who you are …" ((Distler, 2005); emphasis author's own)

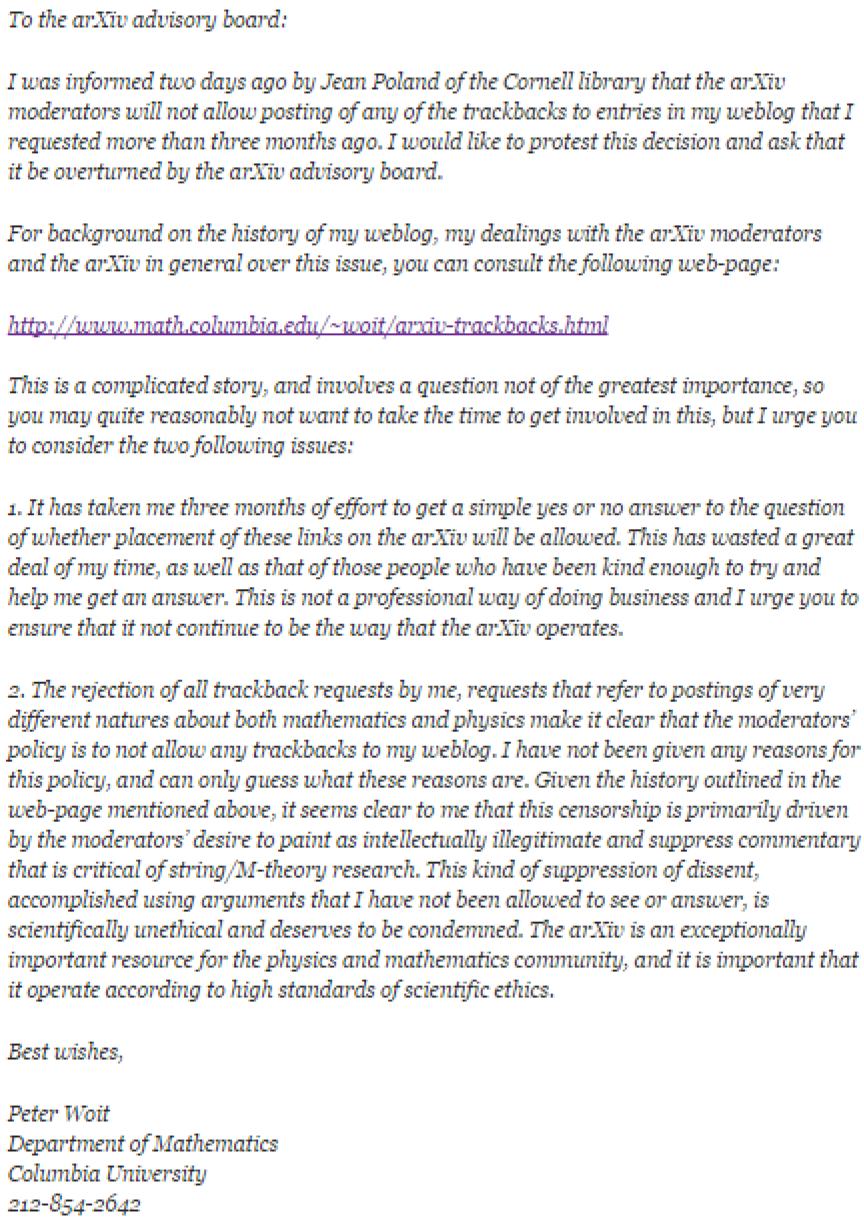
This was the second announcement regarding who would be eligible to receive trackbacks to their blogs (Carroll reported that a statement was initially present on arXiv that indicated that there was a "semi-automated editorial process"[76] (Carroll, 2005a). It was, however, the first announcement to consider in some detail the thoughts behind access to the trackback feature. Distler has also made explicit that despite quoting Torvalds in 2002 ('with many eyes all bugs are shallow') that the trackback feature had not been designed to support peer review as collected from the 'wisdom of the masses' instead certain individuals will be included and excluded.

The post received eleven comments (that were not deleted as of the time of writing). The comments took the form of questions and mainly positive feedback. Several questions pertained to the 'serious physicist blogger'. 'Michael' asked: "But your post suggests that crackpots will be blocked. In that case, it's not clear what "known blogs" means in the arXiv's description. Who's right? Who are the "serious physicist-bloggers"?" ('Michael', commenting on (Distler, 2005)). Distler responded with a clarification of the situation: "trackbacks to the arXivs are, indeed, moderated"(Distler, 2005). Distler did not provide specific detail in relation to who was considered a 'serious physicist blogger'. From the time at which the trackback feature originated, blogs were deemed to be 'in' or 'out' based on the author rather than the content (although, of course, these are linked). Some physicists are categorised as 'serious' and consequently their online output is deemed worthy.

5.4 Letter to arXiv advisory board

On 23 February 2006, in response to an official notice that his trackbacks would not be allowed on arXiv, Woit published a letter he had written to the eight member arXiv advisory board (see Figure 4.3 below (Woit, 2006c)).

[76] The full quote comes from a comment on Distler's post 'Trackbacks and the Arxivs': "Trackbacks will not be immediately visible. Because of widespread Trackback spam we have a semi-automated editorial process that approves trackbacks for display. Trackbacks from known blogs should become visible in a few minutes, but it may take longer for us to recognize new blogs" (Williams quoting arXiv policy on (Distler, 2005)).



To the arXiv advisory board:

I was informed two days ago by Jean Poland of the Cornell library that the arXiv moderators will not allow posting of any of the trackbacks to entries in my weblog that I requested more than three months ago. I would like to protest this decision and ask that it be overturned by the arXiv advisory board.

For background on the history of my weblog, my dealings with the arXiv moderators and the arXiv in general over this issue, you can consult the following web-page:

http://www.math.columbia.edu/~woit/arxiv-trackbacks.html

This is a complicated story, and involves a question not of the greatest importance, so you may quite reasonably not want to take the time to get involved in this, but I urge you to consider the two following issues:

1. It has taken me three months of effort to get a simple yes or no answer to the question of whether placement of these links on the arXiv will be allowed. This has wasted a great deal of my time, as well as that of those people who have been kind enough to try and help me get an answer. This is not a professional way of doing business and I urge you to ensure that it not continue to be the way that the arXiv operates.

2. The rejection of all trackback requests by me, requests that refer to postings of very different natures about both mathematics and physics make it clear that the moderators' policy is to not allow any trackbacks to my weblog. I have not been given any reasons for this policy, and can only guess what these reasons are. Given the history outlined in the web-page mentioned above, it seems clear to me that this censorship is primarily driven by the moderators' desire to paint as intellectually illegitimate and suppress commentary that is critical of string/M-theory research. This kind of suppression of dissent, accomplished using arguments that I have not been allowed to see or answer, is scientifically unethical and deserves to be condemned. The arXiv is an exceptionally important resource for the physics and mathematics community, and it is important that it operate according to high standards of scientific ethics.

Best wishes,

Peter Woit
Department of Mathematics
Columbia University
212-854-2642

Figure 4.3 Woit's letter to arXiv's advisory board

Woit's characterisation of the actions of the arXiv advisory board as "suppression of dissent, accomplished using arguments that I have not been allowed to see or answer" serves to paint himself as the archetypal Galileo character that is railing against an unethical or ideologically motivated majority. He also characterises his opponent's motive as censorship of a dissenting view rather than censorship of an illegitimate view.

This post may be considered to mark the point at which the controversy began to change from a dull roar to a multi-factioned argument. The post spawned many blog postings[77] and elicited 122 comments (not deleted at the time of writing). The tone of the first comments was markedly negative:

> "Peter, I'm not sure if you're looking for an answer or just trying to pick a fight. If you want an answer, I can help you out: We don't need anyone commenting on string theory papers who hasn't done any research in the area, has been academically dead since the 80s." ('Michael', commenting on (Woit, 2006c))

'D R Lunsford' responded with: "Michael, you are such an ass. Peter, good luck" (Lunsford, commenting on (Woit, 2006c)).

The pace of the conversation between participants is quite astounding. One part of a comment written by Chris Oakley attempted to get the attention of Nigel Cook: "while we are on the subject of ArXiv – here is a note for Nigel Cook, who I know reads this and whose e-mail does not seem to work" (Oakley, commenting on (Woit, 2006c)). Despite not having previously commented on the post, Nigel Cook responded within nine minutes, indicating he may have been following the discussion closely or that the discussion may have been brought to his attention by someone else following it closely (Cook, commenting on (Woit, 2006c)). This behaviour indicates that there are individuals, or 'lurkers', who follow the discussions closely but do not comment.

The question of the role of ideological commitments in science is also raised. Woit compares 'Not Even Wrong' (Woit, 2004 - Present), the blog, against string theorist Lubos Motl's blog, 'The Reference Frame: Our stringy universe from a conservative viewpoint' (Motl, 2004 - Present). Motl often used his blog to promote ideas in string theory and degrade alternative approaches. In this instance, Woit criticises what he views as Motl's "fanaticism":

> "[Motl] is a fanatic and an extremist, both in his political and scientific views … he goes on and on about how anyone who disagrees with him about politics or string theory is an incompetent fool, and does whatever he can to suppress any such disagreement… the problem is that the moderators of the arXiv, while lacking Lubos's political fanaticism share his scientific fanaticism. This form of fanaticism has done a huge amount of damage by now to theoretical physics, and promises to do much more in coming years unless people take a stand against it." (Woit, commenting on (Woit, 2006c))

Several commentators also blurred the lines between critique of content of blog postings and critique of the authors. These critiques often took the form of either claiming the author was not a (good) scientist or that the author operated in the public, and non-scientific, domain. One such example is

[77] (Anonymous, 2006b; Carroll, 2006a; Distler, 2006; Hippel, 2006; Motl, 2006a; Orzel, 2006b).

'Benni' who criticised Woit: "I think you are banned, because you do not scientific work. You only criticise stringtheorie (sic) in a rather "public" manner" (Woit, 2006c). Woit was called upon to re-express his criticisms in the form of a published paper. Both critics and supporters of Woit's claim to be able to access the trackback feature accuse each other of violating the norms of the scientific community. Woit and his supporters assert that it is 'scientifically unethical' to operate in an opaque manner, and they level accusations of censorship at arXiv (Various commeningt on, (Woit, 2006c)). The reply from those who do not believe that Woit should have access to the feature centres on the implied endorsement that comes from an association with arXiv. Critics argue that Woit does not do science; instead Woit is labelled as a science populariser. This is a classic example of Gieryn's boundary work where participants in controversies engage in the rhetorical construction of boundaries so that one's opponent is deemed outside of science (Gieryn, 1983, 1999), as was explored in chapter two.

5.5 Woit receives some support

A little under a week after Woit published his letter, the controversy further escalated when Sean Carroll wrote a blog post in defence of Woit, titled 'Crackpots, contrarians, and the free market of ideas' (Carroll, 2006a). In particular, Carroll argued that:

> "On the one hand, I certainly don't think that scientists have any obligation to treat the opinions of complete crackpots with the same respect that they treat those of their colleagues … On the other hand, I don't think there is any sense in which Peter is a crackpot, even if I completely disagree with his ideas about string theory. He is a contrarian, to be sure, not falling in line with the majority view, but that's hardly the same thing. Admittedly, it can be difficult to articulate the difference between principled disagreement and complete nuttiness (the crackpot index is, despite being both funny and telling, not actually a very good guide), but we usually know it when we see it." (Carroll, 2006a)

The comment "we know it when we see it" is particularly revealing and is repeated by several commentators. A significant amount of the discussion in this controversy pertains to establishing the boundary between the characters of the 'crackpot' and of the legitimate contributor. There is unanimous agreement regarding the existence of 'crackpots'; however the community of commentators finds the boundary between crackpots and legitimate contributors imprecise and difficult to determine. Despite these difficulties, the existence of certain norms within the community is not up for dispute so that, even in the face of almost a complete lack of consensus in defining these norms, it is still argued both that those norms exist and that "they know [them] when they see [them]".

Carroll's posting garnered over 100 comments from all over the high energy physics community and community participants continued to comment for over a month. One such comment was provided by an ex-member of arXiv's advisory board:

> "As a long time reader of Peter's blog, a journal editor, and a (former) member of the ArXiv advisory board, I have a few comments. I offer them for what they're worth. Bear in mind that they do not represent the official opinion of the ArXiv, its advisory board, Johns Hopkins University, or any scientific journal or organization, past, present and future.
>
> The ArXiv instituted a standard that they would allow trackbacks only to blogs run by active researchers. That excludes Peter, who likes to discuss physics, but is not a researcher. It also excludes lots of other people, although I can't remember anyone else's name coming up." (Vishniac, commenting on (Carroll, 2006a))

This comment introduced the term 'active researcher'. This was the first time that details of the policies of arXiv with respect to who has access to these features became public, and it is interesting to note that they were aired for the first time in the comment section of a blog. Despite this no specific criteria as to what constitutes an 'active researcher' (beyond the assertion that Woit is excluded from the category) were provided.

Several commentators, such as Motl, advocated for the trackback feature to be removed completely:

> "I fully support blocking most of the trackbacks from his website, and if these policies cause serious problems, I would support to cancel the trackback system altogether. From a moral viewpoint, I find it outrageous that people who don't even try to contribute anything to science – and who build on purely negative support of various crackpots and science-haters – should have a better access to scientific resources than, for example, graduate students who work hard and struggle with serious scientific questions." (Motl, commenting on (Carroll, 2006a))

Motl was not unique in his use of rhetoric focused on ethics. Loop quantum gravity[78] theorist Lee Smolin, in his defence of Woit, also commented: "it seems to me that this is not an issue of personal judgement, as there are principles and ethics in professional academic life that are expressed in policies that those employed by universities are governed by" (Smolin, commenting on (Carroll, 2006a)). Quoting the 2002 Cornell University Faculty Handbook section on 'Freedom in Research', Smolin argued that the principle of academic freedom meant that an academic institution should "do nothing to impede free discussion by professionally competent experts on scientific controversies" (Smolin, commenting on (Carroll, 2006a)). Smolin argued that Woit is part of the academic

[78] 'Loop quantum gravity' is an alternative theory of quantum gravity to string theory and as such is viewed by some as a competitor to string theory.

community due to a variety of claims to institutional and epistemic authority. In particular, Woit had a PhD in physics, held a faculty position at a major university and had previously published papers as well as having a book in press.

Most, if not all, contributors both to the blog posts and to the commentary sections of those posts favour some level of moderation of the trackback feature. Although the existence of ‘crackpots’ is not up for debate, the point of conflict revolves around how a community, an institution or even a blog owner can identify and eliminate the contributions of ‘crackpots’. Smolin does not argue that everyone should have access to the trackback feature; instead, he argues for some level of restriction, couching his claim in terms of “professionally competen[ce]” and members of the “academic community” (Smolin, commenting on (Carroll, 2006a)). Similarly, Woit argues that a line should be drawn as to who should have access to the trackback feature; he just disagrees with where that line is currently placed (Woit, commenting on (Carroll, 2006a)). Indeed despite the almost complete lack of consensus regarding access to the trackback feature, there is nonetheless unanimous agreement that some level of restriction should occur.

5.6 Woit receives some criticism

Motl took to his own blog the next day to further express his views in a post titled: ‘Crackpots and scientific resources’ (Motl, 2006a). Again we see similar themes repeated in Motl’s opening statements:

> “Several blogs have discussed the question whether the crackpots, fringe scientists and especially ‘professional science critics’ should have a free access to scientific resources such as arXiv.org. The boundaries between these groups on one side and scientists on the other side may sometimes be fuzzy, but when you see one, you usually know it’s a crackpot.” (Motl, 2006a)

Motl extends this now familiar claim to a discussion about whose ‘sight’ should determine the policies of institutional science. Motl argues that although those outside a field may well find it difficult to distinguish science, “these people simply shouldn’t determine the policies of arXiv.org because they are laymen or outsiders” (Motl, 2006a). Motl further argues that a university affiliation is not sufficient to determine scientific legitimacy because even though he believes that there is a positive correlation between a university affiliation and scientific legitimacy, there are enough counter examples to render a university affiliation as unreliable. Motl rails against granting anyone who does not express the views of scientific orthodoxy any kind of institutional power.[79] This power, he believes, should rest in the hands of those at arXiv: “it’s the job of the arXiv.org to try and protect the

[79] Ironically, Motl is a climate change sceptic and has written extensively on the subject (see, for example, (Motl, 2007c)). When challenged on the apparent contradiction in his positions, Motl characterised his anti-climate change stance as that of a ‘contrarian’ and not a crackpot (Motl, 2006a).

scientific server from an uncontrollable flow of links to scientifically defective resources" (Motl, 2006a). Motl's picture of arXiv is a one of a repository of items of 'scientific value' (Motl, commenting on (Motl, 2006a)) where the power and the responsibility to maintain and to determine what is of value should be held by the community of 'insiders'.

5.7 The 'active researcher'

On 5 March (two days after Carroll's post), Distler, after refraining from commenting on other blogs, wrote on his own blog with a post titled 'arXiv Trackback Policy' (Distler, 2006). His stated intention was to "explain the thinking that went into the policy, and then solicit your feedback" (Distler, 2006). This was the first 'official' announcement of the details of the policy from someone who was connected to arXiv,[80] and that announcement was unusual in that details of the policy were first published on a personal blog.

As promised, Distler outlined both the process and the details of the policy. He explained that a single stage of filtering had been chosen for unspecified practical reasons. The consequence of that approach was that each blogger either had all or none of his or her trackbacks approved. The endorsement mechanism used as one of the two-part process to get a paper published on arXiv was deemed too loose a criterion for a blogger to be accepted. Distler further claimed that "it is also vital to have a reasonably objective standard. "This looks like an interesting weblog" was not going to be a workable criterion. Nor would any number of other subjective criteria" (Distler, 2006). The standard that was adopted, which Vishniac had previously introduced, was the requirement that a blogger must be an 'active researcher':

> "It's not particularly hard to figure out who's an active researcher: just look at their publications. Exactly what level of activity counts as "active" is an issue. Wherever you draw the line, there will be borderline cases that require a judgement-call. But in most cases, the decision should be (and, indeed, has proven to be) straightforward." (Distler, 2006)

Distler's use of the word 'objective' is unusual; he simultaneously claims that the concept of the 'active researcher' is an objective standard and also admits that the concept is sufficiently vague and will result in borderline cases that require a subject to make a judgement call. However, Distler was adamant that Woit was not one such borderline case: "Peter Woit's publication record doesn't put him anywhere close to "active researcher" status… with *any* reasonable choice to draw the line Woit isn't one of those borderline cases" ((Distler, 2006); emphasis author's own). Distler also included a link to Woit's publications on arXiv (of which there were two): (Woit, 2001, 2002b). As Figure 4.4 below records, the post was updated after Paul Ginsparg sent Distler the latest statistics from the trackback database:

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[80] At this time Distler had a position on the arXiv advisory board.

Update: Paul Ginsparg has sent me the latest stats from the trackback database. There are currently 5132 trackbacks from 51 approved sites. In high energy theory, the sites with 30 or more trackbacks are:

High Energy Theory Sites with 30 or More trackbacks at arXiv.org

Blog	# trackbacks
This Week's Finds	731
The String Coffee Table	300
Musings	268
PhysComments	83
Luboš Motl	56
Cosmic Variance	33

Figure 4.4 List of the top five high energy theory blogs by number of trackbacks (Distler, 2006).[81]

At the time of writing, Distler's blog post was featured on five other blogs and elicited over 100 comments. The controversy raged in the commentary section, with over 22 000 words thrown around (Distler, 2006). Consequently, it is quite difficult to do justice to the variety of concerns and critiques expressed. What is immediately apparent is that no precisely defined understanding of the 'active researcher' was to, or will, be given by Distler: "It's probably not worthwhile trying to pin down what the precise boundary between "active researcher" and John Q. Blogger should be" (Distler, 2006). Instead, Distler gave an example of a blogger who was not 'anywhere close to "active researcher" status' (Woit) and a list of examples of those who qualified for such a status (see Figure 4.4 above) (Distler, 2006). These examples serve as bookends on the crackpot to active researcher spectrum.

From the large number of comments to Distler's post, a central concern emerged: 'what is an active researcher?' Loosely paraphrased, some of the comments typical of those found in the commentary section that questioned the meaning of the 'active researcher' concept were: How many papers? How many pages long? Over what time period? Is there a time limit? Do the papers need to be on arXiv? Do the papers need to be in the sub-field in which a person wishes to leave trackbacks, or do 'active researchers' have access to the feature across the whole of arXiv? What about incivility? What about Nobel Laureates who have not published anything lately? Do the papers need to be peer reviewed? What about Perelman: he did not publish anything for nearly a decade while solving the Poincare

[81] 'This Week's Finds' is a blog authored by John Baez's (Baez, 1993 - 2011). 'The String Coffee Table' has various authors (Various, 2003 - 2006). 'PhysComments' was no longer accessible at the time of writing (the original URL was http://physcomments.org/). 'Cosmic Variance' was written by various authors (including Sean Carroll) (Various, 2005 - 2013).

conjecture? What about anonymous blogs? Are graduate students subject to the same conditions? (Various, commenting on (Distler, 2006)) These questions illustrate a community grappling with complex two-part question: how does a community decide who has earned an epistemically privileged status, and is this status permanent or revokable? This philosophically heavy debate played out on a public medium.

Among the commentators, Woit expressed criticisms similar to the issues raised in the comments identified above. In particular, he stated that: "You aren't using the dictionary definition of "active researcher", you're making up your own, and not able to tell us precisely what it is" (Woit, commenting on (Distler, 2006)). Of course there is no dictionary definition of an 'active researcher' so what precisely is understood to be the 'dictionary definition' of an active researcher is unclear. However, Woit's strategy is clear: he argues that Distler and arXiv's advisory board's use of the term 'active researcher' was not in keeping with the usage of the high energy physics community.

In the days that followed Distler's post, a couple of bloggers, Chad Orzel and 'Capitalist Imperialist Pig' each wrote a commentary on the evolving controversy (Orzel, 2006b) (Anonymous, 2006b). In explicit agreement with 'Capitalist Imperialist Pig', Orzel wrote:

> "Are you trying to cause problems? … Having the ArXiv board decide who is and isn't an 'active researcher' is just insane, if the goal is actually to avoid controversy. Not only is the closed-group nature of the decision ample fodder for conspiracy theorists, just the name is a disaster. If you're going to be insulting, why not go all the way, and just call your approved posters 'Really Smart People'?" (Orzel, 2006b)

Orzel brought attention to another aspect of this controversy. When Distler and the members of arXiv's advisory board created the category of an 'active researcher', they correspondingly created the category of a 'not-an-active researcher', the members of which would be denied from being fully active members of the scientific community. The terminology seems to be chosen so as to render Woit and others as outside the scientific community.

The case study outlined in this paper provides an opportunity to examine several practices, the propriety of which is problematic and unsettled. Harry Collins famously declared that scientific knowledge is like a ship in a bottle: once it is in the bottle it is difficult to imagine how it got it there (Collins, 1975). Shapin and Shaffer extended Collin's ship in the bottle metaphor to include the 'propriety of practices' and argued: "historical instances of controversy over natural phenomenon or intellectual practices have two advantages, from our point of view. One is that they often involve disagreements over the reality of entities or propriety of practices whose existence or value are subsequently taken to be unproblematic or settled" (Shapin & Schaffer, 2011, p. 7). One aspect of the controversy focuses on how to determine the legitimacy of certain contributors to the scientific discourse; that is, what constitutes a crackpot and what constitutes an active researcher?

Initially, it seems that what is at stake in this controversy is who should get an institutionally amplified voice. With arXiv performing such an integral role for the high energy physics community, there is unanimous agreement that not everybody should be able to have links to their blog on an abstract page on arXiv as this implies authority to comment on the paper in question. However there are examples that something more than contested and negotiated normative concerns over best practice is at stake. That something more is the function of science blogs within the scientific community and the role of science blogs within the scientific discourse. Are (some) blogs playing the role of a kind of public peer review? If we examine each of the themes expressed within the literature concerning the role and function of blogs, the case study of the trackback controversy provides both examples in support and counter examples to each of the general claims found in the literature, from which a more complicated picture emerges.

5.8 What constitutes a crackpot and active researcher?

Several themes emerge from the blog posts and ensuing discussion with regards to how to define a 'crackpot'. John Baez's 'crackpot index' starts each individual off with the score of -5 and then gives points ranging from 2 to 50 for making various claims[82] (Baez, 1998). The list of practices identified by Baez is frequently referred to as 'insightful' and amusing, but not definitive (Carroll, 2006a). Instead, much of the debate surrounding how to define a crackpot revolves around what a crackpot is not: a crackpot is not an outsider (Motl, 2006a); a crackpot is not a contrarian; or a crackpot is not an individual with an in principle disagreement (Carroll, 2006a). However these arguments just shift the disagreement and, unlike controversies such as the controversies over climate change, within the wider context of the 'string wars' there is no consensus opinion on which to fall back.

Chapters two and three, as well as (Camilleri & Ritson, 2015; Ritson & Camilleri, 2015), drew attention to the plurality of the controversies over string theory (as will chapter five). In particular in chapter two the debates over the scientific status of string theory were framed as an example of boundary work. The chapter draws on the work of Gieryn to reveal the ways in which protagonists appeal to, and rhetorically construct, different views about the scientific method and the scientific ethos in an effort to legitimise or delegitimise string theory. The chapter argues that the debates over string theory were unlike many other studied episodes of boundary work. Instead of a minority position attempting to widen a conceptualisation of science so as to permit their own membership, in the case of the string theory debates, the string theorists were forced to defend their dominance against accusations that string theory was not science. Distler, as part of the dominant majority, initially attempted to widen the boundaries of legitimate scientific practice so as to include blogging practices.

[82] For some other attempts see (Seigel) and ('t Hooft).

However, faced with criticism, Distler, and by extension the arXiv advisory board, is forced to defend his conceptualisation of the active researcher. Van Fraassen argued that a "vague predicate is usable providing it has clear cases and clear counter cases" (Van Fraassen, 1980, p. 16). However the list of examples provided by Distler is deemed deeply problematic by the participants in the debates, and no uncontested examples of crackpots, contrarians or active researchers were offered by those involved.

Two core concerns emerge from the controversy over the 'active researcher': what authority may be gained, first, from an institutional affiliation and, second, from publications? Participants in the debate argue that there is a significant difference between a tenured professor and a graduate student but struggle to articulate both the difference and significance. Furthermore, concerns relating to permanency further problematise the comparison. Should a tenured professor, once deemed an 'active researcher' be deemed so permanently? Or is the demarcation contingent upon on other factors (perhaps by publication, as will be discussed below)? Of further consideration, to those participating in the controversy, was the difference in research institutions or universities. What bearing upon an individual's claim to be an active researcher does an affiliation to a respected institution, in comparison to some other less respected institution, have? Unlike peer review which, at least in principle,[83] is a blind process, the majority of bloggers write with a known identity and a correspondingly publicised institutional affiliation.

The second concern, authority being obtained from publications, is similarly problematised by the participants in the controversy. Distler first announced that "[i]t's not particularly hard to figure out who's an active researcher: just look at their publications. Exactly what level of activity counts as "active" is an issue" (Distler, 2006). In parallel to concerns over institutions, questions about permanence were also asked: should there be a threshold of a certain number of papers that, once passed, confer upon the author the status of active researcher? The alternative measure that was proposed was a rate of publications per year. However, the concept of a minimum rate of publications raised further questions regarding length, quality, open access, peer review and strength of the publishing journal. Distler claimed that Woit was not a borderline case by posting a link to his arXiv publications, but that claim was later disputed by Woit who pointed to his peer reviewed publications listed on SPIRES (now inSPIRE).[84]

These concerns are hardly revelatory to those who study the history and sociology of science and who are well acquainted with a conception of the norms of scientific community as contingent and negotiated. However this case study reveals an example of a scientific community openly struggling with forming stable concepts, so as to prevent certain individuals from contributing. It is interesting to

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[83] See (Crane, 1967).

[84] inSPIRE is a high energy physics literature database built by European Organisation of Nuclear Research (CERN), Deutsches Elektronen-Synchrotron (DESY), Fermi National Accelerator Laboratory (Fermilab) and Stanford Linear Accelerator Center (SLAC).

note that, in contrast to the traditional demarcation debate in the philosophy of science, the debate in the high energy physics community has focused on the individual rather than on output (i.e. can individual 'A' be demarcated as an active researcher versus can the theory 'B' be demarcated as falsifiable). Perhaps this differing focus can be attributed to the difference between the actors in each debate. In the case of the string wars, each participant would like to secure both the legitimacy of his or her claims as well as citizenship of the authoritative group. Gieryn has described similar controversies as "second-order cartographic squabble[s]" about "who really has the epistemic authority to map science" or, in this case, about who has the epistemic authority to label scientists (Gieryn, 1999, p. 28). This is speculation but if this argument were to hold it would also go some way towards explaining part of the vitriol of the 'science wars' which also featured a controversy that questioned individual's citizenship of a group with authority to speak about science.

Despite the controversy surrounding how the high energy physics community can identify 'active researchers' and 'crackpots', there remains consensus both that the categories exist and that it is important that some kind of segregation should exist. Furthermore, despite the lengthy discussions about how to determine 'active researchers' and 'crackpots', there is a pervasive idea of 'we know it when we see it' and similar expressions are used repeatedly in the high energy physics community. These are often extended to embrace the collective, such as 'we usually know it when we see it' (Carroll, 2006a) . Coupled with unanimous agreement that there should be restrictions as to who may have access to the trackback feature, the community is committed to the existence of crackpots, or at the least 'inactive researchers', who may be identified by the community and should not have access to the full spectrum of scientific discourse.

5.9 Blogs are ephemeral?

While blogs have been placed neatly into the category of ephemeral scholarly communication (a characterisation which seems intuitive for at least blog discourse containing vitriolic content), the case study examined here offers counter examples of certain blog texts that do not neatly fit into the ephemeral category. Importantly, the arXiv trackback feature itself demonstrates that the various attempts to characterise blogs as ephemeral fail to recognise the role blogs played in certain spheres. Once a trackback was listed on the abstract page at arXiv, it remained there so that readers of the paper had permanent links to the blog commentary. This is not to suggest that this case study argues that all blog posts are permanent, or are as permanent as traditional print media such as journal papers and books. Rather, it demonstrates there is some evidence which supports the conclusion that some blog discourse within a select community had more than a 'transitory existence'. However, it also shows that the trackback feature, which in part facilitated the semi-permanence of certain blog discourse, is contested and subject to controversy. This case study demonstrates that to characterise the blog discourse as ephemeral is to misunderstand its contested functionality within the high energy physics community.

Not only do the blogs have some degree of permanence, they are also public and the authors are aware that anyone from the *New York Times* to a graduate student attempting to determine his or her career path is reading these blogs. This awareness leads many authors to guard themselves from anticipated criticism by drawing on support from leaders in the field or canonical references. These actions give some posts the appearance of a finalised product. In a sense a blog post mirrors a conference presentation: it has a (mostly) static, public opening section by one author which is followed by questions and comments from a variety of individuals. In anticipation of criticism from peers during questions and comments, conference presenters often attempt to fortify the static part of their presentation. The case study also reveals that certain blog authors use their blogs to publish official announcements from academic institutions.

However, blogs do not fit into the category of permanent scholarly communication. While authors write so as to protect themselves from anticipated future criticism, each blog post is open-ended and in certain cases becomes a work in progress as new information and comments are received. Furthermore, this ability to respond to feedback, particularly though the author's ability to respond to comments, is evidence for the interactive nature of certain blog posts. Consequently unlike permanent scholarly communication such as journal articles and books, blog posts may evolve in response to criticism and feedback and contain the most up to date information.

5.10 Providing a form of 'public peer review'?

While high energy physics blog authors may be reluctant to make novel positive contributions on the public forum of a blog due to a lack of an institutionalised credit mechanism, this does not prevent the making of novel negative contributions such as critique and analysis of results. A recent example[85] was the blog community's response to the results of the BICEP2 experiments. The BICEP2 team, based at Harvard, published its results on arXiv on March 17 2014 (Ade, Aikin, Barkats, Benton, Bischoff, Bock, Brevik, Buder, Bullock, & Dowell, 2014) and submitted the same paper to *Physical Review Letters* for review. The arXiv paper generated a significant amount of buzz throughout the astrophysics and high energy physics communities and it was speculated that this was potentially a Nobel Prize winning discovery. On Monday, 12 May, a French blog, 'Resonances', published a blog post titled 'Is Bicep2 Wrong' (Falkowski, 2014) and delivered a review of the results and claims outlined in the arXiv paper. This blog post was linked to the abstract page at arXiv by a trackback (at the time of writing there were also nineteen other trackbacks on the abstract page). The peer reviewed

[85] Shema et al. (2012) have also described an interesting example of an online preprint published by *Science* being subject to review by members of the scientific community, through both blogs and Twitter, before *Science* could publish the technical comments. One comment from the blogger was then published by *Science* in the technical comments.

paper did not appear until 19 June and contained significant revisions that addressed, although did not mention, the criticisms outlined at Resonances (Ade, Aikin, Barkats, Benton, Bischoff, Bock, Brevik, Buder, Bullock, Dowell, et al., 2014). Indeed, when the claims were first outlined by Resonances, the BICEP2 team denied that any changes were necessary (Grossman, 2014). Yet, responses to the claims made at Resonances were found in the peer reviewed paper.

Arguments for blogs to perform the function of public peer review were present from the very origins of the trackback feature (Distler, 2002b). The trackback feature, as instituted on arXiv, was explicitly designed to facilitate peer review. In contrast to traditional blind peer review, the trackback feature supports public peer review with both the content and author of the review being made public. The semi-permanence achieved through archiving and linking practices further provides blog discourse with the opportunity to perform a function akin to public peer review. The semi-ephemerality of an evolving medium in which text (both the commentary section and the main body of the blog post) is altered also provides blogs with the opportunity to perform a function akin to public peer review. The interactive nature of the commentary section allows for rapid critique and response that is staggeringly faster than traditional peer review (as was the case with BICEP2). This enables blogs posts to develop raw information quickly and this information is archived and linked back to the original paper.

While there are therefore promising indications that blog discourse may provide an alternative or complementary form of public peer review within certain communities, this case study highlights the complexity of a community negotiating the propriety of intellectual practices. Although this paper has argued that to simply characterise certain kinds of blog discourse as ephemeral is untenable, it does not take the extra step and argue the general statement that blog discourse is permanent. As is apparent from the methodical challenges faced by this study in using blog posts as source material, blogs are to a certain extent transitory. The kind of blog discourse examined in this case study is to a certain extent both permanent and ephemeral. These terms employed above, semi-permanence and semi-ephemerality, are clunky and inexact. This speaks to the need for a better understanding of web 2.0 communications as a form of scholarly communication. Furthermore this paper also calls for more work in order to better understand blog discourse, pointing to blogs as an excellent medium in which to investigate the tension between permanence and ephemerality.

Methodological challenges aside, this case study demonstrates that the most significant impediment to blog discourse performing the function of public peer review was a lack of consensus within the high energy community over the trackback feature and the definition of crackpots and active researchers. We can see, in the controversy that emerges, that there is unanimous agreement that not everyone is a ‘peer’ but almost *no* consensus as to who should be deemed a peer. This amounts to a rejection of ‘the wisdom of the masses’ in favour of the wisdom of the few.

Recently, Collins has claimed that “there has been little systematic work on how experts reject what they consider to be maverick claims … we do not have any systematic information about how maverick claims are treated by different groups of scientists” (Collins, 2014b, p. 723). This case study, while not a systematic study, adds to the body of work that attempts to describe how a dominant group of scientists deal with ‘maverick’ or ‘crackpot’ claims, and it highlights the complexity of these attempts. This question has both a practical component, and this case study provides a concrete example of an attempt at dealing with ‘maverick’ claims, and a deeply theoretical component, in that protagonists in the case study are unable to successfully define a ‘maverick’ or ‘crackpot’. Ultimately, the case study reveals the contested nature of certain forms of scholarly communication and potentially new forms of peer review. More research, including a wide spread quantitative study of blog discourse, arXiv trackbacks and arXiv preprints, which recognises the complex nature of blog discourse, is needed to understand this phenomenon.

Chapter Five: Contested methodologies

"My phenomenological friends tend to laugh at our habit in string theory of having these big sessions where we sit around and talk about our feelings"

Eva Silverstein

Panellist 'The Next Superstring Revolution' Strings05

Introduction

In a public lecture on the pedagogical value of black holes, Andy Strominger closed with a brief appraisal of string theory (Strominger, 2010). Strominger attempted a measure of appraisal of string theory and delivered a report card where he awarded string theory a series of 'grades' for a list of desired attributes (see Figure 5.1 in section 1.4). What is interesting is that Strominger declared that the grades were uncontroversial and that any individual with some knowledge of string theory would agree, give or take a little, with each grade awarded. However, Strominger claimed that, despite this agreement over grades, individuals would disagree as to whether this report card was a pass or fail. Strominger concluded in the following way: "I would just like to comment that [string theory is] the only student in the class and if you flunk her you have to shut the school down" (Strominger, 2010). Two of the more polemical figures in the string theory debates took to their respective blogs to comment on Strominger's report card, each confirming Strominger's characterisation of the situation. It is useful to compare the comments of ardent string theory supporter Lubos Motl and critic of string theory Peter Woit side by side:

> "I think Andy is right that people would agree with the grades; they would disagree with whether it is a passing or failing report card. However, as Strominger emphasizes, string theory is the only student in the class. ;-) If you flunk her, you have to shut the school down." (Motl, 2010)

> "I think Strominger is right that his grades and point of view about string theory are now conventional wisdom among leading theorists. What I find striking about this is the argument that if you are forced to give up on string theory, you have to "shut the school down" … More than 25 years of working on string theory has left Strominger and others somehow believing that there is no conceivable alternative. The failure of string theory as a theory of particle physics leads them to the conclusion that they must not abandon string theory … The obvious conclusion that string theory is just one speculative idea, and that its failure just means you have to try others, is one that they still do not seem willing to face up to." (Woit, 2010)

Not only is the commentary a striking confirmation of Strominger's characterisation of his appraisal of string theory, it also highlights the locus of one aspect of the debates over string theory: where holistic appraisals of string theory diverge despite agreement as to particular characteristics of string theory.

This chapter will examine the debates concerning string theory and methodology. In particular the chapter will examine methodological virtues and the role methodological virtues played in determining divergent assessments of string theory. By way of introduction into the constraining role that methodological virtues may play, the chapter begins with an outline of the role of renormalizability – an undisputed methodological virtue. As we shall see, participants in the debates over methodological virtues are often in agreement with each other as to the current status of string theory with respect to each virtue examined. I argue that the debate is located at the level of divergent positions as to how a methodological virtue should constrain theory construction, selection and appraisal. In the first section I will examine the roles of consistency, background independence, non-perturbative formulations and applications where there is both uniform agreement as to the current status of string theory and commitment to each methodological virtue, and where the disputes are located in precisely how each virtue should be interpreted. The second section will examine the role of anthropic reasoning and uniqueness and, in contrast to the first section, I argue that the debate centres on commitment or abandonment of the methodological virtues.

The debates over methodological virtues centre on the relationship between non-empirical evidence and theory assessment. For example, below we see Smolin set up his rationale for assessing string theory with non-empirical evidence:

> "String theory was invented to solve certain theoretical puzzles. Even absent experimental test, we might be willing to support a theory that provided convincing solutions to outstanding problems ... It is thus fair to assess string theory by asking how well it does this." (Smolin, 2006b, p. 179)

Despite disputes as to how to interpret non-empirical evidence in assessment of theories, these disputes do not necessarily imply a rejection of empirical evidence in some form. For many individuals there is a belief that commitment to certain methodological virtues will result in progress towards the 'correct' theory of quantum gravity, whereby it is inevitable that some form of empirical evidence in support of theory will be found. It is considered likely that the empirical evidence will be from an unexpected source, and recently the expectation is that the evidence will come from cosmological observations. At the Strings 2014 conference, in his vision talk, Strominger outlined what he believed to be string theory's relationship with experiment:

> "String theory is good for many things, but in my opinion is highly unlikely to make predictions for accelerator experiments. I would not take this as the defining goal of our field. At the same time, BICEP2 has vividly reminded us, not withstanding dusty bumps in the road, quantum gravity is an experimental science." (Strominger, 2014)

The rationale behind the belief that an observation may come from cosmology is that higher energies are available in cosmology so more likely to offer indirect evidence of theories of quantum gravity.

Constraining methodological virtues

As discussed in section 1.1 of chapter one, Rickles discussed the role of methodological virtues in the historical development of theories of quantum gravity in ‘Quantum gravity meets & HPS’ (Rickles, 2011b). He argues that the methodological virtues that guide research in quantum gravity have not been empirically based and yet the history of quantum gravity is full of failed theories, asking “if not the standard methodological virtues, what is guiding theory constructions and selection is this case” (Rickles, 2011b, p. 4). Rickles argues that in order to answer this question it is useful to draw upon Galison’s notion of constraints (Galison, 1995a). For Galison, constraints play a role in shaping a research program: “To a large extent, and across many domains of science, constraints are constitutive of the positive research program. They create a problem domain, giving it shape, structure, and direction” (Galison, 1995a, p. 22). Rickles argues that

> “Constraints are very much the life-blood of science. They minimize the latitude one has in theory construction. The satisfaction of constraints can in itself act as an evaluative measure. In the absence of experiments and observation, new kinds of constraints must come to the fore, to guide theorizing.” (Rickles, 2011b, p. 35)

Rickles’ observation is that in the history of attempts of quantum gravity, in the absence of experiments and observation, it must be constraints that are guiding theory construction, selection (and rejection).

Both Rickles and Galison draw on a quotation from Weinberg in their discussion of constraints:

> “[I]t seemed to me to be a wonderful thing that very few quantum field theories are renormalizable. Limitations of this sort are, after all, what we most want; not mathematical methods which can make sense out of an infinite variety of physically irrelevant theories, but methods which carry constraints, because these constraints may point the way towards the one true theory. In particular, I was very impressed by the fact that [Quantum Electrodynamics] could in a sense be derived from symmetry principles and the constraint of renormalizability; the only Lorentz invariant and gauge invariant renormalizable Lagrangian for photons and electrons is precisely the original Dirac Lagrangian”. (Weinberg, 1980, p. 517) quoted in (Galison, 1995a, p. 22) and (Rickles, 2011b, p. 36)

For Galison, this quotation is taken as evidence for the “tremendous positive role of the theoretical constraint in defining the field of inquiry” (Galison, 1995a, p. 22). Rickles examines the particular role of renormalisability in string theory, arguing that within the context of a history of failed attempts at developing a renormalisable theory of quantum gravity the role played by the constraint was theory selection: “string theory was given credence because it offered the prospect of a finite theory” (Rickles, 2011b, p. 36). This view is supported by string theorist, Conlon, in a paper where Conlon argues that “one of the great appeals of string theory is that it solves this problem [the non-renormalizability of gravity]” (Conlon, 2006, p. 122). Conlon argues that the appeal of string theory comes not only from the ability to solve the problem but also from the prior belief that the problem of the non-renormalizability of gravity “made the study of quantum gravity apparently impossible” (Conlon, 2006, p. 129). Both Rickles[86] and Galison historically situate constraints; for Galison, the primary historical-philosophical questions are “how do these constraints arise, what sustains them, how do they act, and what makes them fall?” (Galison, 1995a, p. 14)

If we return to the aforementioned quote we can see that Weinberg identifies the constraint of renormalisability as a potential indicator that a theory is “the one true theory” or at least that a theory has made progress towards “the one true theory” (Weinberg, 1980, p. 517). Galison remains noncommittal as to what he calls “Weinberg’s faith in the existence, or even the approximation, of ‘one true theory’” (Galison, 1995a, p. 22) as he is more concerned with processes, in this instance the derivation of QED. For Rickles, like Weinberg, the concern is more epistemic, which may be identified by the later work ‘AdS/CFT Duality and the Emergence of Spacetime’ in which Rickles identifies a methodological virtue, the identification of core structure, which Rickles argues provides a methodology for scientific discovery for dual theories: identify common structures between theories or structures and then try to understand this common structure via another deeper, broader theory that admits of multiple representations (Rickles, 2013a, p. 320).

This chapter will argue that certain debates over string theory may be framed usefully as debates over methodological virtues. This strategy is useful in that it exposes that the debates over methodology and string theory are not concerned with appraisals of a ‘final theory of everything’. Instead, this strategy exposes the many contested methodological virtues and debates. This chapter will argue that the debates occur at the level of contesting in particular ways how methodological virtues should constrain theory construction, selection and appraisal.

[86] Rickles has a detailed description of the role of a variety of constraints in various theories of quantum gravity in *Quantum Gravity a Primer for Philosophers* (Rickles, 2008a).

1. Undisputed constraints

1.1 Consistency

Consistency is a powerful, if not the most powerful, motivator in quantum gravity research. The problem of quantum gravity can be considered to be a consistency problem whereby current understanding is deficient (in that general relativity is not consistent with quantum field theory). Woit frames the problem of quantum gravity explicitly along these lines: "Still, there is a gravitational field and, for consistency with the rest of physics, one would like to treat it using quantum field theory" (Woit, 2006d, p. 9) and then later: "this is the problem of quantum gravity: how does one find a consistent quantum theory for which general relativity is a good classical physics approximation?" (Woit, 2006d, p. 101) It is difficult to overstate the credence given to consistency constraints; as such it is unsurprising that the claim that string theory is a consistent theory of quantum gravity is an oft cited argument in support of or in defence of string theory. As Tong states in an introduction to string theory (written for graduate students):

> "Our current understanding of physics, embodied in the standard model, is valid up to energy scales of 103 GeV. This is 15 orders of magnitude away from the Planck scale. Why do we think the time is now ripe to tackle quantum gravity? Surely we are like the ancient Greeks arguing about atomism. Why on earth do we believe that we've developed the right tools to even address the question? … *the most compelling argument for studying physics at the Planck scale is that string theory does provide a consistent unified quantum theory of gravity and the other forces*." (Tong, 2012, p. 8) (emphasis added)

That theories should be constrained by consistency is not a controversial claim. Whilst consistency may be considered to be a methodological virtue for theories, and more on that later (inconsistency renders a theory nonphysical[87]), the controversy over consistency in string theory refers to sufficiency of consistency to constrain a theory such that it will pick out *the* theory of quantum gravity. Alternatively, it is argued that consistency is sufficient to constrain the process of theory construction such that progress will occur.

1.1.1 Arguments for the sufficiency of consistency

In the face of the experimental difficulties of *any* theory of quantum gravity, some argue that theoretical criteria will lead them to a solution to the problem of quantum gravity. Utilising an argument of an inference to best explanation, theorists such as Weinberg claim that consistency and rigidity are sufficiently constraining such that any theory that held these properties had to be saying something about the 'real world'; "[string theory] has the kind of rigidity that you look for in a kind of physical theory that will in the end turn out to have something to do with the real world" (Weinberg

[87] Although nonphysical does not necessarily imply non-useful; a famous example being the Bohr model of the atom.

quoted in (Galison, 1995b, p. 386)). In his book, *Dreams of a Final Theory*, Weinberg argues that such a theory will be "logically isolated" because any slight change would destroy the consistency of the theory, and that "we would know on the basis of pure mathematics and logic why the truth is not slightly different" (Weinberg, 1993, pp. 236-237). Schwarz has also argued along these lines: "I believe that we have found the unique mathematical structure that consistently combines quantum mechanics and general relativity. So it must almost certainly be correct" (Schwarz, 1998, p. 2). Greene argues that this "unification utopia" (Greene, 1999a, p. 183) would result in a theory that "would declare that things are the way they are because they have to be that way" (Greene, 1999a, p. 283). Here the combination of internal theoretical consistency and rigidity is argued to be sufficient to determine the 'correct' theory of quantum gravity.

Rather than arguing that consistency may be sufficient to uniquely determine the 'true' theory of quantum gravity, Susskind argues that consistency may be sufficient to determine that progress has occurred towards a theory of quantum gravity:

> "String theory has had a profound, and I believe lasting, influence on how gravity and quantum mechanics fit together. In order to illuminate the conceptual problems of quantum gravity it may not be important to discuss the precise form of the theory that describes our corner of the universe. What may be more important is to know what is, and what is not consistent; what kinds of things are possible; what kinds of structures to expect. One should not underestimate the importance of having a mathematically consistent structure that contains both quantum mechanics and gravity" (Susskind, 2013, p. 176).

For Susskind, that which is important is mathematical, alternately described as internal, consistency and this is sufficient to determine part of the correct picture of a theory of quantum gravity.

1.1.2 Arguments against the sufficiency of consistency

Rickles has also argued that historically in quantum gravity research consistency has been compelling in convincing theorists to pursue particular theories. Here Rickles discusses the rationality of accepting unobservable extra dimensions as motivated by consistency arguments, beginning by examining Einstein's opposition to such:

> "It is anomalous to replace the four-dimensional continuum by a five dimensional one and then subsequently to tie up artificially one of these five dimensions in order to account for the fact that it does not manifest itself" (Einstein, 1931, p. 438)

> "… is this good reasoning? In most other situations no doubt it would; it is a simple application of a principle of parsimony or simplicity. But quantum gravity is all about consistency, and if the only way to get a consistent theory is to postulate extra dimensions then should we not accept them?" (Rickles, 2008a, p. 317).

In contrast to the arguments presented by Weinberg and Greene, Rickles argues for the *rationality* of accepting a theory on the basis of consistency rather than the sufficiency of such arguments to determine the 'true' theory of quantum gravity.

Rickles also argues against the sufficiency of consistency arguments to uniquely determine a theory of quantum gravity: "[internal consistency does] not appear to be sufficiently stringent to uniquely determine the desired theory of quantum gravity; instead there are multiple research avenues that each seem to satisfy the constraint" (Rickles, 2008a, p. 264). Likewise, rather than opposing consistency as constraining methodological virtue in his criticisms of string theory, Rovelli offers a series of criteria by which the success or failure of string theory (and loop quantum gravity) may be judged: "completeness, internal consistency, full agreement with known low-energy physics, simplicity, and, ultimately, experience, will tell" (Rovelli, 2013, p. 19). Rovelli argues that consistency is necessary but not sufficient.

1.2 Background independence

Background independence refers to a property of relativistic theories in which the spacetime metric can be obtained as a solution of the dynamical field equation. General relativity is regarded as a 'background independent' theory, in this sense, because the field equations can be formulated without reference to any particular spacetime coordinate system. Just as with the constraint of internal consistency, physicists generally agree that a quantum theory of gravity should exhibit background independence. However they disagree about exactly how we should interpret this constraint on theories of quantum gravity. Beyond an intuitive notion, that there is no agreed upon technical definition of 'background independence' (Pooley, Forthcoming; Rickles, 2008b; S. Weinstein & Rickles, 2015).

There is a sense in which the constraint of background independence may be understood under the umbrella term of external consistency. External consistency is where a theory of quantum gravity is expected to be consistent with well-established theories. In this case the constraint of background independence may be interpreted as an expectation that a theory of quantum gravity must be compatible with general relativity. The difficulty is in deriving 'precise consequences' of this expectation. In 2001 't Hooft argued for a "physics without experiments" and provided a list of constraints (is his words "tests of the following kinds" ('t Hooft, 2001, p. 2898) for building theories without experiment. The third in that list was that:

> "The theory should agree with older theories that are well-established. Thus, most advanced particle theories such as string theory, M theory and the like are demanded to agree at least with quantum mechanics, and special and general relativity". ('t Hooft, 2001, p. 2898)

This broad commitment to external consistency, or a correspondence principle, is not considered to be problematic: as a generalised expression it is unlikely that the constraint will generate dispute; difficulties arise in particular cases in understanding how the constraint should guide theory construction and evaluation. In what follows it becomes clear that what is disputed is not whether or not theories should be constrained by background independence but how to evaluate whether or not the constraint has been met, as well as the significance of the constraint.

Smolin identifies background independence as the most significant of constraints in developing a theory of quantum gravity:

> "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. We understand string theory in terms of strings and other objects moving on fixed classical background geometries of space that don't evolve in time. So Einstein's discovery that the geometry of space and time is dynamical has not been incorporated into string theory." (Smolin, 2006b, p. 184)

For Smolin, the significance of background independence is that it is a determinant of a successful theory of quantum gravity.

In his response to Smolin, Polchinski argued that Smolin had not understood the way in which background independence should constrain a theory:

> "[Smolin] is mistaking an aspect of the mathematical language being used for one of the physics being described. New physical theories are often discovered using a mathematical language that is not the most suitable for them. This mismatch is not surprising, because one is trying to express something that is different from anything in previous experience." (Polchinski, 2006, 2007a)

It is evident that for Polchinski, just as for Smolin, in the process of theory construction the methodology should be constrained by background independence. Where they differ is in their appraisal of the capacity of string theory to satisfactorily meet the constraint. Polchinski continues: "In string theory it has always been clear that the physics is background-independent even if the language being used is not, and the search for a more suitable language continues" (Polchinski, 2006, 2007a). Polchinski is suggesting a split between the representation of string theory and the 'reality' of string theory. On the basis of this split Polchinski advocates for theory construction to progress by developing a new form of representation. As Polchinski points out, this method of theory construction is not new: theorists build theories with the best available tools at hand (mathematical formalisms) and proceed by a manner of hunches and intuitions developing better tools over time.

Polchinski's account of the *process* of theory construction, where the representation of the physics approaches the physical intuition that the theorist has over time, is contradicted by Rovelli. In 2001

Rovelli argued that the historical insights provided by the introduction of no absolute motion as part of the ontology of general relativity should be considered to be instructive (Rovelli, 2001, pp. 105-109). As such Rovelli criticises the path followed by perturbative string theory, and the approach that Polchinski would outline years later, as in his view it does not follow the insight provided by general relativity. Instead he argues that "right way to go" is to attempt to formulate a background independent theory from the outset rather than "hope" to recover general relativity "down the road" (Rovelli, 2001, p. 109). Rovelli extended this argument in 2013, claiming that the issue is that that background independence is "not yet properly understood" by string theorists (Rovelli, 2013, p. 12). The problem, for Rovelli, is: "in all these cases, instead of addressing the real problem, which is to learn how to do physics where background spacetime plays no role, the strategy is to try to circumvent the problem" (Rovelli, 2013, p. 12). Unsurprisingly Rovelli argues that the strategy employed by loop quantum gravity is superior, an in his view, as the problem is addressed "upfront", resulting in a "conceptually clear, fully general relativistic, and well defined" picture of quantum gravity (Rovelli, 2013, p. 13). Here Rovelli attempts to shift the discussion to epistemic appraisal: "Let's not talk about hopes, let's talk about achievements" (Rovelli, 2003a, p. 1512).

1.3 Non-perturbative formulation

Closely related to the debate over background independence is the debate over a non-perturbative formulation of string theory. String theorists argue that while perturbative string theory is background dependent, there are good reasons to believe that the non-perturbative formulation of string theory is background independent. Perturbative string theory is, strictly speaking, not a theory in its own right, but rather a background dependent *method* that allows quantitative calculations of certain aspects of the theory. The difficulty, as both string theorists and critics point out, is that the dynamical equations of this fully formulated (non-perturbative) theory "are so complicated that no one knows their exact form" (Greene, 1999b, p. 285). Polchinski and others point to dynamical features, such as topology change of perturbative string theories, which would seem to indicate that a non-perturbative formulation is background independent (Polchinski 2006). Prospects for a fully non-perturbative formulation of string theory began to look better during a period of time labelled the 'second string revolution' in the late-1990s, with a deeper understanding of the dualities that relate the five known perturbative string theories (Polchinski, 2004). The AdS/CFT duality, first proposed by Jan Maldacena in 1997, provided physicists with what is believed to be a fully non-perturbative definition of string theory in anti-de Sitter spacetime (J. Maldacena, 1997). Further developments, such as the 'holographic principle', also raised hopes of the possibility of a background independent formulation of string theory.

Crucially for some, such as Smolin, Woit,[88] and 't Hooft, the duality relations (in particular the AdS/CFT duality) are yet to be convincingly proven. For Smolin: "there is evidence to support something like the Maldacena conjecture, but no proof of the full conjecture itself, and only the full conjecture will allow us to assert the existence of a good quantum theory of gravity" (Smolin, 2006b, p. 191). 't Hooft criticises perturbative string theory for "not defining a theory" ('t Hooft, 2013, p. 50). While the duality relationships are identified as 'artillery' against the lack of a non-perturbative formulation of string theory, ultimately 't Hooft finds the strength of this 'artillery' inadequate where the string theories lack "rigorous foundation" ('t Hooft, 2013, p. 50). Here the disagreement rests on the extent to which the duality relations may be considered to support a belief in the existence of a non-perturbative formulation of string theory.

In his review of TTWP and NEW, Polchinski challenged the negative appraisal of string theory where the appraisal is based upon the idea that the duality relationships do not secure the knowledge of a non-perturbative formulation of string theory. Rather than challenge Smolin and Woit's description of the duality relations as incorrect or misunderstood, instead Polchinski challenged Smolin and Woit's understanding of methodology. In particular, Polchinski argued that Smolin and Woit's negative appraisal was dependent on a flawed interpretation of how methodology should constrain theory construction (or progress):

> "Physicists work by calculation, physical reasoning, modelling and cross-checking more than by proof, and what they can understand is generally much greater than what can be rigorously demonstrated. For example, quantum field theory, which underlies the Standard Model and much else in physics, is notoriously difficult to put on a rigorous foundation. … Physicists by their methods can obtain new results whose mathematical underpinning is not obvious. String theorists have a strong sense that they are discovering something, not inventing it." (Polchinski, 2006)

Polchinski is advocating for a methodology of theory building that is not, currently, constrained by experiment. Despite this he compares this methodology to the one employed in partially experimentally determined theories. The difficulty here is how to interpret theoretical 'evidence'. Constraints are argued to be evidence for a variety of claims in that they are considered to be indicative of rational belief in a theory, future likelihood of success or the 'promise' of an approach, as well as the 'truth' of theory.

It seems trivial to reduce these debates to those that are 'optimistic versus pessimistic' where the very issue of appropriate scientific judgement is at stake. There is a noncontroversial sense in which scientists make many non-empirical appraisals of theory. These decisions range from very high level, choosing a speculative theoretical approach to develop, to more day to day decisions concerning

[88] Woit examines this issue at length in NEW (Woit, 2006d, pp. 182-188).

choosing an experimental set up. The scientist will have some measure of confidence in the results, as is evidenced by their investment of time and resources (a particular concern for experimental physics), and this confidence cannot come from empirical results. This confidence is not that the results will be ‘correct’ or provide confirmation; instead it is confidence that the choices made will generate a result with utility. Indeed a disconfirmation may be considered of more valuable in theory construction as was the case with the experimental determination of the mass of the Higgs Boson.[89] Non-empirical assessments of the potential utility of an approach are part of many practical decisions made by scientists on small and large scales. In the case of the debates over string theory, it is interesting to see the role of constraints clearly articulated and debated in this decision making process.

1.4 Applications

If we return to Strominger’s report card (Figure 5.1), from the introduction, we can see that Strominger awarded string theory high grades in two sections devoted to applications: an A for inspiration for pure mathematics and a B for inspiration for other areas of physics (Strominger, 2010). Again, recalling the introduction where two of the most polemical figures in the debates over string theory both agreed on their own respective blogs that Strominger was correct in his assertion that very few would disagree with the grades awarded (Motl, 2010; Woit, 2010).

Again we see there is widespread agreement in a positive assessment of string theory as a tool. Earlier, Woit had also written in praise of the wider applications of string theory: “While supersymmetry and string theory have been remarkably unsuccessful so far in explaining anything about physics, they have led to a great deal of new and very healthy interaction between the fields of mathematics and physics” (Woit, 2006d, p. 193). Mike Duff also drew attention to what he called “applications outside the traditional ‘theory of everything’ milieu that one normally associates with string and m-theory” (Duff, 2013, p. 195).

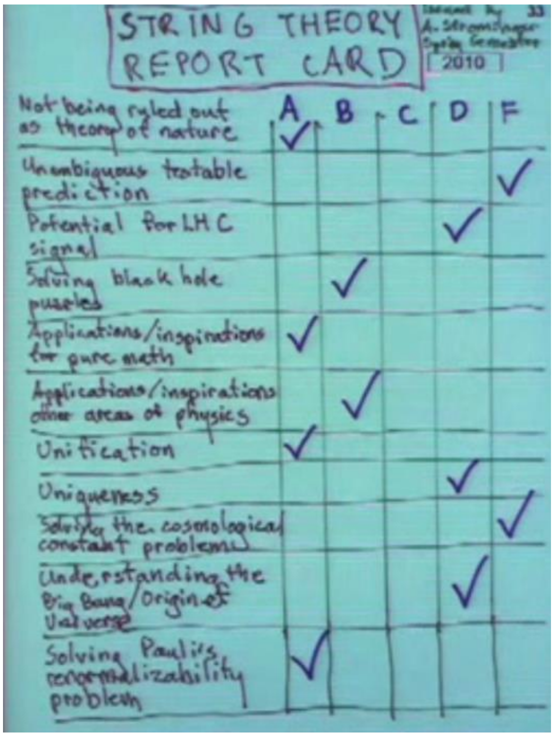

Figure 5.1 ‘String Theory Report Card’ Source: (Strominger, 2010)

Despite the apparent agreement in the utility of string theory where there have been many unexpected applications in both mathematics and physics, there remains disagreement as to how the relationship between tool and TOE string theory should be understood.

[89] In the lead up to the ‘discovery’ of the Higgs boson, what became known as the ‘nightmare scenario’ was discussed by theorists. The nightmare scenario was if the Higgs Boson was to be found to have properties as predicted, a ‘vanilla Higgs’. This would be a ‘nightmare’ as no hints for beyond the Standard Model physics would be provided.

This relationship is argued by some to be an evidential relationship where the successes in string theory as a tool are argued to be evidence for TOE string theory, string theory as a unified theory of quantum gravity. As discussed in section 1.2 of chapter one, Rickles has argued that for many string theorists "the success of the mathematical predictions are seen as evidence for the framework that generated them" (Rickles, 2013b, p. 54). By contrast, others such as Woit and Rovelli argue that appraisals of tool string theory are distinct from TOE string theory, and as such progress in tool string theory cannot constitute progress in TOE string theory or indicate that TOE string theory is 'on the right track'.

'Is String theory a Theory of Quantum Gravity?', written by string theorist Steven B Giddings, is arguably the contribution to the special issue most critical of string theory (Giddings, 2013). The ambiguity in determining levels of criticism comes from a lack of consensus as to what amounts to a critique of string theory. For those that pursue string theory exclusively as a useful set of techniques for problem solving, it is not a criticism to deny that string theory is a candidate theory of quantum gravity. For those that see developing a theory of quantum gravity that unifies the fundamental interactions as the primary goal of string theory, questioning if string theory is a theory of quantum gravity amounts to a serious critique.

It is difficult to pin down the exact nature of the appraisals of string theory based on the relationship between string theory as a tool and as a TOE, partially because the appraisals are not static; instead, they evolve in an almost Bayesian way. There are two parts to the appraisal: the current status of the string theory research program and a projective assessment of the likelihood of future success. With each development, the measure of confidence of future successes of string theory updates. As such, many of the arguments are with regards to evidence that string theory is the most promising approach and much of the language is couched in terms of being "on the right track" (Bergman, 2006b; Witten, 2005, p. 1085), similarly "the right and possibly final track" (Greene, 1999a, p. 20) and as philosopher Dawid put it: "it would look like a miracle if all these instances of delicate coherence arose in the context of a principle that was entirely misguided" (Dawid, 2013b, p. 89). This terminology was later used by Duff, who pointed towards the very high number of citations of Maldacena's AdS/CFT paper (J. Maldacena, 1999). Duff located the cause of citation level in that the AdS/CFT conjecture had found application outside "the traditional "theory of everything" milieu that one normally associates with string and M-theory" (Duff, 2013, p. 196).

Quantum field theorist Matt Strassler and Woit had a, at times, furious debate about the relationship between string theory as a tool and as a TOE (see comments on (Strassler, 2013b)), that resulted in Strassler writing an additional six posts in clarification (Strassler, 2013a, 2013c, 2013d, 2013e). Woit accused Strassler of misleading the public by claiming that progress coming from use of string theory as a tool is indicative of progress in string theory as a TOE (which Woit calls string unification). For

Woit, the applications of string theory are tests of an “approximation scheme” as opposed to tests of a theory (Woit commenting on (Strassler, 2013b)). In the second follow up post Strassler argued that Woit has left unexplained “why string theory could be such a helpful tool for a quantum field theorist like me” and that “[b]y studying imaginary particles and forces, we gain insight into the real world” (Strassler, 2013d).

Recently Clifford Johnson was asked if there were any string theorists still working on string theory as a TOE by Sabine Hossenfelder:

> “I have the vague impression that there are not so many people left working on string theory as ‘the theory of everything’ and instead most are now doing AdS/CFT and extensions thereof (dS, time-dependent, etc), dualities in general and applications. Do you share this impression?” (Hossenfelder commenting on (C. Johnson, 2015))

Johnson responded with a detailed explanation which is worth quoting as length so as to examine in detail the many facets of the arguments between tool and TOE string theory:

> “There is a very diverse set of topics within the subject (or inspired by examples that first arose in the subject) that (along with topics like applications to condensed matter and nuclear topics) are all vital explorations of what string/M-theory really is, and what it can teach us about quantum field theory, spacetime, etc. It was clear to me (and I imagine, others) a very long time ago that it was very premature to have the entire field all working on trying to squeeze the theory into one simple (‘theory of everything’) role, and that we needed to diversify and explore it in many contexts (especially connecting with other types of experimental physics) in order to really get to grips with what we’ve got, and what the theory can and can’t do. The benefits are that we (1) Get insights and useful tools for all those different corners of exploration, and (2) We strengthen the program of developing the subject for its application to the (naive, in my view) “theory of everything” quest.” (C. Johnson, 2015)

Rather than arguing that the diverse applications of string theory are indicators of a final theory, Johnson argues that the field is in a healthy state and as such progress is being made. Progress for Johnson is constituted by applying string theory methodologies to areas such as quantum field theory and gaining useful insights. Crucially, Johnson also argues that in applying string theory methodologies there also insights gained into string theory as a theory of quantum gravity and string theory as a TOE is “strengthened”. Earlier he argued:

> “We do not know whether any of these things have anything to do with our world…. that successful quantum gravity conceivably might not turn out to be *our* quantum gravity for example…. but as a list of things where significant progress has been made in so many

> theoretical physics topics by a single framework … you must agree it is very impressive. This is why it is regarded as "promising"." (Johnson commenting on (C. Johnson, 2006f))

Johnson is clear not to argue that the progress occurring in tool string theory is evidence that TOE string theory is correct. Rather, he argues that the successes in tool string theory are deeply connected to TOE string theory and are evidence for TOE string theory as "promising".

In contrast to the position offered by Johnson, Rovelli provides an alternate explanation of the wide application of string methodologies:

> "string theory techniques may have potential applications to other domains of physics. These are very interesting, but in no way they testify in favor of the relevance of string theory for the fundamental interactions. Enormous intellectual investments have gone into string theory in the last decades and it would be strange if all the theoretical technology developed did not turn out to be good for something." (Rovelli, 2013, p. 17)

For Rovelli, the methodological successes of tool string theory are not considered to be evidence for TOE string theory. Similarly Woit argues that the AdS/CFT correspondence is suitably non-realistic that explanatory successes in tool string theory do not constitute either progress or potential progress for TOE string theory:

> "This kind of string theory is well-worth investigating since it may be a useful tool in better understanding QCD, but it just does not and can not give the standard model. The claim of my book is not that string theories are not interesting or sometimes useful, just that they have failed in the main use for which they are being sold, as a unified theory of particle physics and gravity." (Woit commenting on (Polchinski, 2006))

Camilleri and Ritson have argued that much of the disagreement concerns assessments of the promise and future potential of string theory. As Camilleri and Ritson have argued, "[t]hus while the debate appears on the surface to be about justification and epistemic appraisal, much of the disagreement actually concerns heuristic appraisal" (Camilleri & Ritson, 2015, p. 45).

Rickles has argued that the mathematical applications of string theory are significant for conclusions regarding rationality of pursuing string theory. He employs a modified version of the 'no miracles argument' where Rickles is concerned with miracles that are "surprising *mathematical* facts" (as opposed to surprising empirical success in Putnam's original version (Putnam, 1975)) (Rickles, 2013b, p. 56). Recall the summarised modified no miracles argument from section 1.2 in chapter one:

1. In the case where precise quantitative experiments are unlikely (and it is misguided to demand precise quantitative experiments of a theory of quantum gravity).

2. Where it is also the case that a combination of physical constraints and mathematical consistency in string theory has led to mathematical insight.
3. The best explanation for the mathematical fertility of string theory is that string theory is in some sense ‘true’.

Therefore “it is perfectly rational to pursue string theory” (Rickles, 2013b, p. 78).

This style of appraisal is not ‘total’; instead, it is driven by an understanding that appraisals of theories of quantum gravity should reflect the non-empirical methodological virtues that guide theory construction. Rickles argues “having an hypothesis that naturally generates a consistent and widely applicable mathematical framework ought, I argue, to increase our credence in that hypothesis, if only in a relatively small way. In the absence of alternative sources of evidence, then even so small an increase in the credence given to a theory is not insignificant” (Rickles, 2013b, p. 70).

One result along similar lines that deserves closer attention is the derivation of black hole entropy. The dispute over the significance of the result is such that it is difficult to locate it in either tool or TOE string theory.

1.5 Derivation of black hole entropy

In 1996 Strominger and Vafa published a landmark paper titled ‘Microscopic origin of the Bekenstein-Hawking entropy’ (Strominger & Vafa, 1996). This development was considered a key element of the increase in optimism among string theorists that characterised the so-called ‘second superstring revolution’. In the case of the work done by Strominger and Vafa, the optimism was a result of a long standing expectation that a theory of quantum gravity should allow one to calculate the entropy of a black hole[90] (Bekenstein, 1973; Hawking, 1974). As with the renormalisation problem, there is a long history of attempts to understand Bekenstein-Hawking entropy (Unruh, 2001), following Hawking’s discovery that black holes radiate (Hawking, 1974, 1975). Strominger and Vafa showed that for extremal five dimensional black holes the quantum microstates could be counted by hand in agreement with the Bekenstein-Hawking area law. The choice of language to describe this development is difficult as it has been contested by some, such as John Baez (Baez comments on (Bergman, 2006a) and, as discussed in section 1.2 of chapter one, Erik Curiel (Curiel, 2001). Baez and Curiel were critical of the descriptions of the class of black holes utilised in the calculation. Much of this disagreement comes from differing interpretations of the significance of the

[90] In 1974, drawing on the earlier work of Bekenstein, Hawking predicted that black holes radiate energy. The amount of energy radiated would be proportional to the gravitational ‘temperature’ which is also proportional to the mass, angular momentum, and charge of the black hole. The expectation for a theory of quantum gravity is that it would allow a calculation of the of the entropy of a black hole of given mass, angular momentum, and charge where the entropy corresponds to the number of quantum microstates of the gravitational field having the same mass, charge, and angular momentum. Curiel has been critical of this ‘test’ of a theory of quantum gravity as the predicted radiation has not yet been observed, as such Curiel denies that the successful calculation performed by Strominger and Vafa secures the scientific status of string theory (Curiel, 2001).

calculation in the case where the black holes are "unphysical", which is to say that the black holes do not physically exist somewhere in the universe.

There are many testimonials as to the significance of the development. Mike Duff argued that:

> "String and M-theory continue to make remarkable theoretical progress, for example by providing the first microscopic derivation of the black hole entropy formula first proposed by Hawking in the mid-1970s. Solving long outstanding theoretical problems such as this indicates that we are on the right track." (Duff, 2013, p. 184)

There are two potential interpretations of Duff's 'we' here. One is that string and M-theory, as a TOE, is getting closer to the goal of being the correct theory of quantum gravity. This interpretation is given greater credence by the inclusion of 'M-theory', the as yet unknown theory that is believed to be a (or the correct) unified theory of quantum gravity. A second interpretation, is that solving problems is for Duff considered to be evidence for string and M-theory as a *methodology.* Again this interpretation is given greater credence by the inclusion of string theory, as distinct from M-theory. I argue that both interpretations are consistent with Duff's position: Duff is arguing that the calculation is evidence for both string theory as a tool and as a TOE, for both the theory and the methodology being on the right track.

In his review of NEW, Aaron Bergman argued that that "finding the individual states that lead to this entropy has been one of the holy grails[91] of quantum gravity research" (Bergman, 2006b, p. 10). For Bergman the significance of the derivation is also evidence for string theory as a theory of quantum gravity. He argues

> "This is a striking confirmation that, for whatever its other flaws, string theory really is *a* theory of quantum gravity … The significance of this is that, even if string theory turns out to be the wrong theory of quantum gravity, how it solves the puzzles presented by the unification of quantum mechanics and gravity will aid us in understanding and formulating future theories." (Bergman, 2006b, p. 10)

Rather than being evidence that string theory, as understood in 2006, was the correct theory of quantum gravity, instead Bergman argued that the methodology for the successful calculation of black hole entropy for extremal black holes, the "*how* it was calculated" as opposed to the calculation, is instructive for TOE string theory.

Rovelli and 't Hooft, whilst agreeing that the calculation is a success for string theory, disagree as to the significance. Rovelli argues that the derivation is insufficient to differentiate between the two rival

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[91] Bergman's use of the phrase 'holy grail' in his review was also criticised by Baez as misleading: "When Aaron starts using "holy grail" imagery, nonphysicists will think he means the real thing we've been searching for all these years - not just a model we're using as a warmup for the real thing" (Baez commenting on (Bergman, 2006a)).

approaches, string theory and loop quantum gravity, as loop quantum gravity can also claim a partial success. Furthermore for Rovelli it is at best is a "partial" success. Rovelli remains unconvinced that a full solution is forthcoming:

> "The string derivation is still confined to, or around, extreme situations, as far as I know, and since it is based on mapping the physical black-hole solution into a different solution, it fails to give us a direct-hand concrete understanding of the relevant black hole degrees of freedom, as far as I can see." (Rovelli, 2013, p. 16)

For Rovelli, the calculation is a successful application of string theory and not sufficient evidence for string theory to be considered the most promising approach in quantum gravity research.

Where Bergman and Duff saw evidence for a successful methodology, Rovelli saw a methodology couched in avoidance, that rather than solving the problem "the strategy is to try to circumvent the problem" (Rovelli, 2013, p. 12). Along similar lines, 't Hooft also criticised the methodology as unvirtuous and likely to deter progress:

> "[String theorists] appear to prefer to discover more and more new "stringy miracles", such as new miraculous matches of black hole microstates, or new cosmological scenarios. If any logical jumps appear to be too large to comprehend, we call these "conjectures", find tests to corroborate the conjectures, and continue our way. These are easier ways to score successes but only deepen and widen the logical depths that block any true understanding." ('t Hooft, 2013, p. 47)

There is of course a long history in physics of using toy models for calculating complex and difficult scenarios. The difficulty in the case of assembling a theory of quantum gravity, where experiment is unlikely to provide evidence, is in how to interpret theoretical successes as evidence for a theory or a methodology.

1.6 Non-empirical appraisal as guided by methodological virtues

Rather than believing in string theory as a 'final theory' or as the correct theory of quantum gravity, a more prevalent belief is a stance that string theory is the best approach. This is a belief in string theory as constrained by certain methodological virtues. This is an assessment of string theory as most likely to, or unlikely to, deliver future results. For the most part, the appraisal of string theory methodology is an appraisal of a theoretical methodology, as opposed to a methodology bound by experiment. In these debates the disagreement does not centre on experiment versus theory, as is often assumed. Woit, one of the strongest critics, does not oppose string theory for the lack of experimental rigour; instead he opposes it for a lack of mathematical rigour:

> "To mathematicians, what is at issue here is how strongly to defend what they consider their central virtue, that of rigorously precise thought, while realising that a more lax set of

behaviours is at times needed to get anywhere. Physicists have traditionally never had the slightest interest in this virtue, feeling they had no need for it. This attitude was justified in the past when there were experimental data to keep them honest, but now perhaps there are important lessons they can learn from the mathematicians … every effort must be made to achieve precision of thought wherever possible and always to be clear about exactly what is understood, what is not, and where the roadblocks to further understanding lie." (Woit, 2006d, p. 263)

Given that there is agreement that string theory is not a theory, but instead is a collection of insights, techniques and methodologies, it is not at all surprising that a significant dimension of appraisal of 'string theory' concerns the appraisal of methodologies. These disagreements go beyond the appraisal of methodology so as secure the scientific status of string theory. As discussed in chapter two, the disagreement over the scientific status of string theory was not so much concerned with appraisal of methodology; rather, definitions of science were constructed rhetorically by certain individuals in an example of boundary work.

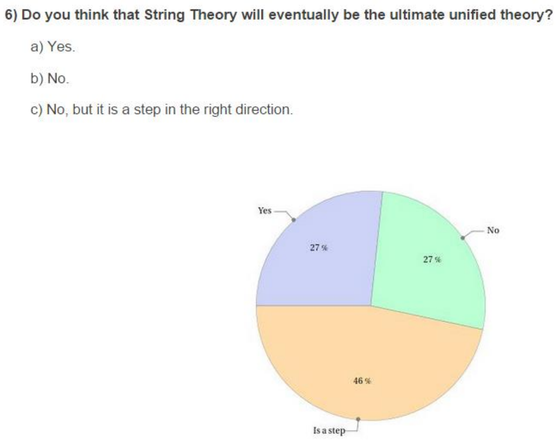

Figure 5.2 'Survey results: 'Do you think string theory will eventually become the ultimate unified theory? ("Survey," 2013)

Whilst not conclusive, a survey conducted at a conference in Madrid (Why mH= 126 GeV?) indicates that, for many, the appraisal of unified theories of quantum gravity is an appraisal of the capacity of a theory ("Survey", 2013). For the majority, rather than appraisal of string theory as a final theory or not, appraisal of string theory occurs as the level of a more or less promising approach. Beyond this survey, there are a large spectrum of responses.

In part one of this chapter, it was shown that those who have felt compelled to speak in the public domain have been divided in their assessments of whether string theory is the most promising approach to constructing a (unified) quantum theory of gravity. Whilst there has been diversity in the arguments presented as to how to interpret methodological virtues, there is a broad, and unsurprising, trend where those with a positive assessment of the promise of string theory, such as Polchinski, Greene, Schwarz, Conlon and Duff, are those working in the field in some capacity. By contrast, those who do not believe that string theory is the most promising approach, such as Smolin, Woit, 't Hooft, Baez, Rovelli, do not. Part two will examine the debates over the legitimacy of anthropic reasoning and the necessity of uniqueness. In these debates, even the string theory community itself is divided.

2. Disputed Constraints

In this section, I will identify points of conflict within the string theory community as to virtue of anthropic reasoning and uniqueness. These two contested virtues are intimately connected, as both are concerned with the landscape scenario and as such are often conflated. However the following will argue that there are two, sufficiently distinct disputes occurring and that there is value in exploring each separately. There is now a burgeoning literature in the philosophy of cosmology dedicated to issues concerning the multiverse interpretations of the landscape.[92] This chapter will focus only on the debates internal to the string theory community with regards to the legitimacy and utility of anthropic reasoning and the necessity of uniqueness.

2.1 Uniqueness and the landscape problem/solution

In 2003 the string theory community began to discuss the multiverse scenario, where it was believed there were a large number of consistent string theories (on the basis of the landscape of 10^{500} or more metastable low energy vacua). As is further discussed in chapter two section 1.9, the multiverse scenario has led some to question whether it is possible, in principle, to develop testable predictions for string theory. This chapter will examine a further point of contention that questions whether uniqueness is methodologically virtuous. The controversy over the necessity of uniqueness in string theory, while not absent, was certainly heightened following 2003. Drawing on the work of what is known as the KKLT paper (Kachru et al., 2003) and Buosso and Polchinski (2000) in 2003 Susskind uploaded a paper to the arXiv, 'The Anthropic Landscape of String Theory' that has since been published in *Universe or Multiverse?* (Susskind, 2003a, 2009). The paper argued for the legitimacy of anthropic reasoning (which will be discussed in the following section) as well as for the rejection of uniqueness as a constraint. Susskind's proposal was a controversial one and was greeted with hostility, not only by some critics of string theory but also by many in the string community who viewed the rejection of uniqueness as a rejection of a long term aim of string theory, quantum gravity, and physics.

2.1.1 The necessity of uniqueness

The search for a unique explanatory framework was uncontroversially one of the driving forces behind string theory for almost three decades. Greene argued that uniqueness was paramount: "the ultimate theory should take the form that it does because it is the unique explanatory framework capable of describing the universe" (Greene, 1999a, p. 283). This view represents what Kragh describes as the "Einsteinian ideal" where the construction of unique unified theory was considered to be a continuation of the work of Einstein (Kragh, 2011a, p. 214). Here we find the virtue of uniqueness is closely related to theory construction. The vision of Einstein invoked by critics, such as

[92] For a general introduction see (Carr, 2009), for a historical introduction see (Kragh, 2009) for an introductory survey (Tegmark, 2009) and for philosophical discussion of the multiverse and string theory focusing on typicality see (Azhar, 2014, 2015; S. Weinstein, 2006).

Howard Georgi, contrasts sharply instead they highlight Einstein at the end of his career – an old man, working on a doomed attempt at unification because he was unaware of the strong and weak forces and placed a mistaken emphasis on the value of elegance and beauty (Galison, 1995b, pp. 392-393). A second significant figure in the debates over uniqueness is Geoffrey Chew, who in the 1960s developed the bootstrap model for the strong interaction and was strongly driven by considerations of uniqueness (Cushing, 1990). Chew was also the PhD supervisor to David Gross[93] who reflected on the influence of his supervisor:

> "Geoff transmitted to us his unique passion for physics. We were not merely doing phenomenology of the strong interactions, but were embarked on a great adventure to find a unique theory of hadrons. Geoff inspired us to think big, to attempt to achieve ambitious goals and in particular to search for uniqueness in physical theories." (David Gross, 1985a, pp. 128-129)

For Gross, the appeal of uniqueness formed a large part of his positive appraisal of string theory in 1985:

> "One of the most exciting features of these string theories, which have the possibility of containing all known low energy physics, is their large degree of uniqueness. If a unified string theory turns out to be correct, it could not only allow us to calculate all of Eddington's fundamental constants but could even determine the number of spatial dimensions." (David Gross, 1985a, p. 136).

The situation in 1985 was strikingly different to the current picture which is dominated by the landscape. In 1985, during the period known as the first superstring revolution, it was believed that there were five distinct and consistent string theories (type I, type IIA, type IIB, and two flavours of heterotic string theory (SO(32) and $E_8 \times E_8$), and it is to these that Gross refers to in the above quotation (David Gross, 1985b).

In 2005, faced with the landscape scenario, one panel at the Strings 05 conference was dedicated to the 'next superstring revolution'. The panel featured eight of the most influential string theorists: Raphael Bousso (UC Berkeley), Shamit Kachru (SLAC & Stanford), Ashoke Sen (Harish-Chandra Research Institute), Juan Maldacena (IAS,[94] Princeton), Andrew Strominger (Harvard), Joseph Polchinski (KITP[95] & UC Santa Barbara), Eva Silverstein (SLAC & Stanford), Nathan Seiberg (IAS, Princeton) and was moderated by Steve Shenker who encouraged the audience to be "impolite". Shenker described the initial pull of string theory's claim to uniqueness: "there was the most amazing sense that quantum gravity was special and unique and it took delicate miraculous mechanics to unify

[93] David Gross would go on to supervise the PhD thesis of Edward Witten.
[94] Institute for Advance Study.
[95] Kavli Institute for Theoretical Physics.

quantum mechanics and gravity. It was really exhilarating. But that sense of uniqueness and distinctiveness has receded" ("The Next Superstring Revolution," 2005). In response to this, the string theory community divided, with some arguing that the necessity of uniqueness as a constraint on a theory of quantum gravity should be abandoned. For those who remained committed to uniqueness, the landscape was interpreted as a solution to an as yet unknown but unique theory (M-theory). For those who argued against uniqueness, the landscape was argued to be populated by distinct string theories.[96] For Gross, there was only one option as he declared in his closing remarks at the Strings 03 conference quoting Churchill: "Never never never never never give up" (David Gross, 2003a).

2.1.2 Arguments against the necessity of uniqueness

Much of Susskind's popular book, *The Cosmic Landscape*, is devoted to a rejection of uniqueness (Susskind, 2005). Susskind characterised the situation thus:

> "During the 1990s the number of possibilities grew exponentially. String Theorists watched in horror as a stupendous Landscape opened up with so many valleys that almost anything can be found somewhere in it... Judged by the ordinary criteria of uniqueness and elegance, String Theory has gone from being the beauty to the beast." (Susskind, 2005, p. 125)

Arguing that uniqueness and elegance are myths that have existed since the time of Pythagoras and Euclid, Susskind has argued that such virtues are more a question of taste rather than necessary criteria for a theory of physics (Susskind, 2005, pp. 118, 111). In a similar vein, Schellekens has argued that, "historically, whenever alternatives were imaginable, the hypothesis of uniqueness has almost systematically been a failure" (Schellekens, 2008, p. 2). Citing examples such as the mistaken belief that the earth is unique in being located at the centre of the universe, Schellekens argues against the classic anthropocentric arguments for uniqueness. He concludes that we should put no stock in the uniqueness of our universe. Indeed Susskind has gone so far as to argue that a lack of uniqueness is actually a virtue (Susskind, 2005, p. 126). Susskind and Schellekens make a historical claim to counter the long held aspirations to uniqueness which they hold to be a mistaken normative virtue.

Susskind defines reductionism as a commitment to a "hierarchy of structure", where "big complicated things are made of smaller simpler things; and that the properties of the bigger things are explainable in terms of the laws governing the smaller things" (Susskind, 2013, p. 177). For Susskind there can be two variations of a reductionist commitment: one where the hierarchy of structure has no end point and one where an end exists and therefore there is a fundamental entity. On this definition of reductionism, Susskind argues that "string theory is telling us that in a deep way reductionism is wrong" (Susskind, 2013, pp. 177-178) and that this is due to ambiguities in string theory where the

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[96] On each interpretation a selection principle is needed to in order to choose a particular location within the landscape that describes our world. In the following section the debate over the anthropic principle, as a potential selection mechanism, will be discussed.

choice of elementary objects is a matter of convenience. Or, as he describes it in the talk, that “which is fundamental and which is composite doesn’t have a unique answer” (Susskind, 2011). Ultimately Susskind’s interest in an anti-reductionist position is expressed as an argument against the necessity of uniqueness.

2.2 Anthropic reasoning and the landscape solution

In Susskind’s 2003 paper, he also proposed that environmental or anthropic reasoning could be used as a selection principle that would reduce the size of the landscape. Susskind argued that “in an anthropic theory simplicity and elegance are not considerations. The only criteria for choosing a vacuum is utility, i.e. does it have the necessary elements such as galaxy formation and complex chemistry that are needed for life” (Susskind, 2003a, pp. 4-5). Following Susskind’s 2003 paper, Smolin wrote a direct response challenging the legitimacy of Susskind’s approach titled ‘Scientific Alternatives to the Anthropic Principle’ (Smolin, 2004). This trigged a series of emails between the two authors, culminating in each writing a single letter to the other that would be published at the same time on the website ‘The Edge’ (Smolin & Susskind, 2004). After the debate between Smolin and Susskind a second debate occurred, concerning the anthropic principle, but on a much larger scale. At the conclusion to the aforementioned panel at the Strings 05 conference there was much discussion of the anthropic principle. During his short presentation, Polchinski argued that the third revolution had already occurred, stating: “Steve and several other people have said to me ‘so Joe, what are you going to say about the anthropic principle’ but I guess I took his instructions too literally to talk about the next superstring revolution because by my count this is the one that has just happened” ("The Next Superstring Revolution," 2005).

As the number of possible values of physical parameters provided by the string landscape increases, “the more string theory legitimates anthropic reasoning as a new basis for physical theories” (Weinberg, 2009, p. 39). On this view, physicists will have to resign themselves to exploring the vast terrain of the string landscape. Weinberg admits that such theories “certainly represent a retreat from what we had hoped for: the calculation of all fundamental parameters from first principles”, but “we may just have to resign ourselves to a retreat” (Weinberg, 2009, p. 39). In a similar vein, Polchinski remarked that “anthropic reasoning runs so much against the historic goals of theoretical physics that I resisted it long after realizing its likely necessity” (Polchinski quote requested for (Roebke, 2005)).

The debate over the anthropic principle is often described as a divide between two styles of physics, East Coast versus West Coast. The two styles are supposedly characterised by institutional affiliation in the United States, where Stanford, on the West Coast, is considered a locus for research utilising the anthropic principle and on the East Coast Princeton, Harvard, and others on the East Coast, remain against such usage. Like many stereotypes this characterisation does not match the messiness of reality, but also contains an element of truth. For one, it is a very US-focused picture of quantum gravity research and does not take into account many high profile individuals who have commented on the issue such as Martin Rees (from the UK), Ashoke Sen (from India) and Lee Smolin (from Canada). It also implies a simplistic divide of 'for' and 'against' when, as I shall argue in the following section, there are a variety of points of conflict concerning alternate positions on the aim of science.

2.2.1 Arguments for the explanatory power of anthropic reasoning

Pre-empting that his suggestion would be, at least initially, unpopular, Susskind outlined his argument for the potential of anthropic reasoning:

> "With nothing preferring one vacuum over another, the anthropic principle comes to the fore whether or not we like the idea. String theory provides a framework in which this can be studied in a rigorous way. Progress can certainly be made in exploring the landscape. The project is in its infancy but in time we should know just how rich it is. We can argue the philosophical merits of the anthropic principle but we can't argue with quantitative information about the number of vacua with each particular property such as the cosmological constant, Higgs mass or fine structure constant. That information is there for us to extract." (Susskind, 2003a, p. 17)

The idea behind the anthropic principle, as applied to string theory is to turn the string theory landscape from problem to solution in order to answer a separate problem: the unexpected value of the cosmological constant. Without a formula to accurately predict the value of the cosmological constant (Quantum Field Theory is off by a factor of 10^{120}), it is instead proposed that, rather than being a constant of nature, the cosmological constant is an environmental constant. This argument claims to avoid having to answer to the questions of apparent fine tuning in the case where it appears that life is only possible for a very precise vacuum energy. This is because anthropic reasoning 'explains' the unexpected value of the cosmological constant because in a space of 10^{500} vacua it is to be expected that life would find itself in a universe with a vacuum energy such that life could exist and that the other universes would remain (mostly) unpopulated as the conditions are not conducive to life. As discussed earlier, Susskind utilises an understanding of explanation that is reminiscent of the understanding employed by Hempel,[97] i.e. the behaviour is expected when a string world picture is applied. This became known as "the landscape solution to the cosmological constant problem" by some such as: (Bousso, Freivogel, & Yang, 2009, p. 47).

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[97] In his now ubiquitous account of explanation as a logical argument Hempel argues the key to understanding an explanation is that: "given the particular circumstances and the laws in question, the occurrence of the phenomenon *was to be expected*; and it is in this sense that the explanation enables us to *understand why* the phenomenon occurred" (Hempel, 1965, p. 337) (italics author's own).

2.2.2 Arguments against the necessity of anthropic reasoning

The critique of anthropic reasoning as an unjustified rejection of the traditional aims of science comes in two forms: first, that any cosmological theory that is constructed with guidance from anthropic reasoning will be in principle non-predictive and therefore unscientific; and, secondly, that anthropic reasoning is a methodological ‘cop-out’, or an *ad hoc* manoeuvre, and that any approach guided by anthropic reasoning is unlikely to produce interesting results and as such will restrict or even prevent progress. The first objection was discussed in section 1.9 of chapter two, and concerns the necessity of in principle unobservable domains of the universe, or alternate universes (a multiverse). The second is concerned with the explanatory aim of a theory of quantum gravity.

2.2.3 The ‘cop-out’ argument and the (non)-utility of anthropic reasoning

There are many who argued that absence of a dynamic selection principle does not justify the use of the anthropic principle as a selection mechanism. In a blog post titled ‘The Anthropic Lack of Principles’, Motl, in response to a post on the subject from Carroll (Carroll, 2004), outlined his objections to the Anthropic Principle in detail (Motl, 2004). Motl argued that “the ‘anthropic principle’ is a philosophical paradigm designed to reduce our curiosity about the patterns in Nature” (Motl, 2004). Taking a historical approach, Motl delivered his argument for the anthropic principle as ‘defeatism’:

> “The first objection is the "defeatism" of the anthropic reasoning. Yes, I think that it is a very wrong approach to science. One might have stopped the progress in science at virtually any moment in the past by claiming that some not-quite-understood features of reality are consequences of unexplainable dynamics involving zillions of Universes (or choices for the laws of Nature), and the only reason why reality behaves the way it does is that if it behaved otherwise, we would not be here.” (Motl, 2004)

Here Motl and Woit are united despite their long history of antagonism.[98] Also in response to Carroll’s article, Woit outlined his disagreement with the use of the anthropic principle.

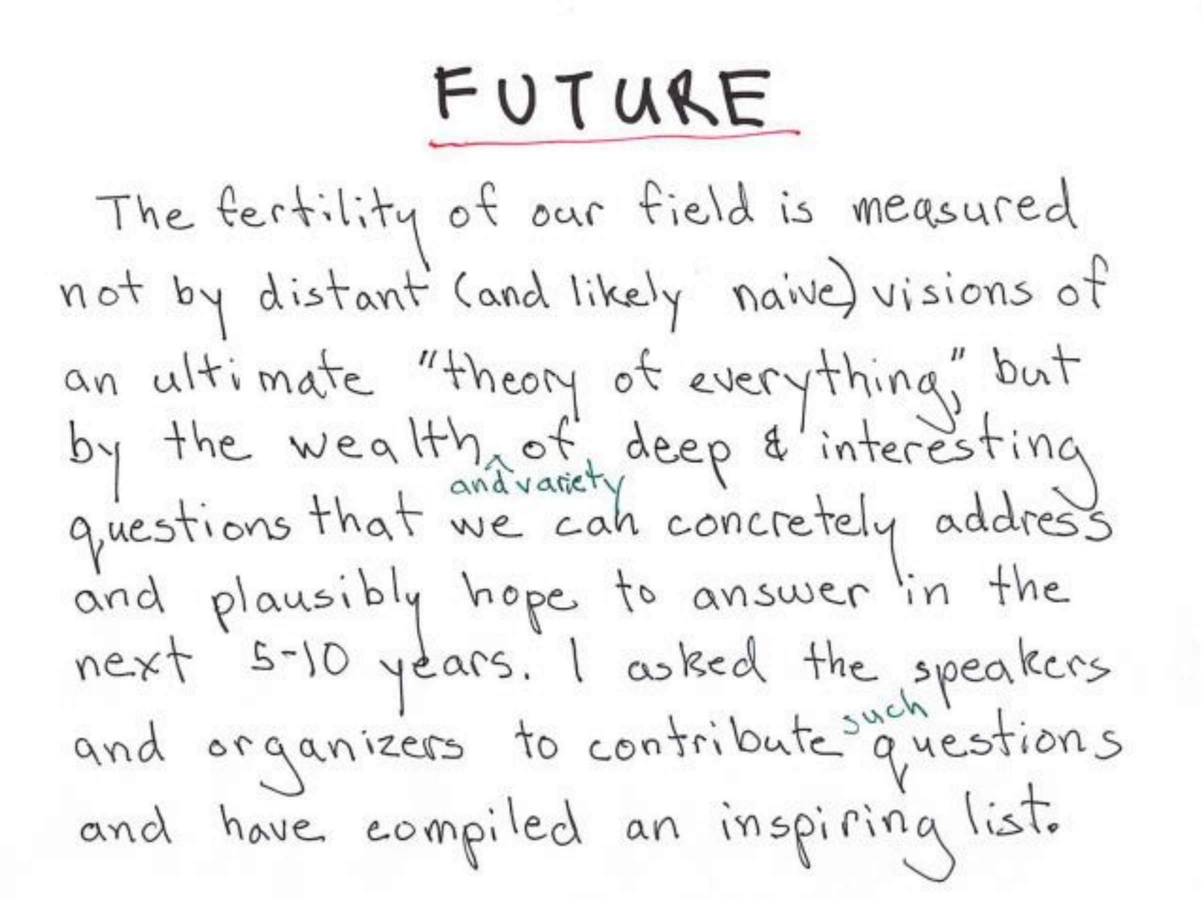
[98] For a number of years if anyone attempted to follow a link to Motl’s blog from Woit’s blog a pop up would appear stating:

> “Sorry but I really can't afford to share readers with that particular person who produces so many untrue statements and who parasites on the work of scientists. I've had huge problems with the people who were being sent from that website to my weblog. I apologize if you're not one of these aggressive and extremely ignorant problem-makers but there's no way to distinguish.
> Virtually everything written on the blog you visited a minute ago is nonsense and virtually all contributors are crackpots. If you want to know who the owner of the blog you visited a minute ago is and why he's doing the nasty things he's doing, listen e.g. to this MP3 file containing an interview with Prof Susskind, one of the leading physicists in this world. Go to 2:50 or so. The first person Prof Susskind refers to is P.W., the second person is Lee S.
> As a minimum, I want the misinformation posted at "Not Even Wrong" to stop and the individual who has been generating it for several years to apologize.” ("Blog Life: Not Even Wrong," 2007)

Likewise, during his short presentation at the Strings 05 panel ‘The Next Superstring Revolution’, Strominger urged the audience to avoid the anthropic principle, arguing: “I have no logical objection to the anthropic principle. It could be true that there are some things we cannot explain anthropically. But it just doesn’t look like to me that we are going to learn anything interesting … nothing interesting is going to come out of it” ("The Next Superstring Revolution," 2005). After the aforementioned debate that occurred at the end of the panel discussion, moderator Steve Shenker put to the audience the question whether: “by the year 3000, say, the value of the cosmological constant would be explained by the anthropic principle or by fundamental physics” ("The Next Superstring Revolution," 2005). The panel spilt evenly but the majority of those in attendance voted against the anthropic principle; Aaron Bergman put the numbers at around 4:1 in favour of fundamental physics (Bergman comment on (Woit, 2005c)).

Conclusion

"The philosophy underlying loop gravity is that we are not near the end of physics, we better not dream of a final theory of everything, and we better solve one problem at the time, which is hard enough." (Rovelli, 2013, p. 16)


FUTURE

The fertility of our field is measured not by distant (and likely naive) visions of an ultimate "theory of everything," but by the wealth and variety of deep & interesting questions that we can concretely address and plausibly hope to answer in the next 5-10 years. I asked the speakers and organizers to contribute such questions and have compiled an inspiring list.

Figure 5.3 'Future' (Strominger, 2014)

As outlined in chapter one, section 1.2, Dawid argued that current practice in the string theory community and the debates over string theory are evidence for a meta-paradigmatic rift (Dawid, 2009, 2013a, 2013b). This chapter has found that, instead of an emergent paradigm that rejects a traditional understanding of scientific methodology, there is a complex array of points of conflict over constraining methodological virtues. Rather than disagreement as to methodology, a high level of agreement was found as to the commitment to constraints, but disagreement then exists as to the sufficiency of consistency, the path to background independence and a non-perturbative formulation, and how to interpret the significance of applications. The string theory community itself was shown to be deeply divided as to the necessity of uniqueness and the legitimacy of anthropic reasoning. This shows that there is no alternate paradigm, nor is any such paradigm emergent. The string theory community, united under Dawid's description of a meta-paradigm, is instead internally debating methodological norms. Furthermore, there are many in the string theory community who wish to stick to what is considered to be a 'traditional' constraining virtue (uniqueness) and who reject the legitimacy of anthropic reasoning. The many points of conflict identified in the debates indicate that the string theory community is not united in their commitment to set of emergent norms. Additionally, the debates over uniqueness and anthropic reasoning have many historical precedents, as Schellekens identifies (Schellekens, 2008). For more examples see *Higher Speculations* (Kragh, 2011a). See section 1.4 of chapter one for further discussion of the difficulty of the use of 'emergence' or novelty in Dawid's work.[99]

The claim that assessment of string theory is motivated by 'final theory' claims (Dawid, 2013a, 2013b) was also found to be descriptively inaccurate. Final theory claims are rare. They are mostly offered up by those that do not currently contribute to the peer reviewed literature or engage in discussion with their peers (on blogs). The references cited by Dawid: (Greene, 1999b; Hawking & Mlodinow, 2010; Kaku & Thompson, 1997; Weinberg, 1993) each made these comments in 'popular' books, mostly as reasons why the uninitiated should get interested in string theory and why string theory should be funded. At the 'Why Trust a Theory' conference, in response to questioning from Dawid, Gross rejected the idea of a final theory. Gross argued that he was broadly agnostic to the concept, but said that he saw no signs that a final theory was close and was sceptical that a final theory could be found. For Gross "the issue in confronting the next step is not one of ideology but strategy: what is the most useful way of doing science?" (Gross quoted in (Wolchover, 2015))

Instead of final theory claims, non-empirical theory assessment occurs in relation to constraints and utility (or what Gross calls strategy). The advantage of this philosophical framework in attempting to understand the debates over string theory is that it is more descriptively accurate and can be expanded to look at other theories of quantum gravity, both current and past. Furthermore the framework can be expanded to understand the graveyard of abandoned attempts of theories of quantum gravity, which died on the basis of non-empirical theory assessment. This does not imply a lack of 'scientific' assessment. Assessments of utility are intimately tied up with assessments of methodology that are considered likely to deliver a theory tested to be correct. This is based on a semi-realist assumption that if certain constraints are satisfied, the theory is more likely to be to a certain extent correct and therefore in time a test will be developed.

String theorists and others who have worked on theories of quantity gravity have not been behaving in an irrational manner when engaging in non-empirical theory assessment since the problem of quantum gravity was first expressed. Those who work in quantum gravity work in an established research tradition where there are a series of expectations (constraints) that have been developed as to what would be considered a successful, possibly unified (this too is contested), theory of quantum gravity.

[99] See section 1.4 of chapter one for further discussion of the difficulty of the use of 'emergence' or novelty in Dawid's work.

It has been long established that experiment is unlikely for any theory of quantum gravity. It is therefore inevitable that historically non-empirical theory assessment has occurred. In the case of string theory, as with alternative approaches, there are a large number of people who have a significant degree of trust in string theory. Dawid argued that: “without establishing local limitations to scientific underdetermination, string physicists would have to treat their theory as a mere speculation and could not find any rational reasons for having any trust in its viability” (Dawid, 2013b, p. 96). Other rational reasons to support string theory, and other approaches, identified in the methodological debates over string theory are: solving long outstanding problems that other theories of quantum gravity have not been able to solve, and the satisfaction of constraints such as renormalisability or indications that the constraints will be satisfied. These are rational reasons to have trust in a theory’s viability. The conflict comes when there is no agreement as to the constraints.

Assessments of string theory are projective or, to quote from the survey undertaken at the conference in Madrid, a belief that string theory is “a step in the right direction” or it is a mistaken direction. However these expectations are not static: as the research tradition develops these expectations develop. Assessment of a theory and what is considered significant for an assessment of a theory evolve together. This is not counter intuitive; instead, it is the same process that occurs when assessment is to a significant extent based on practice. Pickering called in the “mangle of practice” (Pickering, 1995). As Cushing said: “Scientists make the best arguments they can in a given set of circumstances. Some carry the day and some do not. There is no algorithm for success in this enterprise” (Cushing, 1989, p. 20).

Conclusion

There are many points of conflict in the string wars. Rather than a debate between two incompatible and opposing sides, this thesis offers a more complex understanding of the string wars. The picture that is presented is organised into a taxonomy that groups the points of conflict into debates concerning ‘philosophy’, ‘sociology’, ‘technology’ and ‘methodology’. This approach seeks to shift the understanding of the debates from where it currently stands, namely where the string wars are held up as evidence of an emergent conceptualisation of science that is contested by a traditional conception of science, to a more nuanced understanding. Instead of two opposing sides, characterised by a positive and negative appraisal of string theory, a variety of positions can been identified, each concerning a different point of conflict.

In chapter one, which analysed the debates concerning ‘philosophy’ and string theory, a point of conflict was identified as to whether there are circumstances in which the philosophy of science can play a normative role in understanding and evaluating a case study. This was closely linked to positions concerning the lack of empiricism in string theory. The lack of empiricism in string theory was evaluated as significant for the scientific status of string theory, as well as evidence for a new conception of science based on a new methodology. There were further points of conflict concerning the interpretation of the duality relationships in string theory. The duality relationships were evaluated in terms of underdetermination, scientific realism, fundamentality and reductionism. Arguments were also offered for philosophically motivated constraints on a (unified) theory of quantum gravity. These constraints were argued to be significant for the appraisal of current attempts at a (unified) theory of quantum gravity. From these points of conflict, the categories of ‘empirical’, ‘science’, ‘consistent’, ‘simple’, ‘background independent’, and ‘non-perturbative’ were each consistently claimed to be significant for the appraisal of string theory.

In chapter two, which analysed the debates over the ‘scientific status’ of string theory as an example of the rhetorical construction of the boundaries between science and non-science (Gieryn, 1983, 1999), several points of conflict were identified. These points of conflict were, namely, the scientific status of string theory, the in principle testability of string theory and the falsifiability of string theory. In addition, there were points of conflict concerning the string theory research program including self-immunisation strategies, *ad hoc* manoeuvres, retrodictions and solved problems. From these points of conflict, the categories of ‘testable’ and ‘progressive’ were consistently claimed to be significant for the appraisal of string theory.

In chapter three, which analysed the debates over ‘sociological’ appraisals of string theory as arguments for the epistemic significance of the organisation of science, toy sociological models were invoked so as to appraise the string theory research program. The points of conflict identified were organised into two areas: dominance and expertise. In the debates concerning dominance, the points of conflict were the vision of leaders, arrogance, groupthink, resources and the job market, motivations, scientific judgement, and string theory as the 'only game in town' (or arguments for no alternatives). In the debates concerning expertise, the points of conflict identified were popularisations, public acknowledgement of failure, and legitimate sites for promotion and critique. From these points of conflict, the categories of 'dominant', 'expert' and 'public' were consistently claimed to be significant for the appraisal of string theory.

In chapter four, which analysed the debates over literary technologies as evidence for a potentially new form of peer review, the trackback feature was shown to be somewhat of a red herring. It was instead argued that the debates were concerned with the constitution of a 'crack pot' and an 'active researcher' as legitimate witnesses. A further point of conflict identified was the ephemerality of the literary technology of the blog. From these points of conflict, the categories of 'active researcher' (or 'peer'), and 'crack pot' (or 'illegitimate witness') were consistently claimed to be significant for the appraisal of string theory.

In chapter five, which analysed the debates over 'methodology' as debates concerning non-empirical theory assessment, the organisation of these debates departed from that of the previous chapters. Instead the debates were, uniquely, concerned the significance of categories for the appraisal of string theory. Similarly to the previous chapter, several points of conflict were identified concerning consistency, background independence, non-perturbative formulations of theories of quantum gravity, and applications. From these points of conflict, the categories of 'consistent', 'background independent', and 'non-perturbative' were each claimed to be significant for the appraisal of string theory. By contrast, the points of conflict that concerned uniqueness and anthropic reasoning saw arguments for the rejection of uniqueness and the inclusion of anthropic reasoning as significant for the appraisal of string theory.

In the many debates over string theory, the appraisal of theories of quantum gravity, and in particular string theory, is deeply rooted in assessments of the future potential of a theory. Contrasting projective assessments of string theory stem from disagreement among many of the protagonists in these debates as to what constitutes, to borrow Duhem's term, 'le bon sens' (Duhem, 1974). The protagonists in the string wars vigorously debate what they understand to be good science through categories, such as 'crack pot' and 'uniqueness'. Within the context of these debates, the categories are considered to be epistemic; even in the 'sociology' debates, Smolin, Woit, Polchinski and Johnson have each argued that the categories of dominance and expertise have epistemic consequences. Whilst Duhem was notoriously vague as to what exactly constitutes 'good sense' (Ivanova, 2010), the protagonists in debates over string theory are informed by a belief that their understanding of a constraint or category, if applied, will result in future success: a theory of quantum gravity with testable, maybe even confirmable, consequences. Rather than string theorists abandoning empiricism, in the face of the difficulties that *any* theory of quantum gravity will face, they debate the correct methodology (construed very broadly) that will likely produce a testable theory of quantum gravity.

Across the history of the debates, there is continuity of commitment to epistemic categories, but no consensus is formed as to how to understand and/or apply these categories. At each point of conflict, there is commitment that the category of, for example, background independent theories is significant. However, there is no continuity, or consensus, as to precisely how this category should inform an assessment of theories of quantum gravity.[100] The perpetuated use of the categories is somewhat analogous to historical evidence offered in arguments for structural realism. In his influential paper that introduced structural realism, Worrall argued that:

> "There was an important element of continuity in the shift from Fresnel to Maxwell – and this was much more than a simple question of carrying over the successful empirical content into the new theory. At the same time it was rather less than a carrying over of the full theoretical content or full theoretical mechanisms (even in "approximate" form) … There was continuity or accumulation in the shift, but the continuity is one of form or structure, not of content." (Worrall, 1989, p. 117)

In the midst of the string theory debates, it is impossible to predict the victor. However, what is very likely is that the categories invoked in these debates will continue to be considered significant. In the case of background independence, there is agreement that an appraisal of theories of quantum gravity will be informed by considerations of background independence. The analogy with structural realism is not that the continuity is evidence for some form of realism, but rather that the continuity preserves a structure for quantum gravity and theories of everything research that is sociological, philosophical and methodological.

The string theory debates are useful to historians, philosophers or sociologists of science because the points of conflict in those debates foreground contested epistemic categories. The categories may be identified, as points of conflict, because of the way they cluster in the discourse of the debates. This cluster effect comes from consistent appeals to each category, despite disputes as to how to understand the category. This thesis, which identifies these clustering categories, bears some resemblance to Pigliucci and Boudry's (Pigliucci & Boudry, 2013) project to resurrect the demarcation problem using Wittgenstein's cluster concept to inform a philosophy of science and pseudoscience. This is not to say that the continuity of structure identified here gives a definition of science or essentialises science. If this project were to be applied to a different knowledge-producing community, the structure that might be identified would be strikingly different. This thesis does not

[100] In the case of background independence, Rovelli and Polchinski differed in how background independence should constrain theory construction (Polchinski, 2006, 2007a; Rovelli, 2013). See section 1.2 of chapter five for more details.

intend to have any bearing on Pigliucci and Boudry’s project and does not attempt to solve the demarcation problem (although I expect that it might provide useful material for their project). Instead, it demonstrates how in the string wars there is an observable continuity of structure.

However, this thesis does not suggest that the structure is permanent. Two exceptions to this continuity of categories have emerged with the rise of landscape theories. The necessity of uniqueness and the legitimacy of anthropic reasoning are two interrelated debates that occur within the string theory community. Each of these points of conflict, unlike the others discussed, see individuals debating the introduction of an epistemic category, anthropic reasoning, and the potential rejection of an epistemic category: uniqueness. These debates are, following an understanding of how categories may constitute structure for a research area, potentially the ‘deepest’ debates as the resolution of these debates might result in changes to the structure. In stark contrast to the conventional picture of the string wars (as two incompatible sides: string theorists against their critics), these are debates that divide string theorists. This conclusion again reinforces the central claim of this thesis: that there are many varied points of conflict in the string wars.

Appendix One: On being blogged about

One of the well-known difficulties of writing history and philosophy of science about living people is that they often disagree, sometimes publicly, with the details of the accounts of their lives or work. One of the strange things about writing history and philosophy of science about living people and blogs is that you might become part of the world you write about. During the process of writing my thesis this happened to me twice, each time because Peter Woit noticed a publication about, in part, himself and the string wars. The two publications were ‘Contested Boundaries: The String Theory Debates and Ideologies of Science’ and ‘The Role of Heuristic Appraisal in Conflicting Assessments of String Theory’ (Camilleri & Ritson, 2015; Ritson & Camilleri, 2015). Woit’s reasonably lengthy review, ‘Contested Boundaries’, can be found at (Woit, 2015) and a shorter review can be found as part of the post (Woit, 2016).

The odd thing about writing a PhD thesis is that you are expected to have an online presence in order to promote you publications. In my case, as a result of being a co-author of a paper, I had an online presence generated for me automatically. As it was there already, I thought I might as well fill in the blanks. This is how I came to have an academia.edu page. The strange thing about having an academia.edu page is that you are alerted anytime someone clicks on your academia.edu page when it has appeared as a search result in either Google or Bing. This happens in real time, and the alert informs you of what the search terms were and the location (country and city) at which the search was performed. This is how I first came to know I had been blogged about as I went from being a relatively anonymous PhD candidate to being notified repeatedly over a couple of days that people were ‘Googling’ me. The numbers are certainly not staggering, but were enough for me to notice and to look at some blogs where I thought one of my papers might have been mentioned. See Figure A.1 below for an image of the analytics provided by academia.edu. The blue line represents profile views, the green is document views and the purple is unique visitors over a 60-day period. The second instance in which I was blogged about corresponded with the two peaks.

Whilst this is by no means conclusive, it is a small piece of anecdotal evidence of the potential impact the blog entries that discuss and review journal papers can have. Following the first instance, I received several emails from individuals who featured in the paper I had written with Dr Kristian Camilleri telling me exactly what they thought of it and, in the case of one individual, their plans to use the paper in an upcoming publication. This raises another possible consequence of being blogged about. The papers discussed included analysis of the string wars. What if I had influenced the very subjects I was writing about?

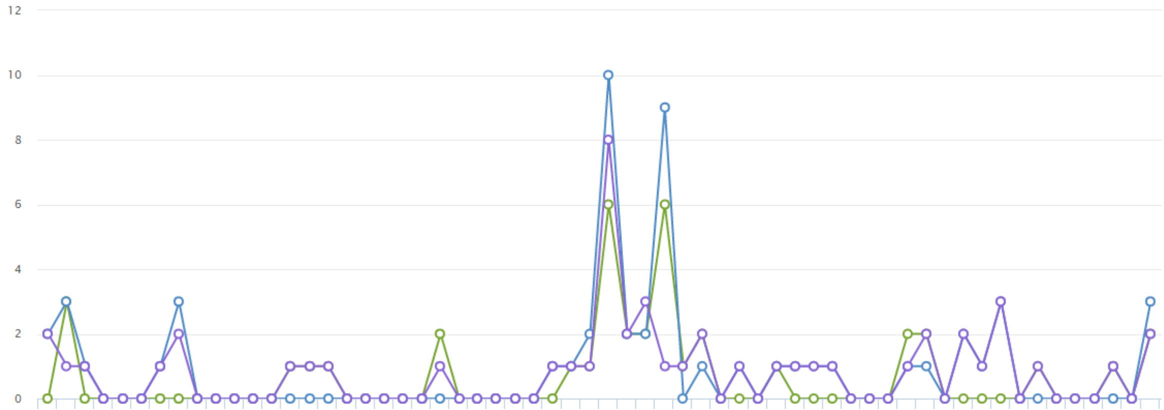


Figure A.1: Analytics taken from academia.edu

Appendix Two: A timeline of the superstring wars (in publications)

1986

25.4.1986	(arXiv preprint) (Ginsparg & Glashow, 1986)
5.1986	Nakanishi 'Comments on the Superstring Syndrome' (Nakanishi, 1986a)
8.7.1986	'Physics May be Up Against The Wall This Time' (Browne, 1986) Dr. Chodos worries that "faddish" particle physicists have begun to flock all too uncritically to a notion called "superstring theory"
23.7.1986	Chronicle of Higher Education publishes 'Controversial 'Superstring' Theory Captures Physicists' Imagination but Could Prove Impossible to Verify' (McDonald, 1986)
9.1986	Nakanishi '"Superstring theory" Syndrome' (Nakanishi, 1986b)
16.10.1986	'The superstring-Theory of everything, or of nothing?' (J. Ellis, 1986)

1987

18.10.1987	K.C. Cole in published in the NYT 'Theory of Everything' (Cole, 1987)
1987	Interviews for Superstrings (Davies & Brown, 1988) J. Ellis interview (J. Ellis, 1987) Feynman interview (Feynman, 1987) Glashow interview (Glashow, 1987) Green interview (Green, 1987) Gross interview (David Gross, 1987) Salam interview (Salam, 1987) Schwarz (Schwarz, 1987) Weinberg interview (Weinberg, 1987) Witten interview (Witten, 1987)

1988

14.2.1988 and 25.8.1988	Interviews aired on BBC 3 (All interviews published a year later in (Davies & Brown, 1988))
1988	*A Brief History of Time* published (last chapter dedicated to String theory) (Hawking, 1988)
1988	Feynman interview with Mehra published in (Mehra, 1994, p.507)

1993

1.1993	'Can the Superstring theory become physics' (Nakanishi, 1993)
9.1993	*Dreams of a Final Theory* Published (Weinberg, 1993)

1993	*The End of Physics* (Lindley, 1993)

1995

1995	*The End of Science* (Horgan, 1997)
1995	*Beyond Einstein: The cosmic quest for the theory of the universe* published (Kaku & Thompson, 1997)

1996

12.6.1996	Gross and Witten write op-ed 'The Frontier of Knowledge' (David Gross & Witten, 1996)

1997

1997	'String theory is testable, even supertestable' (Kane, 1997)

1999

1999	UK edition of *The Elegant Universe* (Greene, 1999a) US edition (Greene, 1999b)

2000

2000	'Will we have a final theory of everything' (Weinberg, 2000)

2001

29.1.2001	'String theory an evaluation' (Woit, 2001)

2002

3-4.2002	'Is String Theory Even Wrong?' (Woit, 2002a)
24.10.2002	'Physics bitten by reverse Alan Sokal hoax?' (Baez, 2002)
9.11.2002	'Half empty or half full' (Bogdanov Hoax) (Distler, 2002a)
17.11.2002	'Ideas & Trends; In Theory, It's True (Or Not)' (Bogdanov hoax) (G. Johnson, 2002)
2002	Horgan and Kaku bet $2000 "By 2020, no one will have won a Nobel Prize for work on superstring theory, membrane theory, or some other unified theory describing all the forces of nature." (Horgan & Kaku, 2002)

2003

12.2.2003	Susskind comments on ‘The Landscape’, comments from: ’t Hooft, Susskind, Giddings, Steinhardt, Spriopulu, Smolin, Segre, Kelly and Vilenkin (Susskind, 2003b)
12.10.2003	‘Dialog on Quantum Gravity’ (Rovelli, 2003a) (Rovelli, 2003b)
28.10.2003	*The Elegant Universe* (Nova miniseries written by and starred in by Brian Greene)
	The Elegant Universe: Einstein's Dream (Cort, Greene, & McMaster, 2003a) The Elegant Universe: String’s the thing (Greene & McMaster, 2003) The Elegant Universe: Welcome to the 11th Dimension (Cort, Greene, & McMaster, 2003b)
28.10.2003	Viewpoints on String Theory Jim Gates (Gates, 2003) Sheldon Glashow (Glashow, 2003) David Gross (David Gross, 2003b) Joe Lykken (Joe Lykken, 2003) Amanda Peet (Peet, 2003) Steven Weinberg (Weinberg, 2003) Edward Witten (Witten, 2003)
2003	The non-inclusion on String Theory in (Veltman, 2003) justification on p. 308

2004

17.3.2004	Woit begins blog ‘Not Even Wrong’ (Woit, 2004 - Present)
18.8.2004	Smolin Vs. Susskind: The Anthropic Principle (Smolin & Susskind, 2004)
15.10.2004	Carroll ‘Anthropic Principle’ (Carroll, 2004)
15.10.2004	‘Sean Carroll on the Landscape’ (Woit, 2004b)
16.10.2004	‘The Anthropic Lack of Principles’ response to Carroll and Woit (Motl, 2004)
12.2004	*Parallel Worlds: A journey through creation, higher dimensions, and the future of the cosmos* published (Kaku, 2005b)
2004	*The Road to Reality* Published (Penrose, 2004)
2004	*The Fabric of the Cosmos: Space, Time, and the Texture of Reality* published (Greene, 2004)

2005

1.2005	‘Where do we stand in fundamental string theory?’ (David Gross, 2005)
4.1.2005	Phillip Anderson excerpt in the NYT (Various, 2005), also available here (Anderson, 2005)
14.3.2005	‘Theory of everything tying researchers up in knots’ (Davidson, 2005)
5.2005	*Einstein’s Cosmos: How Albert Einstein’s vision transformed our understanding of space and time* published (Kaku, 2005a)
12.7.2005	Panel discussion at Strings 2005 ‘The Next Superstring Revolution’ featuring Raphael Bousso (UC Berkeley), Shamit Kachru (SLAC & Stanford), Ashok Sen (Harish-Chandra Research Institute), Juan Maldacena (IAS, Princeton), Andrew Strominger (Harvard), Joseph Polchinski (KITP & UC Santa Barbara), Eva Silverstein (SLAC & Stanford) and Nathan Seiberg (IAS, Princeton)
12.7.2005	‘Anonymous Remailers’ (death threats) (Motl, 2005a)
21.7.2005	‘Two Cheers for String Theory’ (Carroll, 2005b)
24.8.2005	‘ArXiv joins the blogosphere’ (Carroll, 2005a)
24.8.2005	‘arXiv trackbacks’ (Woit, 2005a)
24.8.2005	‘Trackbacks and the Archives’ (Distler, 2005)
28.9.2005	‘Into the Swampland’ (Woit, 2005b)
29.9.2005	‘Swampland’ (Motl, 2005b)
10.2005	*Warped Passages: unravelling the mysteries of the universe's hidden dimensions* published (Randall, 2005)
5.11.2005	‘From the Sublime to the Ridiculous’ (criticism of Krauss) (C. Johnson, 2005)
7.11.2005	On Krauss’s NYT piece (controversy once the article in published the next day in the comments) (Trodden & Krauss, 2005)
8.11.2005	‘Science and Religion see fascination in things unseen’ (Krauss, 2005)
18.11.2005	‘Particle physicists perspective’ (Hewett, 2005)
19.11.2005	‘Particle physicists perspective’ (Woit, 2005d)

2006

23.2.2006	‘Letter to the arXiv Advisory Board’ (Woit, 2006c)
3.3.2006	‘Crackpots, contrarians and the free market of ideas’ (Carroll, 2006a)

4.3.2006	‘Crackpots and Scientific Resources’ (Motl, 2006a)
5.3.2006	‘Archive Trackback Policy’ (Distler, 2006)
6.3.2006	‘Physics Catfight’ (Orzel, 2006b)
3.2006	‘What Makes a Theory Testable, or Is Intelligent Design Less Scientific Than String Theory?’ (Ehrlich, 2006)
27.4.2006	‘Falsifying String Theory Through WW Scattering’ uploaded to the arXiv (Distler et al., 2006b)
1.5.2006	Woit claims ‘Falsifying String Theory Through WW Scattering’ (Distler et al., 2006b) does not contain a falsifiable test of string theory (Woit, 2006b)
19.5.2006	‘String Theory Backlash’ (Carroll, 2006d)
19.5.2006	‘Sean Carroll joins Peter Woit’ (Motl, 2006b)
18.6.2006	Greene debates Smolin on radio (Greene & Smolin, 2006)
6.2006	English edition of *Not Even Wrong* published (Woit, 2006d)
2.8.2006	Hossenfelder’s review of a final draft of *Trouble with Physics* (Hossenfelder, 2006)
2.8.2006	Smolin interview with Hossenfelder (Smolin, 2006a)
5.8.2006	Review of *The Trouble with Physics* (G. Ellis, 2006)
21.8.2006	Storm in a Teacup I (C. Johnson, 2006a)
25.8.2006	Susskind’s review of NEW and TTWP (Susskind, 2006)
28.8.2006	Woit reviews *The Trouble with Physics* (Woit, 2006f)
19.9.2006	US edition of *The Trouble with Physics* published (Smolin, 2006c)
9.2006	US edition of *Not Even Wrong* published (Woit, 2006e)
10.2006	UK edition of *The Trouble with Physics* (Smolin, 2006b)
1.10.2006	‘Falsifying Models of New Physics via WW Scattering’ uploaded to the arXiv (Distler et al., 2006a)
3.10.2006	Carroll reviews *The Trouble with Physics* (Carroll, 2006e)
5.10.2006	Storm in a Teacup II (C. Johnson, 2006b)
5.10.2006	Storm in a Teacup III (C. Johnson, 2006c)
5.10.2006	Storm in a Teacup IV (C. Johnson, 2006d)
8.10.2006	Cole’s review (Woit claims it contains fraudulent material) (Cole, 2006)
9.10.2006	‘Spacetime in the Ultimate Theory’ (Nakanishi, 2006b)
10.10.2006	Orzel’s review of the *Trouble with Physics* (Orzel, 2006a)
19.10.2006	Bergman’s review of *Not Even Wrong* (Bergman, 2006a)

20.10.2006	Greene's Op-Ed 'Universe on a string' (Greene, 2006)
20.10.2006	'String Wars' (G. Johnson, 2006)
21.10.2006	'Scott Aaronson on the 'string wars'' (Carroll, 2006b)
27.10.2006	Storm in a Teacup V (C. Johnson, 2006e)
30.10.2006	Short version of Carroll's review: 'The Strings the Thing' published (Carroll, 2006c)
10.2006	Note added to 'Can the Superstring theory become physics' (Nakanishi, 1993)
10.2006	'Theory in particle physics: theological speculation versus practical knowledge' published (Richter, 2006)
10.11.2006	Storm in a Teacup VI (C. Johnson, 2006f)
16.11.2006	'Answering critics can add fuel to the controversy' (Witten, 2006)
7.12.2006	Polchinski reviews Smolin and Woit (Polchinski, 2006)

2007

22.1.2007	'Falsifying Models of New Physics via WW Scattering' (Distler et al., 2007)
1-2.2007	'All Strung Out' Polchinski's review published in American Scientist (Polchinski, 2007a)
25.2.2007	'This Week's Finds in Mathematical Physics (Week 246)' Baez discusses *Not Even Wrong* and *The Trouble with Physics* (Baez, 2007b) commentary (Baez, 2007a)
17. 3.2007	Woit gives a talk in Rome (8.3.2007) and Pisa (15.3.2007) on 'Is String Theory Testable' and publishes the slides on his blog (Woit, 2007a, 2007b)
28.3.2007	Greene and Krauss debate with Michael Turner asking questions (Greene & Krauss, 2007)
5.3.2007	Smolin, Duff and Cartwright Debate (Smolin et al., 2007)
13.3.2007	Storm in a Teacup VII (C. Johnson, 2007)
1.4.2007	'Cool – aid' (Orzel, 2007)
4.2007	Smolin Responds to Polchinski's review (Smolin, 2007a)
21.5.2007	Polchinski responds to Smolin's response (Polchinski, 2007b)
24.5.2007	'String Theory – not dead yet' (Carroll, 2007)
9.2007	'Stringscape' published (Chalmers, 2007)
3.9.2007	Woit's response to 'Stringscape' (Woit, 2007b)
3.9.2007	Motl's response to Stringscape (Motl, 2007b)

4.9.2007	Motl's response to Stringscape II (Motl, 2007a)

2008

10.5.2008	String theory and the crisis of particle physics II or the ascent of metaphoric arguments (Schroer, 2008a, 2008b)
13.7.2008	'The Emperors Last Clothes? Overlooking the string theory landscape' published (Schellekens, 2008)
2008	'Einstein and the Quest for a Unified Theory' (David Gross, 2008)

2009

26.10.2009	'Transcript for the String Wars Video' ("Transcript for the String Wars Video," 2009)
2009	'Living in the Multiverse' published in *Universe or Multiverse* (Weinberg, 2009)
2009	*Universe or Multiverse* published (Carr, 2009)

2010

26.2.2010	'String Theory for the Scientifically Curious with Dr. Amanda Peet' (Peet, 2010)
4.2010	'String theory: Physics or metaphysics' (Veneziano, 2010)
22.10.2010	Baez Summary of Bogdanov Affair (Baez, 2010)

2011

7.12.2011	Woit and Smolin respond to the arXiv paper 'String and M-theory: answering the critics' (Duff, 2011a; Woit & Smolin, 2011)
2011	*Faster Than the Speed of Light: the story of a scientific speculation* published (Magueijo, 2011)
2011	'Theory of Everything: Answering the Critics' (Duff, 2011b)

2012

5.2012	*Birth of String Theory* published (Cappelli et al., 2012)
31.7.2012	'Fundamental Physics Prize' (Woit, 2012b)
9.8.2012	'Reminder to Readers' (Strassler, 2012b)
15.8.2012	'From String Theory to the Large Hadron Collider' (Strassler, 2012a)

2012	'Frontiers Beyond the Standard Model: Reflections and Impressionistic Portait of the Conference' published (M Shifman, 2012)

2013

1.2013	Special Issue in *Founation of Physics* 'Forty Years of String Theory Reflecting on the Foundations' (Haro, Dieks, 't Hooft, & Verlinde, 2013)
	'A Critical Look at Strings' (Rovelli, 2013) 'A Perspective on the Landscape Problem' (Smolin, 2013) 'On the Foundations of Superstring Theory' ('t Hooft, 2013) 'Mirror Symmetry and Other Miracles in String Theory' (Rickles, 2013b) 'Theory Assessment and Final Theory Claim in String Theory' (Dawid, 2013b) 'What We Don't Know About Time' (Balasubramanian, 2013) 'Is String theory a Theory of Quantum Gravity?' (Giddings, 2013) 'The Gauge-String Duality and Heavy Ion Collisions' (Gubser, 2013) 'Evolving Notions of Geometry in String Theory' (Martinec, 2013) 'String Theory' (Susskind, 2013) 'String and M-theory: Answering the critics' (Duff, 2013)
14.5.2013	Woit reviews *String Theory and the Scientific Method* (Woit, 2013b)
15.5.2013	Motl reviews *String Theory and the Scientific Method* (Motl, 2013)
16.5.2013	Baggot and Duff Debate: A Theory of Everything … has physics gone too far? (Baggott & Duff, 2013)
5.2013	*Farewell to Reality: How Fairytale Physics Betrays the Search for Scientific Truth* published (Baggott, 2013)
5.2013	*String Theory and the Scientific Method* published (Dawid, 2013a)
7.9.2013	'Did the LHC just Rule Out String Theory?!' (Strassler, 2013b)
19.9.2013	'Am I Misleading You on String Theory?' (Strassler, 2013a)
23.9.2013	'Quantum Field Theory, String Theory, and Predictions' (Strassler, 2013d)
24.9.2013	'Quantum Field Theory, String Theory, and Predictions (Part 2)' (Strassler, 2013e)
1.10.2013	'Quantum Field Theory, String Theory and Predictions (Part 3)'

2014

1.2014	What Scientific Idea is Ready for Retirement? Question posed and answered at theedge.com by various indivisuals:
	Carroll 'Falsifiability' (Carroll, 2014a)

	Gefter ‘*The* Universe’ (Gefter, 2014) Kane ‘Our World Has Only Three Space Dimensions’ (Kane, 2014) Linde ‘Uniformity and the Uniqueness of the Universe’ (Linde, 2014) Rees ‘We'll Never Hit Barriers To Scientific Understanding’ (Rees, 2014) Smolin ‘The Big Bang Was the First Moment of Time’ (Smolin, 2014a) Steinhardt ‘Theories of Anything’ (Steinhardt, 2014) Weinstein ‘M-theory / String Theory is the Only Game in Town’ (E. Weinstein, 2014) Woit ‘The Naturalness Argument’ (Woit, 2014d)
7.1.2014	‘What Scientific Ideas Are Ready for Retirement?’ Blog post (Carroll, 2014b)
30.5.2014	Symposium on Evidence in the Natural Sciences Panel Discussion with Brian Greene, Peter Galison and Jim Baggott (Greene, Galison, & Baggott, 2014)
5.2014	‘Supersymmetry and the Crisis in Physics’ (Joseph Lykken & Spiropulu, 2014)
30.5.2014	‘New Evidence’ talk by Peter Galison at the Symposium on Evidence in the Natural Sciences (Galison, 2014)
9.6.2014	‘The Evidence Crisis’ response by Jim Baggott to the Symposium on Evidence in the Natural Sciences Panel Discussion (Baggott, 2014)
4.7.2014	Interview with Dawid (Dawid, 2014)
10.7.2014	Woit reviews reactions to *String Theory and the Scientific Method* ‘String theory and post-empiricism’ (Woit, 2014e)
12.7.2014	‘Post Empirical Science is an Oxymoron’ (Hossenfelder, 2014b)
27.8.2014	Quora question: ‘Is it true that many physicists are abandoning String Theory? (Various, 2014)
16.12.2014	Editorial defending an empirical scientific method published. Calls for help from philosophers. ‘Scientific method: Defend the integrity of physics’ (G. Ellis & Silk, 2014)
17.12.2014	Woit and Hossenfelder respond to ‘Scientific method: Defend the integrity of physics’ (Woit, 2014b) (Hossenfelder, 2014a)
18.12.2014	Orzel responds to ‘Scientific method: Defend the integrity of physics’ (Orzel, 2014)
19.12.2014	Woit reviews (Polchinski, 2014) and the debate over access to the arXiv restarts (Woit, 2014c)
2014	Smolin reviews Dawid’s *String Theory and the Scientific Method* (Smolin, 2014b)

2015

26.1.2015	Frank responds to ‘Scientific method: Defend the integrity of physics’ (Frank, 2015)
1.4.2015	April Fools ‘A Farewell to Falsifiability’ (Scott et al., 2015)
18.5.2015	Hossenfelder reviews *String Theory and the Scientific Method* by Richard Dawid (Hossenfelder, 2015)
21.5.2015	‘What Happens When We Can’t Test Scientific Theories?’ (Close, 2015)
24.5.2015	Clifford Johnson responds to Close with ‘On Testability’ (C. Johnson, 2015)
28.5.2015	‘Layers of Reality’ (Carroll, 2015)
9.6.2015	‘Could the evolution of theoretical physics harm public trust in science?’ (Corneliussen, 2015)
10.6.2015	‘A Crisis at the Edge of Physics’ NYT editorial (Frank & Gleiser, 2015)
11.2015	‘What Every Physicist Should Know About String Theory’ (Witten, 2015)
7-9.12.15	‘Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics’ conference in Munich, speakers include Peter Achinstein, Elena Castellani, Radin Dardashti, Richard Dawid, Gia Dvali, George Ellis, Sabine Hossenfelder, Gordon Kane, Helge Kragh, Dieter Lüst, Viatcheslav Mukhanov, Massimo Pigliucci, Joseph Polchinski (absent, paper read by Gross), Fernando Quevedo, Carlo Rovelli, Björn Malte Schäfer, Joseph Silk, Chris Smeenk, Karim Thébault and Chris Wüthrich
8.9.15	Polchsinki publishes ‘String Theory to the Rescue’ (Polchinski, 2015)

2016

22.1.16	Polchsinki published some follow up remarks to the Munich conference: ‘Why trust a theory? Some further remarks (part 1)’ (Polchinski, 2016b)
26.1.16	Polchsinki published a modified version to follow up remarks to the Munich conference: ‘Why trust a theory? Some further remarks (part 1)’. Section critical of Woit is removed (Polchinski, 2016a)

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