Contested Boundaries: The String Theory Debates and Ideologies of Science

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Over the last three decades, physicists have engaged in, sometimes heated, debates about relative merits and prospects of string theory as a viable research program and even about its status as a science. The aim of this paper is to provide a deeper understanding of this controversy as a particular form of boundary discourse. Drawing on the sociological work of Thomas Gieryn and Lawrence Prelli, we bring to light the way in which protagonists appeal to, and rhetorically construct, different views about the scientific method *and the* scientific ethos*, in an effort to legitimize or delegitimize string theory.*

1. 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 its practitioners as the only the only viable option for constructing a unified theory of gravity and elementary particle physics. It has attracted an unprecedented number of researchers, including many Nobel Laureates, and has been instrumental in opening up new areas at the intersection of mathematics and physics. (Kragh 2011, p. 314). Yet, since the 1980s it has been mired in controversy, and 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 2007, p. 265), the first plausible candidate for a final theory (Weinberg [1992] 1994, p. 212), and a catastrophic failure (Smolin 2008, p. 170). Over the last three decades, physicists have engaged in, sometimes heated, public debates about relative merits and prospects of string theory as a viable research program and even about its status as a science. Indeed the debate has, as Peter Galison rightly points out, "raised deep questions about the very

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nature of physics as a discipline" (Galison 1995, p. 403). Much of the controversy surrounding string theory is born out of its continual failure to make contact with experiment in any simple sense. In spite of the fact that string theory has not yet found experimental support, it has been through two self-proclaimed revolutions and continued to be at the forefront of research in theoretical physics.

Over the last three decades string theory (broadly construed here to include its successor M-theory) has emerged as the leading hope for the unification of general relativity and elementary particle physics. Gravity is currently explained by Einstein's theory of general relativity. The electromagnetic, the strong and the weak forces are described by quantum field theory, which forms the theoretical basis for the Standard Model of particle physics. From an empirical point of view, the standard model has provided physicists with an immensely successful account of elementary particles, but it ignores the effects of gravity, which is negligible at the quantum level in all but very few scenarios (e.g., black holes). The result is two incompatible approaches: the theory of general relativity, which is applicable at large scales, and the standard model, which is applicable at the quantum scale. This situation is deemed extremely problematic and a successful unification of the four forces, or a quantum theory of gravity, has been become for many theorists, the "Holy Grail" of physics (Greene 1999, p. 15). Attempts at reconciling these two pillars of modern physics have been plagued with problems. In spite of the immense difficulties, the search for a unified theory has been a major stimulating force for theoretical research since the 1980s (Weinberg 1994, p. 17).

While there exist a number of other current approaches to research on quantum gravity, most notably 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 and the weak force. As Rovelli explains, "the unification of the forces and the quantization of gravity are two conceptually distinct problems [...]. String theory is an attempt to solve the two problems at once" (Rovelli 2013, p. 15).

While criticisms of string theory first appeared in the 1980s, 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 (Brian Greene and Lawrence Krauss 2011; Lee Smolin, Michael Duff, and Nancy Cartwright 2007). String theorists have reacted strongly to their critics, calling into question their integrity and motives, and in some cases engaged in bitter polemics. A number of string theorists have refused to engage in debate with Woit or Smolin, or anyone who has read their books. In 2006 George Johnson, the journalist in residence at the Kavli Institute for Theoretical Physics, labeled the escalating controversy the "String Wars."[1]

The public debate that has erupted over string theory in recent years raises important philosophical questions, but also questions about the social dynamics of scientific controversies. Critics like Smolin and Woit have argued that their decision to write books addressed to the wider public was in part motivated by a need to respond to what they saw as the misleading narratives of progress and triumph, which appeared in the popular media and in the popular books written by physicists like Steven Weinberg (1994), Kaku and Thompson (1997), Brian Greene (1999), and Leonard Susskind (2005).[2] Moreover, the recent criticisms, which address philosophical or sociological concerns, are to some extent at least, beyond the sphere of physics proper.

The public nature of the recent controversy has itself been cause for concern for many physicists. In defending string theory against its detractors, Mike Duff has recently remarked that: "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, p. 7). Even some critics, like Gerard 't Hooft, have expressed concerns over the public nature of debates: "By addressing a larger public, one generates the impression that quite general arguments could suffice to disqualify this kind of research, but that is definitely not the case" (quoted in Chalmers 2007, p. 47).

While some physicists have expressed serious reservations about responding publicly to critics like Smolin and Woit for fear of fuelling the controversy, others like Carroll have welcomed the opportunity to engage in public debate (Brumfiel 2006, p. 491). Mike Duff has been more reluctant to engage in public controversy, but sees it as important: "misguided though some criticisms of string and M-theory may be, they can still be very damaging and so require a response." A public defence of string theory is therefore deemed necessary "not only because a public understanding of science is a good thing, but also because decisions about the future direction of scientific research are increasingly being made by non-scientists, some of which are hostile to string theory" (Duff 2013, p. 184). Duff underscores this

1. George Johnson, the science journalist in residence at the Kavli Institute for Theoretical Physics, gave a talk titled "The String Wars" on October 20, 2006 about the reaction of the media to Smolin and Woit's books. (Johnson 2006)

2. It would be interesting to conduct a sociological study of the in which the use of different popular media—books, popular journal and magazine articles, public lectures, radio, television, the internet have shaped the debate.

point by drawing the reader's attention to the fact that shortly after Smolin's and Woit's books were published, "funding for the two European research networks on string theory was withdrawn" (Duff 2013, p. 188).

This paper focuses on two central issues that have acquired prominence in the public controversy over string theory. The first, 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. Critics and defenders of string theory have engaged in a discourse over *whether string theory legitimately counts as science*. Thomas Gieryn has aptly described this kind of discursive activity as "boundary work." In this paper we use and expand on Gieryn's notion of boundary work (1983) 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, we find a variety of responses as to what conclusions we should draw 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.

The second dimension of the debate, which we focus on, and which has arisen more recently, concerns the institutional dominance of string theory. Some critics, most notably Lee Smolin, have argued that in spite of its failure to generate empirically testable predictions, string theory has become dominant in theoretical physics, to such an extent that it has now become detrimental to further progress. Critics argue that other potentially fruitful avenues of research, like loop quantum gravity, have been closed off. Here the critical discourse turns from questions of methodology to the sociological norms of scientific inquiry. Critics accuse string theorists of engaging in power politics, groupthink and self-serving hiring practices. Protagonists on both sides of the debate appeal to, and construct, different values underpinning the scientific ethos (Mertonian associations intended). Defenders of string theory have been at pains to point out that such criticisms are misguided and in many instances blatantly ideologically motivated by outsiders. A new generation of theoretical physicists have gravitated towards string theory research, they claim, not because of political pressures, or because of the lack of professional opportunities in other avenues of research, but because string theory is still the most promising, indeed the only viable, candidate for a unified theory.

In drawing attention to these two dimensions of the controversy, this paper 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. This paper does not offer a philosophical analysis of the demarcation problem in the context of string theory, nor do we provide an assessment of theoretical problems that have plagued string theory in the quest to find a unified theory.[3] Instead this paper focuses on how rhetorical discourse has been deployed in the controversy over string theory. In focusing on this 'rhetorical' aspect we do 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 1995, p. 403).

This paper 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, there are growing signs that the controversy has subsided over the last few years, or has receded into the background. If the recent Strings-conferences are any indication, it appears that many string theorists have, at least temporarily, abandoned work on unification and turned their attention to problems of cosmology and quantum field theory.

A comprehensive social and intellectual history of the string theory controversy remains a formidable task for the future, and remains beyond the scope of this paper. Nevertheless it is clear that the methodological and sociological 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 the string theory controversy particularly interesting from both a historical and sociological perspective.

2. The Discourse of Demarcation

2.1 The Concept of Boundary Work

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, 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

3. String theory has recently begun to attract the attention of historians and philosophers of science such as Helge Kragh 2011, Nancy Cartwright and Roman Frigg 2007, p. 20, Dean Rickles 2013, p. 43, Richard Dawid 2006, p. 73, 2013a, 2013b, Elena Castellani 2012, and Reiner Hedrich 2007, p. 38.

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 non-science' for "ideological" reasons (Gieryn 1999, p. 26).

Recognising the label "science" carries with it intellectual legitimacy, professional opportunities and material resources, scientists endeavour to construct the boundary in such a way as to ensure that their own work qualifies as scientific, while at the same time discrediting other theories or activities they deem to be non-scientific or pseudo-scientific. 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, 34: 91)

As we shall argue below, the debates over string theory offer an interesting case of boundary work. In this controversy, we find no single view of what constitutes science, 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). As we shall see, this nicely captures what has unfolded in the string theory debates, in which physicists have responded in a variety of ways. Here we find, the "contours of science are shaped by the local contingencies of the moment" (Gieryn 1983, p. 5).

2.2 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 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 2011, 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 and theoretically consistent framework. 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 (Kragh 2011, p. 306). Perhaps not surprisingly, high-energy experimental physicists expressed serious concerns about string theorists' preference for theoretical abstraction over the laboratory (Richter 2006, pp. 8–9). After 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 2008, 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. Glashow claimed that string theory "might be the sort of thing that Wolfgang Pauli would have said was 'not even wrong" (Ginsparg and Glashow 1986, p. 39).[4] 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" (quoted in Chalmers 2007, p. 35). In 2001, vocal critic, Peter Woit reiterated these 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 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, 2)

4. This Pauli quotation has been quoted extensively and was used by Woit as the title of a blog he began in March 2004 which was dedicated to discussions about and criticisms of string theory. It also serves as the title of his book published in 2007.

Here Woit called 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, [. . .] *there is nothing wrong with this*; such explanations and predictions are still way out of reach for respectable theories of physics" ('t Hooft 2013, p. 47). According to Carlo Rovelli, "loop quantum gravity is in no better shape than string theory in making verifiable predictions. There are no experiments supporting loops, nor any other quantum theory of gravity" (Rovelli 2013, p. 18).

The difficulties in drawing any clear demarcation between science and non-science emerge clearly when we take into account the fact that string theory is not really a theory, in any logical sense, but rather 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*.[5] As Woit acknowledges: "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 2007, p. 213). What counts as scientific can be broadened to include forms of speculative theorizing "that would definitely make superstring theory a science" (Woit 2007, 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 2007, pp. 214–15)

5. Nancy Cartwright and Roman Frigg have also explored string theory as a research program in an attempt to determine, in the Lakatosian sense, if it is progressing or degenerating (Cartwright and Frigg 2007, p. 20).

As Woit points out, in the case of string theory, the demarcation of science from non-science becomes a matter of scientific judgment. 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 judgments may of course be contested, and in ambiguous cases, boundary work assumes critical importance.

The sticking point for many physicists is not whether string theory as it currently stands is falsifiable, but whether it is showing signs of heading in the right direction. 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 and 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 and 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)" (quoted in Chalmers 2007, p. 37).

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.[6] 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 1986, pp. 143–53). Reiterating this point in a paper with Ginsparg, he argued: "Superstring arguments eerily recall 'arguments from design' for the existence of a Supreme Being" (Ginsparg and Glashow 1986, p. 7). In 1988 Glashow again attacked string

6. 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)

theory, characterizing it as a form of inquiry “more appropriate to departments of mathematics or even to schools of divinity than to physics departments” (Glashow 1988; 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).[7] 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 ‘non-science’” (Gieryn 1983, p. 791). By inviting the comparison between string theory and medieval theology or intelligent design, Glashow, Richter and Krauss attempt to create doubt by association.

2.3 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 1999, 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 drawing the distinction between falsifiable *in practice* and falsifiable *in principle*, string theorists can affirm their commitment to falsifiability as a criterion for demarcating science from non-science, while maintaining the view that string theory qualifies as science. This position is taken up by a number of prominent defenders of string theory, notable among them Gabriele Veneziano, who resolutely maintains that “string theory is falsifiable” (Veneziano 2010, p. 18).

String theory is not the first theory to be in this position, as advocates like to point out. They list examples such as black holes, neutrinos and neutron stars, all of which were predicted by theories, but were not falsifiable when

7. The scientific status of Intelligent Design became a controversy that was argued all the way up to the high court of the United Stated of America. Intelligent Design was deemed not to be science, not just once but twice, partly on the basis that it was an unfalsifiable theory.

they were first proposed. 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 1999, 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.

Defenders of string theory typically draw a distinction between *predictions* and *testable predictions*. As Veneziano points out, contrary to what is sometimes maintained by 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, in 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 or cosmic strings in the sky, and this level of prediction is perfectly acceptable in almost every other field of science" (quoted in Chalmers 2007, p. 36). Only in experimental particle physics is it the case that "a theory can be thrown out if the 10^{th} 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 explains, "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 falsifiability 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.

2.4 Self-Immunization Strategies and Ad Hoc Maneuvers

The introduction of a new form of symmetry, dubbed 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.[8] 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, and would have been observed. Given that this is not the case, some form of spontaneous symmetry breaking must take place (Polchinski 1998, pp. 512–13). 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 Large Hadron Collider, 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 one could therefore expect "that some of these superpartners should be observable at the CERN Large Hadron Collider (LHC)" (Schwarz 2000, p. 4). Ed 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, "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 track" (Greene 2011). The testability of

8. Pierre Raymond 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).

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 have not been discovered.[9] 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 1999, 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). Critics like philosopher Reiner Hedrich see 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).[10]

Some critics of string theory see this as a kind of ad hoc maneuvering, 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. 9). The different roles of supersymmetry throughout the history of string theory illustrate this point. It 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.

9. Recent developments at the Large Hadron Collider have cast doubts over finding evidence for supersymmetry at an energy scale below 1 TeV.

10. We may note that similar strategy was employed in the defence of Copernican astronomy against Tycho's objection that we cannot observe stellar parallax. Here it was assumed the orbits of the planets must be 700 times larger than was thought to be the case in the geocentric universe.

2.5 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 instead have instead emphasized that string theory predicts certain observed phenomena for which experimental evidence "already exists" (Greene 2008, 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).

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 discovered that the massless spin-2 particle, which emerged as a consequence of quantizing the dynamics of relativistic string states, could be interpreted as the graviton—the theoretical messenger 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 1999, 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 1999, 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.

2.6 String Theory Makes Progress by Solving Problems

Critics have typically portrayed string theory as a degenerating research program, in a Lakatosian sense, for its failure to make novel testable predictions.[11] 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, Joe 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 re-normalizable perturbation theory, the application of Calabi-Yau manifolds, and the discovery of and dualities between different kinds of physical 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" (Moskowitz 2011).

Here we 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 and 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 renormalization problem of infinite self-energies for the gravitational field (Chalmers 2007, p. 38). This result, which heralded the beginning of the "first superstring revolution," perhaps more than anything

11. 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 problemshift*'): 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 problemshift*')" (Lakatos 1978, p. 112). Nancy Cartwright and Roman Frigg concluded in their Lakatosian analysis of string theory that string theory can be characterised as a degenerating research program (Cartwright and Frigg 2007, p. 20).

else, was responsible for the enormous interest in string theory during the 1980s.

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 the anti-de Sitter/conformal field theory correspondence (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).

Whereas 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 realization of a unified theory. As Richard Dawid has argued: "The disputes between critics and exponents of string physics in this light appear as disputes between defenders of the traditional paradigm of theory assessment and adherents of a newly emerging one." (Dawid 2013a, p. 82). According to Dawid, whereas "critics of string theory stick to the traditional understanding" of scientific progress based on empirical confirmation, string theorists see "their theory is the only viable option for constructing a unified theory of elementary particle interactions and gravity" (Dawid 2013a pp. 86–7).[12]

2.7 The Usefulness of String Theory

String theorists 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). Leonard Susskind points out, "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

12. Dawid bases this conclusion on the understanding that "supergravity cannot provide a satisfactory solution to the problems of non-renormalisability that arises in field theoretical attempts to carry out such unification." Moreover, "analysis within the framework of string physics could have but has not led to the emergence of alternative theories" and "there are vague arguments that even attempts to sacrifice very basic physical principles in order to find alternative scenarios to string theory would, if made coherent, lead back towards the string theoretical approach" (Dawid, 2013a, p. 88).

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" (Shifman 2012, p. 10).

The anti-de Sitter/conformal field correspondence (AdS/CFT duality), first proposed by Juan Mandelcena in 1997, marked a major theoretical breakthrough by providing physicists with a non-peturbative definition of string theory. 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).

Critics point out that these spin-offs have increasingly become largely divorced from the original program of string theory unification (Woit 2011). Topological string theory, for example, uses "simplified versions of string theory" that "do not unify the forces and particles observed in nature" (Smolin 2008, pp. 195–6). 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 2008, 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 2012). 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.

2.8 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).

Nevertheless, a few string theorists, most notably, Leonard Susskind, have explicitly attacked the appeal to falsifiability, and argued that the criticisms of string theory as unfalsifiable and therefore unscientific, 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"[13] (Feynman quoted in Susskind 2005, p. 192). 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, 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.

13. 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.

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 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.[14]

2.9 The Landscape of String Theory: Physics or Metaphysics?

In spite of Susskind'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 1999, pp. 284–5). 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 of the positive cosmological constant have 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."

14. Historians and philosophers of science, such as Peter Galison (1995) and Richard 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 recognizes. Dawid has developed this position in his recent work, *String Theory and the Scientific Method* (2013b).

However, much as we would have liked to make such predictions, "*the inability to do so doesn't render string theory non-scientific*" (Carroll 2005). 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 2005). 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" (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 space-time 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 fact 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 space-time 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" (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" (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 2007, 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 2011, 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 2008, p. 150).[15] Smolin calls for physicists to strongly resist "special pleading that the standards of science should be lessoned 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). Indeed some cosmologists, such as Aurélien Barrau, argue, "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). Both Smolin and Woit make a plea 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 2007, p. 216). Here they make explicit appeal not only to methodological, but also to sociological, norms of scientific inquiry. It is to this that we now turn our attention.

3. The Sociology of String Theory and the Scientific Ethos

3.1 The Crisis of String Theory?

This sociological dimension of the critique of string theory has been particularly emphasised by Smolin in his book *The Trouble with Physics*, but one can find similar criticisms from a number of authors (Smolin 2008). Here we find a different kind of boundary work, not between science and non-science, but between good science and pathological science. The fundamental question which Smolin seeks to address in his book is: "Why despite so much effort by thousands of the most talented and well-trained scientists, has fundamental physics made *so little definitive progress* in the last twenty-five years?" (Smolin 2008, p. 261). Smolin's diagnosis of this crisis in theoretical physics goes beyond methodological criticisms—he identifies a dysfunctional "sociology"

15. 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).

as responsible for the current situation. Here it is worth quoting Smolin at length:

> I have become convinced we have to talk about the sociology of theoretical physics, because the phenomena we refer to collectively as its "sociology" are having significant negative effects on its progress. Even though most string theorists are people of integrity who pursue their work with the best of intentions, there are aspects of the field's sociology that are aberrant, compared with the ideals that define the larger scientific community. These have led to pathologies in the methodology of theoretical physics that delay progress. The issue is not whether string theory is worth doing or should be supported, but why string theory, in spite of a dearth of experimental predictions, has monopolised the resources available to advance fundamental physics, thus choking off the investigation of equally promising alternative approaches. (Smolin 2008, pp. 267–8)

Smolin's sociological analysis, and the form of boundary work he engages in here (in distinguishing good from bad science), has both a descriptive and a normative aspect. It weaves together a *descriptive* sociological analysis of certain trends which have enabled string theory to maintain its position "as the dominant paradigm of theoretical physics" (Smolin 2008, p. 199), and a *normative* view of the scientific ethos. While Smolin's work constitutes the most elaborate attempt to develop this sort of sociological critique of string theory, we will draw on the writings of other physicists like Woit and Friedan, who have contributed to this sociological critique in certain ways, before turning our attention to the way string theorists have responded.

Smolin's agenda is stated clearly from the outset. Given the current situation, and the absence of experimental results, other theoretical approaches to quantum gravity deserve more: more funding, more publicity, more resources, more opportunities, and more recognition. Here we should note that while Smolin has published on string theory, his recent theoretical work is on loop quantum gravity—a research program that adopts a background-independent approach to quantum gravity. While he acknowledges that "string theory is certainly among the directions that deserve more investigation," he insists that "there is compelling evidence that something has gone wrong" (Smolin 2008, p. 198). With this in mind, Smolin calls for a more democratic view of theoretical research that encompasses a plurality of theoretical viewpoints. Unsurprisingly string theorists have dismissed Smolin's attacks as misguided, and have argued that, in spite of the obvious difficulties, string theory remains by far the most promising theoretical approach to a theory of quantum gravity.

3.2 A Sociology of String Theory

One of the striking aspects of Smolin's recent book is his focus on the sociological dimension of current practices in theoretical physics. Here Smolin emphasizes: "my concern is not with string theorists as individuals, some of whom are the most talented and accomplished physicists I know," but rather with a "trend in which only one direction of research is well supported while other promising approaches are starved" (Smolin 2008, p. xxiii). Here 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 the tenured positions offered.

In an atmosphere of intense competition for research positions, those that 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 2008, p. xx). As one *New York Times* article reported, "string theorists are already collecting the spoils that ordinarily go to the experimental victors, including federal grants, prestigious awards and tenured faculty positions" (Glanz 2001; quoted in Smolin 2008, p. 338) Institutional practices requiring positive references from those already established in the field and statistical measures of achievement such as levels of citation are largely to blame. These factors combine to ensure that the majority view continues to propagate. Smolin sees these practices and mindsets as detrimental to the field. As an advocate for a research program which is in the minority 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).

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 2007, 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 2007, p. 206). Put simply, "the difficulty of superstring theory [...] makes it hard for researchers to leave" (Woit 2007, 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 judgment 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 2007, pp. 205–6). 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 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 2003, 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 judgment 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."[16] 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

16. 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" (Shifman 2012, p. 2).

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). The dangers posed by this situation may be avoided, in Smolin's view, by a renewed commitment to the scientific ethos.

3.3 The Rhetorical Construction of the Scientific Ethos

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 2008, pp. 289–307). Instead, he insists, "the success of science is due to the formation of communities tied together by ethical principles." This sociological conception of science, Smolin argues, is "the major theme of the book" (Smolin 2007a). Scientific progress is contingent on the existence of a scientific community "that is defined and maintained by adherence to a shared ethic" (Smolin 2008, p. 301). Smolin's commitment to the scientific ethos, rather than any one version of the scientific method puts him closer to a Mertonian than a Popperian view of science. "The ethos of science" as Merton defined it, is constituted by the "complex of vales and norms which is held to be binding on the man of science." Such values and norms "are legitimized in terms of institutional vales" (Prelli 1989, p. 87).

Smolin articulates the core values he sees as underpinning the scientific ethos. "If we are forced to reach a consensus by the evidence, we should do so" (Smolin et al. 2007, p. 4). If, however, "rational argument from the publicly available evidence does not succeed in bringing people of good faith to agreement on an issue, society must allow and even encourage people to draw diverse conclusions" (Smolin 2008, 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. Contemporary theoretical physics has, according to Smolin, failed to adhere to the democratic values of the scientific ethos.

In engaging in this kind of discourse, Smolin engages in what Lawrence Prelli has appropriately termed, "the rhetorical construction of the scientific ethos." This discourse is characterized by the attempt to define the set of norms and values, which are taken to be constitutive of "good science." Here we have a different kind of boundary work to that examined in the previous section. Protagonists on both sides seek to construct an idealized image of science for their readers. Such constructions serve an important rhetorical function, which can "be explained only partly by the [...] need to address the scientific laity that is incapable of following the relevant technical arguments" (Prelli 1989, p. 98). As Prelli explains:

> [...] when scientists resort to these common themes in discussing, justifying or evaluating actions, the alleged norms and counter-norms of science serve a *rhetorical* function, regardless of whatever other functions they may be said to serve [...]. Scientific *ethos* is not given; it is constructed rhetorically [...]. Whatever is said or done to influence perception of a scientist's *ethos* will arise from a finite set of values implied by the notion of doing "good science" [...]. [Typically] attention to the constituents of the scientific ethos becomes salient only when the discourse of one scientist is made and evaluated by others in scientific situations that are rhetorical; that is problematic or ambiguous situations that involve inducing adherence to ideas presented as "scientific." (Prelli 1989, pp. 88–9)

Prelli's analysis provides an extension of Gieryn's notion of boundary work, through which we can better appreciate an important aspect of the string theory controversy. Indeed, one may also find examples of other virtues or norms, which physicists take to be integral to the scientific ethos. As we shall see, critics such as Woit and Friedan also construct notions of the scientific ethos, embodying norms such as honesty, humility, and open-mindedness, which they allege have not been adhered to by the string theory community. String theorists like Polchinksi, Gross, Susskind, and Duff have responded to such charges, by providing alternative sociological views of string theory, and taking issue with Smolin's view of the scientific ethos.

Smolin has been especially critical of many recent popularizations of string theory, which in his view have tended to overstate their claims to have definitively solved a range of crucial problems such as quantum gravity, black hole entropy, moduli stablization, background-independence in presenting a misleading image of string theory as triumphantly marching towards a "theory of everything" (Duff 2011a, p. 210). Here he reminds the reader that "we physicists require significant resources, which are provided largely by our fellow citizens" and 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." Regrettably, some physicists "have been less than careful about explaining just how far the new ideas are from experimental and mathematical proof" (Smolin 2008, xxi–xxii). Woit makes a similar complaint in his blog a category titled "This Week's Hype" where he provides links to various popular pieces that engage in hubris about string theory without making important qualifications (Woit 2004–2013). 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. As we shall see, string theorists have responded by accusing their critics of systematic bias, and misinformed and damaging distortions.

Woit sees the refusal of the theoretical physics community to acknowledge the failures of string theory as perhaps the most disturbing recent trend in recent years. Like Smolin, 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). Another critic of string theory, Dan Friedan, has stressed the importance of recognizing failure as an integral "part of the scientific strategy". Scientists, according to Friedan, have "*a responsibility to recognize failure. Recognizing failure is an essential part of the scientific ethos*" (Friedan 2003, p. 8; emphasis added). This offers yet another characterization of the scientific ethos, and the values that underpin it. Friedan argues that the refusal to recognize failure is detrimental to scientific progress. Friedan's view can be usefully contrasted with that articulated at the "Strings2003" 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" (quoted in Woit 2007, p. 10). Here Gross identified persistence, not a readiness to acknowledge failure, as the virtue most befitting the theoretical physicist.

3.4 A Sociological Defence of String Theory

Not surprisingly, string theorists and 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 characterization of string theory, his allegations of institutional bias and self-serving hiring practices, and claim has made virtually no progress over the last twenty-five years has infuriated many theorists, many of whom have simply refused to engage in public 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 (Smolin et al. 2007, p. 5). As Polchinski puts it, "much of what Smolin and Woit attribute to sociology is really a difference of scientific judgment" (2007a). 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 realizing the goal of a unified theory of quantum gravity.

In an exchange with Smolin following the publication of *The Trouble with Physics* 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-stablization problem, were in fact, successfully solved once the appropriate tools became available. In his reply to Smolin, Polchinski writes, 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 response to Smolin's characterization 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. (2007b)

Here Polchinksi offers both a *sociological description* of the string theory community and a *normative* account of the scientific ethos. In addressing the first, Polchinski draws attention to the diversity of approaches within string theory, as well as 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 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 characterization of the ethos of science is in some respects similar to Smolin's. Here Polchinski demands that "scientists take responsibility for what they say." 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 (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 sociological factors have 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). Here it is clear that for Polchinski, it is Smolin, not string theory community, who is guilty of an ethical failing.

In a similar vein, Sean Carroll has argued that string theory has become the dominant paradigm of theoretical physics "for intellectual reasons, not socio-psycho-political ones". 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 2006). This is an ethotic argument of a different sort. Rather than direct their attacks on the scientific community at large, defenders of string theory appeal to the critical consensus that has emerged in the theoretical community, and launch a counter-attack on the credibility of the critics. 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 of what is really going on, along with an alternative view of the scientific ethos. He makes an implicit appeal to the credibility and reputation of America's leading research institutions such as Berkeley, Princeton, Stanford and Harvard in defending the legitimacy of string theory. Critics are here compared to "disgruntled," "angry," "conspiracy theorists." Susskind here implies that there are ulterior motives at play, thus rendering their judgments as suspect. As Geoff Brumfiel has commented, the recent criticisms of string theory are "written by outsiders" and "have stirred deep resentment in the tight knit community" (Brumfiel 2005, p. 491). Smolin is typically portrayed as an outsider in spite of the fact that he has published on string theory.[17] In defence of Smolin, Michel André, Adviser to the Director General of the European Union responsible for research policy issues, has argued that: "Smolin has all too readily been labelled a frustrated scientist bent on revenge for his lack of personal recognition" (Duff 2013, p. 197). Here the debate becomes a debate over credibility.

As the debate spilled over into the public arena, a number of defenders of string theory have also attempted to further discredit their critics by pointing out that in going public they have bypassed the established channels for scientific criticism. Woit's criticisms of string theory have been dismissed by some physicists because his primary medium is a blog, and that makes them science journalism rather than science proper. In his reply to critics, Duff emphasizes the problems associated with the public nature of the controversy: "The internet, where everyone is an expert, now provides their ideal forum. 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. 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. To this extent, serious commentary should not be left to those who have little or no grasp of the physics involved.

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" (2011a, 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 2006). 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.[18] Here we may characterize the situation in the terms set out by

17. As Prelli has noted, scientific "insiders" and "outsiders" typically "construct conflicting perspectives on scientific *ethos*" (Prelli 1989, p. 97).

18. A deeper analysis of the debate over what constitutes a background-independent theory, and how the problem is construed differently by different researchers is beyond the scope of this paper, however, we plan to discuss its implications for theory appraisal in a forthcoming paper.

Larry Laudan. Different judgments about the progress of string theory rest on "divergent views about the attributes our theories should possess" (Laudan 1990, p. 42).

4. 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 paper, the debate raises a number of issues that go well beyond the sphere of theoretical physics. Prescriptions concerning the nature of scientific progress, the demarcation of science from non-science, the sociological norms of scientific inquiry and the scientific ethos feature prominently. 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, good science and bad 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.

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 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 the normative structure of 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 they could 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. Here we see 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. 177–9).

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 "bad science." 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.

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