Mirror Symmetry and Other Miracles in Superstring Theory

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Abstract The dominance of string theory in the research landscape of quantum gravity physics (despite any direct experimental evidence) can, I think, be justified in a variety of ways. Here I focus on an argument from mathematical fertility, broadly similar to Hilary Putnam’s ‘no miracles argument’ that, I argue, many string theorists in fact espouse in some form or other. String theory has generated many surprising, useful, and well-confirmed mathematical ‘predictions’—here I focus on mirror symmetry and the mirror theorem. These predictions were made on the basis of general physical principles entering into string theory. The success of the mathematical predictions are then seen as evidence for the framework that generated them. I shall attempt to defend this argument, but there are nonetheless some serious objections to be faced. These objections can only be evaded at a considerably high (philosophical) price.

Keywords String theory · Mirror symmetry · No miracles argument

1 Introduction

The debate over the scientific status of string theory—whether it is ‘genuine’ science, or pure mathematics, or, worse, mere metaphysics—continues unabated. At the core of the troubles is the claim that string theory lacks an experimental basis. But string
theorists are not under any illusions about this situation. In this paper I argue that there are subtler, but nonetheless perfectly rational principles underpinning faith in string theory. A curious combination of physical constraints and mathematical consistency has led string theory to some rather spectacular mathematical insights. This interplay between physics and mathematics plays an important role in string theorists’ self-appraisal of their work. I attempt to link this alleged methodology of string theory up to a series of contemporary debates in the philosophy of science and mathematics.

Philosophers of science must bear a sizeable portion of the brunt of the blame over the unwarranted rejection of string theory, for it is the traditional methodological rules developed by philosophers (based around empirical confirmation or disconfirmation) that now underlie many scientist’s rejections. I try to show how there are other philosophical views, and other methodological virtues, that show string theory in a better light.

The paper proceeds as follows. In Sect. 2 I explain why the demand for precise, quantitative experiments in the context of quantum gravity is misguided. In Sect. 3 I give a capsule presentation of the early history of string theory, highlighting the physical principles that form its core. In Sect. 4 I introduce those portions of string theory that will be needed to make sense of the core example. Then in Sect. 5 I present this example. I then introduce the no-miracles argument (from mathematical success) in Sect. 6 and attempt to defend it from a range of objections. Section 7 turns the focus on the related issue of testability. Finally, in Sect. 8 I offer a positive evaluation of the status of string theory focusing on combinations of evidence, rather than any singular contribution.

2 Experimental Distance in Quantum Gravity

Like most (if not all) quantum gravity research, string theory is bound to increase the amount of indirectness between theory and experiment. As the authors of the first textbook on superstring theory wrote: “Quantum gravity has always been a theorists puzzle par excellence. Experiment offers little guidance” ([20], p. 14). The scale at which unique, quantitatively determinable new predictions are made is well beyond the reach of any experiment, past, present, or (conceivable) future. We should therefore expect that new methods guiding theory evaluation will arise to compensate for this.¹ Schrödinger inadvertently pointed toward this issue in 1955:

It might [have been] the case that in experimental physics the method for establishing laws were the same as in astronomy. . . . But it is not so. And that is small wonder. The physicist has full liberty to interfere with his object and to set the conditions of experiment at will. ([44], p. 13)

If the physicist loses the ability to interfere with his objects (as in string theory), then the implication would seem to be that the method of establishing laws and facts must thereby be modified. In this case, there would appear to be three broad strategies:

¹Of course, the standard collider methods in particle physics have an in-built ‘Moore’s law-style’ termination point since deeper scales require ever higher energies.
1. Shift to the observational methods of precisely the kind relied upon by astronomers and cosmologists.

2. Reduce the emphasis placed on quantitative predictions (in favour of weaker, qualitative predictions).

3. Attempt to utilise a range of other theoretical virtues, such as the ability of a theory to unify a broad range of disparate (old) knowledge.

String theory makes use of all three of these methods in varying degrees: along the lines of the first strategy, though still in an embryonic stage, string cosmology is emerging (in which the exceptionally large energies/small scales of the very early universe are utilised in a bid to find stringy remnants: cf. [52], pp. 18–19); in the second case one can point to supersymmetry, holography, and quantum geometry, in the latter case, the theoretical unification of gauge theory and gravity.²

It is well-known, then, that string theory doesn’t have an experimental leg to stand on, at least not by way of novel experimentally testable predictions.³ And there are intuitively obvious reasons why this should be so, as a result of the scales involved in the new physics. It is also the case that string theory scores highly on its ability to unify ‘old evidence’, and it is generally understood that this puzzle-solving ability is what gives string theory its credibility. Though there is certainly truth in this claim, in this paper I present an alternative account for the credibility of string theory, and argue that it is more likely than not the argument that underlies the faith of string theorists, and also mathematicians who study the theory. The argument I present bears striking similarity to the Smart-Putnam ‘no-miracles argument’ from the philosophy of science literature [39, 47]. The crucial difference is that the ‘miracles’ are not surprising physical facts but surprising mathematical facts instead. After presenting this mathematical version of the no-miracles argument I then attempt to defend it as offering support to string theory qua physical theory.

I should note, however, that not having a firm experimental basis does not imply a disconnection from physical reality. There are countless features of the world at lower energies (that the Planck scale and the several orders of magnitude above) that string theory must either accommodate or be consistent with. The mathematical

²Physicists often refer to these as ‘retrodictions’, though philosophers refer to such instances as ‘accommodations’. I will discuss, in Sect. 6, the issue of the relative weights assigned to prediction on the one hand and accommodation on the other, for it is a matter that divides philosophers of science (and statisticians)—see [27]. Note, in any case, that philosophers and historians of science are usually suspicious of anything claiming to be ‘the scientific method’: the notions of ‘testability’ and ‘falsifiability’ are, in particular, notoriously flawed. (It is somewhat shocking to see that in his review of Lee Smolin’s The Trouble with Physics, Michael Riordan goes so far as to claim that string theory is scientifically on a par with the theory of intelligent design [42], p. 39)! He sees science as tantamount to the production of testable predictions. This demonstrates a woeful ignorance of much of the painstaking work that historians and philosophers of science, since Pierre Duhem and, more obviously, Kuhn, have carried out.)

³I’m referring to string theory qua quantum theory of gravity (or TOE). There are several instances of string theoretic models being usefully employed to make empirical predictions. For example in the study of quark-gluon plasmas in heavy ion physics (such as those produced at RHIC) [34], and most recently in high $T_c$ superconductivity [22]. It is essentially the 2D conformal invariance, coupled with the holographic principle that does the work here—both of which are, of course, more general than superstring theory. However, since string theory implies these features, one could still make a case that they ‘weakly confirm’ the framework of string theory.
structure of string theory is guided in very large part by the desire to achieve consistency with putative physical laws—i.e. the most general principles of physics, such as unitarity and Lorentz invariance—and known physical data (including the number of spacetime dimensions and the number of particle types and their various properties). It is precisely this feature that prevents string theory from degenerating into Laputian speculation.

3 The Colourful Early History of String Theory

In the 1950s particle physics underwent a significant change; the development of large particle accelerators made it possible to create new hitherto unseen particles. These new particles posed peculiar novel problems for theorists: the particle types were too large in number and their properties (spin against mass squared) fell into patterns (such as the linear Regge trajectories represented on a Chew-Frautschi plot) that did not fit into any of the then standard frameworks provided by quantum field theory—indeed, many spins seemed too high to described by consistent quantum field theories. This led to the development of S-matrix based approaches, in which data coupled with axioms concerning the structure of the S-matrix were used to derive physical predictions. The final stage of this programme was the construction of the so-called dual resonance models, which were able to combine the various desirable properties of the S-matrix for strong interaction physics. The dual resonance model was soon seen to be derivable from a dynamical theory of strings. However, it had several features (the wrong particle spectrum, too many Lorentz dimensions, and other empirical inadequacies) that made it an unlikely candidate for describing the strong force. The emergence of ‘colour physics’ proved to be the death knell of string theory as a theory of hadrons.

The two key vices of the early string theory, the particle spectrum problem (notably the existence of a massless spin-2 particle) and the expansion in the number of Lorentz dimensions (to uphold Lorentz invariance in the light cone gauge formalism), were turned, by Joël Scherk and John Schwarz [46] (and independently, by Tamiaki Yoneya [47], into virtues (cf. [45], p. 269) of a new theory with a different target: (quantum) gravity. Though the new product emerged from a retuning of a free
parameter (the string tension), we should really view this as an entirely new theory, for the intended systems that the theory is seen to apply to are entirely different in kind. Despite this, there are still general physical principles (originally seen to stem from physically necessary properties of interacting hadrons) that form the basic mathematical structure of the new theory. That is to say, the principles deemed physically necessary in the hadronic case were of a sufficiently general nature to be applicable in the case of the new quantum gravity theory.

The refashioning of the theory into a theory of gravity was fairly natural since both involve the imposition of dynamical consistency conditions on the structure of spacetime. The geometrical basis of string theory was put on firm footing by Alexander Polyakov in 1981: [37, 38]. String theory is, according to this approach, the theory of 2D Riemann surfaces (the worldsheets of the evolving strings) in a 10 or 26 dimensional (target) spacetime. We introduce some bare details of this approach in the next section. But we note here that the modelling of string dynamics as a theory of embedded 2D Riemann surfaces brings with it copious amounts of extremely interesting mathematical machinery, having to do with 2D conformal symmetry, modular invariance, Kac-Moody algebras, and more. The ‘targeting’ of mathematical structures by physical principles and physical data (responsible for the overall structure of string theory) is not unique to string theory, of course. It is a general feature of physical theories that they pick out mathematical structures that are used to represent physical systems described by the theory. The mathematical structures identified by string theory, however, are especially powerful and fruitful.

4 What is String Theory?

We saw above that string theory developed in a rather peculiar way. The historical complexities don’t end with its transition from a theory of hadrons to a quantum gravity theory, but we shall leave these aside in the following and present a fairly streamlined formulation of the basic ideas behind string theory. The focus is on bringing to the surface those elements that are relevant to the example of mirror symmetry presented further on.

The fundamental idea of string theory is, of course, simple enough: instead of a local quantum field theory of point-like particles, the theory employs one-dimensional objects, that can be open or closed. Whereas in the quantum field theory of point-like excitations we have worldlines, meeting at single distinguished points (the vertices at which interactions happen, with a strength determined by the coupling constant of the relevant theory), in string theory one has worldsheets (for open strings) and worldtubes (for closed strings). In the latter case the interactions do not happen at some

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Schwarz and Scherk paper that is usually credited as the first ‘string theory as a quantum theory of gravity’ paper—that certain dual resonance models (the Virasoro-Shapiro model) contained Einstein’s theory of gravity as a zero slope limit. This paper marks the birth of string gravity, at least in the published record.

Of course, there can often be interesting multiplicity in this respect, as in the case of quantum mechanics in the Schrödinger, Hamiltonian, and Feynman path-integral approaches. As we see in the discussion of dualities below, a similar multiplicity appears in string theory, though in this case we find apparently distinct theories (though related by a duality symmetry) that represent the same physical system.
single point, and the question of whether or not there are interactions is answered by global properties of the worldsheets and worldtubes—specifically the genus $g$ (or number of holes) of the surface.

Like standard quantum field theory, string theory too is usually presented in a perturbative fashion, expanding out string worldsheets in powers of the coupling constant $g_s$ of the theory. We can make sense of this idea by introducing the sigma-model. This will give us a description of string dynamics in terms of mappings of Riemann surfaces into spacetime. Quantum theory and the invariances of the classical theory then impose conditions on the nature of the spacetime.

The initial step is to consider a map $\Phi$ from a complex curve (a Riemann surface) $\Sigma$ into the ambient target space $X$ (with metric $G$ and additional background fields $B^i$) representing the 2-real-dimensional string worldsheet:\footnote{This worldsheet has a metric $h_{\alpha\beta}$ defined on it in the so-called Polyakov version. In the original Nambu-Goto version the worldsheet was metric-free. The surface also has a genus $g$ which plays a crucial role in the quantum theory and in the generation of low-energy physics.}

$$\Phi : \Sigma \longrightarrow X$$

(1)

The action is then a function of this map (including the worldsheet’s metric), given the background fields $G$ and $B^i$:

$$S(\Phi, G, B^i)$$

(2)

The $\Phi$ field gives the dynamics of a 2-dimensional field theory of the worldsheet relative to the fixed background fields, one of which is the metric. The quantum theory is then given by the path-integral (over moduli space: i.e. the space of inequivalent 2D Riemann surfaces)$^{11}$:

$$P(X) = \sum_g \int_{\text{moduli}_g} \int D\Phi e^{iS(\Phi, G, B^i)}$$

(3)

There are many consistency conditions that must be met by such string models. The most important of these concerns the restriction of the number of Lorentz dimensions in order to resolve the conformal anomaly (i.e. the breakdown of conformal symmetry

\footnote{I should perhaps point out that this perturbative ‘worldsheet’ formulation is somewhat less fashionable these days, since computations are so difficult to carry out within it—Oswaldo Zapata notes that the pure spinor formalism of Nathan Berkovits may help alleviate some of the computational intractability (personal communication). However, it is at least fairly well-defined and enables one to see in a fairly visual way how the interesting elements of mathematics (such as Riemann surfaces, modular invariance, and the like) enter into string theory and then find reapplication in pure mathematics. Though I don’t discuss it here, the modular invariance lies beneath some of the deepest connections between physics and mathematics, and is connected also to $S$-duality (the strong-weak coupling duality).}

\footnote{Note that the physical quantities of quantized string theory are functionals on Teichmüller space, i.e. the orbit space of metrics modulo conformal and diffeomorphisms symmetries. Moduli space is more tightly circumscribed, involving also ‘large’ diffeomorphisms (those not connected to the identity). When one further quotients Teichmüller space by the modular group of transformations, one has moduli space (of a Riemann surface), over which the path-integral in (3) is performed. Mirror symmetry (and other string dualities) will have the effect of producing further identifications of points in the parameter space of a string theory.}
in the quantum theory): 26 (in the bosonic ‘string theory’ case) or 10 (in the bosons + fermions ‘superstring theory’ case).

In order to preserve broad qualitative properties—such as the appearance of 4D spacetime, the empirical adequacy of general relativity at low energies—one needs to compactify the residual dimensions on a manifold with a very stringent structure. Calabi-Yau manifolds are the natural candidates for the compact, internal manifold that are demanded by internal and external (i.e. phenomenological) consistency. It’s invariant properties are responsible for determining the observable low energy physics in the non-compact, 4-dimensional manifold we ordinarily call spacetime. Let us spell out the details of this compactification strategy a little more, since it is utilised in the example of mirror symmetry that forms the basis of the central argument of this paper.

Quantum superstring theory remains Lorentz invariant only if spacetime has 10 dimensions. To construct a realistic theory therefore demands that the vacuum state (i.e. the vacuum solution of the classical string equations of motion, supplying the background for the superstrings) is given by a product space of the form \( M \times K \), where \( M \) is a non-compact four dimensional Minkowskian (or possible more general) space-time and \( K \) is a compact 6-real dimensional manifold. One gets the physics ‘out’ of this via topological invariants of \( K \) and gauge fields living on \( K \). One chooses the specific form of the compact manifold to match the observed phenomena in \( M \) as closely as possible. For example, if one wants \( N = 1 \) supersymmetry in the non-compact dimensions \( M \), then one requires a very special geometry for the compact dimensions \( K \), namely a Calabi-Yau manifold mentioned above. This is defined to be a compact Kähler manifold with trivial first Chern class—which is just mathematical shorthand for saying that we want to get our low-energy physics (Ricci flatness\(^{12}\) and the single supersymmetry) \(^{12}\) out of the compact dimensions.

There are five quantum-mechanically consistent superstring theories (in 10 dimensions: we ignore the purely bosonic case): Type I, \( SO(32) \)-Heterotic, \( E_8 \times E_8 \)-Heterotic, Type IIA and Type IIB. The Type I theory and the heterotic theories differ from the Type II theories in the number of supersymmetries, and therefore in the number of conserved charges. One is able to compute physical quantities from these theories using perturbation expansions in the string coupling constant. Given the extensive symmetries of this (worldsheet) string theory (i.e. diffeomorphism and conformal symmetries), there is just a single Riemann surface for each order of the expansion—that is, the initially distinct diagrams can be topologically deformed into one another since there are no singularities representing interaction points: interactions are determined by global topological considerations of the world sheet (such as the number of handles), rather than local singularities.\(^{13}\) By looking at these expansions, in the ‘different’ theories, one can find cases where the physics is identical so

\(^{12}\)The first Chern class \( c_1(X) \) of a metric-manifold is represented by the 2-form \( 1/2\pi \rho \) (with \( \rho \) the Ricci tensor \( R_{ij} dz^i \wedge d\bar{z}^j \)). Calabi and Yau determined the various interrelations between Chern classes, Kählericity, and Ricci forms. If one has a Ricci flat metric then one also gets the desired single supersymmetry since Ricci flatness is a sufficient condition for an \( SU(3) \) holonomy group. Any textbook on complex algebraic geometry will explain these matters in detail—\([4]\) and \([32]\) are good sources of information.

\(^{13}\)Note, this is true for all but the Type I theory since its strings can be opened up. However, this does not need to concern us in what follows.
long as one makes transformations of a certain kind. Since these transformations are not taking us to a physically distinct state and relate states in different theories, they are referred to as ‘dualities’.14

5 Mirror Symmetry

As is well known from Kaluza-Klein compactifications onto circles (with particles), momentum is quantized according to the relation \( p = n/R \) (where \( n \in \mathbb{N} \) and \( R \in \mathbb{R} \) is the radius). If we then consider the mass-energy of a system in such a compactified configuration then we must add an additional term corresponding to these so-called ‘Kaluza-Klein modes’:

\[
E^2 = M^2 + \frac{n^2}{R} \tag{4}
\]

So far what we have said applies to particles as well as to strings. Strings, however, have an additional property not shared by particles: they can wrap around the compact dimension. This brings with it another term: the ‘winding modes’ (where \( m \) counts the number of such windings and \( E \propto mR \) since \( E = mR \) vanishes at \( R = 0 \)). These must be added to the total energy-mass, giving the following equation for computing the mass-energy spectrum (\( \alpha' \) is the string coupling constant):

\[
E^2 = M^2 + \frac{n^2}{R} + \left( \frac{mR}{\alpha'} \right)^2 \tag{5}
\]

If we then make the following (duality) transformations we leave the energy invariant:

\[
R \longleftrightarrow \alpha' \frac{R}{\alpha'} \tag{6}
\]

\[
m \longleftrightarrow n \tag{7}
\]

since we then have:

\[
E^2 = M^2 + \frac{m^2}{\alpha'} + \left( \frac{n\alpha'}{\alpha'} \right)^2 \tag{8}
\]

This can easily be seen to be equivalent to the original expression, hence the energy is invariant. This is a fairly obvious consequence which we could easily guess at without going through the computation: since the Kaluza-Klein modes are inversely proportional to the radius while the winding modes are directly proportional, the switching of modes combined with the switching of the radius to the inverse radius will leave any function of such terms invariant. So expressed, the phenomenon of T-duality loses a little of its surprising nature, though it is certainly not without philosophical interest.

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14They have similarities with symmetries and gauge redundancies. However, with gauge redundancies we view the gauge related situations to represent one and the same physical state of affairs. In sting dual cases this does not seem possible since the ‘dual objects’ can have distinct dimensionalities, sizes, and even differ as to whether they contain gravity or not, or are quantum mechanical or not.
(nor potentially physical application, especially in cosmology). Mirror symmetry is far more puzzling.

The discovery of mirror symmetry is one of the jewels in the crown of string theory’s relationship with mathematics. It is essentially a generalization of T-duality from homeomorphic manifolds to topologically inequivalent manifolds. The are two prongs to the importance of mirror symmetry from the point of view of evidential support offered to string theory. On the one hand is the basic discovery of the relation between \textit{a priori} unrelated mathematical objects as a result of string theoretic investigations. On the other hand is the ‘practical’ application of this principle (before the availability of a rigorous mathematical proof of the ‘mirror principle’) to an unresolved problem in enumerative geometry.

Recall that a phenomenologically respectable string theory requires that six of the 10 dimensions be hidden from the view of low-energy physics. This is done via compactification, writing the 10 dimensional spacetime $M_{10}$ (required by quantum consistency) as a product space of the form $M^4 \times K^6$, where $M^4$ is flat Minkowski spacetime and $K^6$ is some compact 6 real-dimensional space. $M^4 \times K^6$ then forms the background space (the ground state in fact) for the classical string equations of motion. One chooses $K^6$ so that its geometrical and topological structure determine (when strings are threaded through it) the physics in the four non-compact spacetime dimensions (i.e. the ‘low-energy’ physics). By choosing in the right way one can get explanations for a host of previously inexplicable features of low-energy physics, such as the numbers of generations of particles in the standard model, the various symmetry groups of the strong, electroweak, and gravitational forces, and the masses and lifetimes of various particles.

Calabi-Yau manifolds were found to be of importance in string theories since they allow for $\mathcal{N} = 1$ supersymmetries in four spacetime dimensions and many other nice properties. Calabi-Yau manifolds are compact spaces satisfying the conditions of Ricci-flatness (to accommodate general relativity at the phenomenological 4D level) and Kählericity (generating the $\mathcal{N} = 1$ supersymmetry in the non-compact dimensions). The problem is, there is a huge number of Calabi-Yau spaces (in $D = 6$) meeting the required conditions, so the selection of a single such space is a difficult task. However, what I want to discuss here is the identification of various of these, seemingly very different, manifolds via mirror symmetry (roughly, identifying manifolds that differ by a change of sign of a certain parameter).

To characterize manifolds one needs to know about their topological structure. To pick out this structure one looks for the invariants, of which there are various kinds. For example, a real 2-dimensional manifold is specified by its genus. In string theory, the topological and complex structure of the compact manifold determines the low energy physics in the real, four non-compact dimensions. What was required by the string theorists, in order to remain consistent the observed particle physics, was a Calabi-Yau space with an Euler characteristic $\chi$ of $\pm 6$. These can be found (and were found by Yau himself). However, there is an entire family of ‘mirror’ Calabi-Yau spaces with opposite Euler number. These look distinct from a topological and complex structure perspective, but from the point of view of the (worldsheet) string theory living on these spaces, the difference is illusory: the field theory is \textit{insensitive} to the mirror mapping (i.e. it possesses mirror duality) and is, in this sense, background independent. This is a crucial point: the physics of string propagation allows
one to probe the structure of Calabi-Yau spaces in a deeper way than previously possible, and in so doing it suggested that certain such (apparently very different) spaces were in fact equivalent.

The concept of the Hodge diamond makes the phenomenon of mirror symmetry easy to see in a visual way, and was in fact named as a result of this visual appearance. Hodge numbers are to (complex) Kähler manifolds what Betti numbers are to real manifolds: they specify topological invariants of the manifold and correspond to the dimension of the relevant cohomology group. The Betti numbers count the number of irreducible \( n \)-cycles of some manifold—see Fig. 1.

The \( n \)-cycles themselves are defined as ‘chains’ without boundary, where chains themselves are sums of (oriented) submanifolds of the manifold. So, for example, \( H_{n=0} \), a 0-cycle is a 0-chain and is simply a point—note, cycles are considered equivalent if they differ by a boundary; so, for example, for a \textit{connected} manifold, all points occupy the same equivalence class. The Hodge numbers perform the same function, but for complex cycles \( p \) and their complex conjugates \( \bar{p} = q \). The Betti number \( b_n \) is defined as the dimension of the DeRham cohomology group \( H^n_D \):

\[
b_n \equiv \dim(H^n_D)
\]

(9)

The Hodge number \( h^{p,q} \) is defined as the dimension of the Dolbeault Cohomology Group, \( H^{p,q} \):

\[
h^{p,q} = \dim(H^{p,q})
\]

(10)

The Betti number and the Hodge number are then related (by ‘Hodge decomposition’) as:

\[
b_n = \sum_{p+q=n} h^{p,q}
\]

(11)

The Hodge diamond encodes these various Hodge numbers as follows:

\[
\begin{array}{cccccc}
  & h^{3,3} & h^{3,2} & h^{3,1} & h^{3,0} & \\
  h^{2,3} & h^{2,2} & h^{2,1} & h^{2,0} & \\
  h^{1,3} & h^{1,2} & h^{1,1} & h^{1,0} & \\
  h^{0,3} & h^{0,2} & h^{0,1} & h^{0,0} & \\
  & h^1 & h^0 & h^1 & h^0 & h^1 & h^0
\end{array}
\]

(12)
For a complex 3-dimensional manifold, we can compute the entries of the Hodge diamond via the Hodge decomposition (and various dualities—see below), giving:

\[
\begin{align*}
  b_0 &= 1 \\
  b_1 &= 0 \\
  b_2 &= h^{1,1} \\
  b_3 &= 2(1 + h^{2,1}) \\
  b_4 &= h^{2,2} = h^{1,1} \\
  b_5 &= 0 \\
  b_6 &= 1
\end{align*}
\]  

(13)

The only independent Hodge numbers of the 3-manifold (with non-vanishing Euler characteristic—see below) are \(h^{1,1}\) (roughly describing, via a number of real parameters, the size, or radius, and shape of the manifold) and \(h^{2,1}\) (roughly the number of complex parameters to describe the complex structures that can be defined on the manifold). The other numbers are set by various mathematical identities and properties:

- Complex conjugation gives the duality: \(h^{p,q} = h^{q,p}\).
- Poincaré duality gives: \(h^{p,q} = h^{3-p,3-q}\) by Poincaré duality (also giving us the identity \(h^{1,1} = h^{2,2}\) above).
- The condition of vanishing first Chern class sets up an isomorphism between entries \(h^{0,p}\) and \(h^{0,3-p}\).

Hence, we have:

\[
\begin{array}{cccc}
  & 1 & & \\
 0 & & 0 & \\
0 & h^{1,1} & 0 & \\
1 & h^{2,1} & h^{2,1} & 1 \\
0 & & h^{1,1} & 0 \\
0 & 0 & & \\
& & & 1
\end{array}
\]  

(14)

It is a claim of algebraic geometry, having its origin in string theory, that every space described by such a Hodge diamond has a ‘mirror’ (with the axis of reflection lying along the central diagonal). The phenomenon of mirror symmetry then refers to an isomorphism between pairs of conformal field theories (worldsheet string theories) defined on prima facie very distinct Calabi-Yau manifolds, differing even with respect to their deep topological structure. In this case the manifolds have their Hodge numbers switched as:

\[
H^{p,q}(M) \xrightarrow{\text{Mirror Dual}} H^{n-p,q}(\tilde{M})
\]  

(15)

Where \(n\) is the (complex) dimension of the manifold. In the case where this is 3, we find that the remaining Hodge numbers \(h^{1,1}\) and \(h^{2,1}\) are intertransforable. These
The torus (with top and bottom and left and right identified) is an example of a 1-dimensional Calabi-Yau manifold. Deformations of the Kähler form of the torus change the volume while leaving the shape invariant (that is, the angles between the independent cycles are constant). A complex structure deformation does the opposite: it changes the shape (the angles) while leaving the volume invariant. (Adapted from Greene [21], p. 25.)

numbers parametrize the size and shape of the compact space, along with its complex structural properties—see Fig. 2.\textsuperscript{15} Mirror symmetry tells us that the physics (of relativistic quantum strings) is invariant when these, apparently very different (with different corresponding classical theories), features are exchanged.

Since the Euler number $\chi$ for a real manifold is computed via the Betti numbers as:

$$\chi = \sum_n (-1)^n b_n$$

the Euler characteristic for a complex Kähler manifold can be computed, again invoking Hodge decomposition, as:

$$\chi_k = \sum_{p,q} (-1)^{p+q} h^{p,q}$$

This number is, as mentioned above, crucial in the mapping to real-world, low-energy physics: the Euler character is equal to twice the number of particle generations. It can be connected to these shape and size parameters as follows:

$$\frac{|\chi|}{2} = |(h^{1,1} + h^{2,1})| = |(h^{1,1} - h^{2,1})| = \frac{|-\chi|}{2} = \text{No. Gen.}$$

To achieve a realistic string theory, then, one needs to find a Calabi-Yau manifold with $h^{1,1}$ and $h^{2,1}$ satisfying:

$$|(h^{1,1} + h^{2,1})| = 3$$

Gang Tian and Shing-Tung Yau discovered such a manifold [55]. Though there is degeneracy here too, with multiple candidates available.

\textsuperscript{15}They correspond to topologically nontrivial 2-cycles and 3-cycles respectively.
This setup was used to great (and surprising) effect to resolve a problem in pure mathematics, in the field of enumerative geometry. Briefly, what are now known as Gromov-Witten invariants (interpreted as topological string amplitudes) were used to calculate the number of curves of a given degree intersecting a particular surface. Using string theory, Candelas et al. [9] constructed a generating function to find the number of curves \( n \) for all degrees \( d \) through a particular surface, a well-known Calabi-Yau complex 3-manifold known as a quintic (in fact, the simplest possible Calabi-Yau manifold), defined by the equation:

\[
x_o^5 + x_1^5 + x_2^5 + x_3^5 + x_4^5 = 0 \supset \mathbb{P}^4 \tag{20}
\]

The function they came up with was based on string perturbation theory (that is, a sum-over-Riemann-surfaces approach):

\[
K(q) = 5 + \sum_{d=1}^{\infty} n_d d^3 \frac{q^d}{1 - q^d} \tag{21}
\]

Mathematically, \( n_d \) is the number of rational curves of degree \( d \), and \( q = e^{2\pi i t} \). In terms of the physics, \( n_d \) is the ‘instanton number’, pertaining to the quantum corrections. Each curve of degree \( d \) adds \( d^3 \frac{q^d}{1 - q^d} \) to the Yukawa coupling. This spits out the various intersection numbers as coefficients in the expansion:

\[
K(q) = 5 + 2875 \frac{q}{1 - q} + 609250 \cdot 2^3 \frac{q^2}{1 - q^2} + 317206375 \cdot 3^3 \frac{q^3}{1 - q^3} + \cdots \tag{22}
\]

Associating coefficients with the values for \( n_d \), we find:

\[
n_1 = 2875 \\
n_2 = 609250 \\
n_3 = 317206375 \\
\vdots
\]

The \( d = 1 \) and \( d = 2 \) cases were already well-known to algebraic geometers. But \( d = 3 \) was the subject of investigation. The string theorist’s mirror symmetry based calculation turned out to be correct, giving strong evidence that the formula was giving the correct values for other values. The application of mirror symmetry here

\[\text{Full and very readable accounts of mirror symmetry, including the application discussed here, can be found in: [12] and [24]. An excellent, elementary overview (at least, as elementary as is possible), including aspects of enumerative geometry, is [25].}\]

\[\text{In more rigorous accounts, } n_d \text{ is taken to represent the Gromov-Witten invariants of the space. These, roughly, correspond to the structure that is left invariant under deformations of the complex structure (i.e. those infinitesimal deformations parametrized by the cohomology group } H^{2,1}).\]

\[\text{For a discussion of the methodological ramifications of this scenario (vis-à-vis the concept of evidence for string theory) in [41]. Peter Galison has a related, though more historical article, covering similar themes: [19].}\]
amounts to the ‘simulation’ of difficult quantum corrections (which generate the desired intersection numbers as instanton corrections) using the classical geometry in the mirror theory. In more string theoretic terms what is going on is that the Yukawa coupling (here, the 3-point vertex function or correlation function) is providing the count of the curves. This function contains both a classical (= easy) part and a quantum corrected (= hard) part. One can compute the quantum part using elements of the classical geometry and then convert back.

Given the remarkable nature of this application of the mirror map, one might not unreasonably view the positive results as offering evidence for the correctness of the duality and the theory to which it belongs, namely superstring theory. I shall attempt to defend such a view (making an inference from novel mathematical predictions to physical theory) in the following two sections. But let me first sum up this section by stating as explicitly as possible what the claim is that will be utilised in the subsequent sections.

Mirror symmetry was a mathematical discovery that arose from the study of (what purports to be) a physical theory, based on general physical principles combined with mathematical consistency. What the physical investigations suggested was that when one formulates a 2D conformal field theory (a string theory) on certain kinds of manifold (those with several dimensions compactified in ‘the right sort of way’) one finds that there is more than one such compact manifold for a single conformal field theory. In other words, the map from the structure of the compact dimensions to the low energy physics is many-to-one. Hence, string theory suggested an equivalence between mathematical objects that were previously thought to be quite distinct, that are in fact topologically inequivalent. String theorists managed this feat using physical principles and physical data. The suggestion was later confirmed and made into a rigorous theory by mathematicians. Note also that the predictions about mirror manifolds of string theory were tested in a weak sense by computer simulations.

6 Mathematical Miracles and Scientific Methodology

In his The Trouble with Physics, Lee Smolin writes that “[d]espite the absence of experimental support and precise formulation, the theory is believed by some of its adherents with a certainty that seems emotional rather than rational” ([48], p. xx). Smolin is not convinced that string theory’s ability to generate mathematical results is enough to justify its level of support within the physics community. For Smolin, what is needed are tests: concrete, physical tests—or, at least the ability to suggest

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19The formula of Candelas et al. was in fact made more rigorous using a variety of techniques external to string theory, see [26] for example. The various proofs of mirror symmetry can be found in [24]. The establishment of the mirror symmetric counting of curves depended precisely on the formalization of the instanton corrections using the tools of Gromov-Witten theory.

20Though it begins to stretch credulity somewhat, one might argue that since the predictions about mirror manifolds are intended to be claims about spacetime and its possibilities, they are testable in principle. The computer simulations would then constitute simulations of physical scenarios, much as one performs simulations of certain black hole scenarios (that would be hard to observe directly) as a way of testing general relativity.
potential tests. Is it true that string theorists’ often strong adherence to their theory is *irrational*? I argue not.

6.1 From Cosmic Coincidence to Realism

In the context of realism about scientific theories, J.J.C. Smart argued that instrumentalists (i.e. anti-realists) about scientific theories must believe in “cosmic coincidence”. He puts the point this way:

Is it not odd that the phenomena of the world should be such as to make a purely instrumental theory true? On the other hand, if we interpret a theory in the realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for it there really are electrons, etc. this is just what we should expect. A lot of surprising facts no longer seem surprising. ([47], p. 39)

Belief in the *truth* (or approximate truth) of an empirical successful scientific theory is, on this argument, the only stance that does not make that success puzzling. If the theory is true, or approximately true, then of course it will enjoy some empirical success. Hilary Putnam [39] labels this the “no-miracles argument” for scientific realism.

The argument begins with some puzzling fact (some phenomenon that cannot otherwise be accounted for). It is then noted that this fact can be derived as a theorem of some theory, and this is finally taken as *evidence* for the theory that generated that fact. If some theorem of the theory has not yet been checked against the world and is then found to make a successful prediction, then our evidence in the truth of the generating theory goes up.\(^{21}\) What I am suggesting in this paper, though disanalogous in a many important ways, is that there is something like this ‘inference to the best explanation’-style argument supporting belief in string theory.

But clearly (and this is the most glaring disanalogy) success in the string theoretic context cannot be *empirical* success in the sense of accurate physical predictions about real-world string-theoretic systems. Here I am understanding success to be *mathematical* success.\(^{22}\) Let us take as an example of what I have in mind the famous calculation of the magnetic moment of the electron using QED, and compare it with the Candelas *et al.* prediction of the number of degree \(d\) curves through a quintic 3-fold, as given above. The magnetic moment as computed using quantum field theory is \(1.00115965246 \pm 0.00000000020\). This same quantity was measured experimentally as \(1.00115965221 \pm 0.0000000004\)—as Feynman was fond of pointing out this level of accuracy “is equivalent to measuring the distance from

\[^{21}\]Bas van Fraassen [51] has provided a fairly convincing ‘Darwinian’ anti-realist counter-argument to this no-miracles argument: “any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature” (p. 39). Success here is just a selection-effect, analogous to the fitness of an organism: theories not well adapted to their environment (i.e. the actual regularities in nature) are quite naturally rejected. Truth *per se* (beyond the empirical) is playing no role in success.

\[^{22}\]But, as mentioned in footnote 20 above, these mathematical predictions *could* be construed as physical predictions, only of a very hard-to-test sort.
Los Angeles to New York, a distance of over 3000 miles, to within the width of a human hair” ([17], p. 118). He concludes by making a remark quite in keeping with the no-miracles line of reasoning: “These numbers are meant to intimidate you into believing that the theory is probably not too far off!” (ibid).

There is some similarity between the appearance of such predictions and the string theory-based predictions discussed above. For example, by using a generating function $K(q)$ drawn from string theory (from the sigma model) one can make predictions—and initially, these were predictions since they were not previously computed or established on a rigorous footing—about the number of curves of any degree through a quintic. For example, as Yuri Manin notes ([30], p. 161), one can ‘predict' the value of $d = 10$, “a theoretical(?) number still unchecked in an experiment(?)”, giving the intimidating number: 70428 81649 78454 68611 34882 49750! This remarkable interplay between physical models and pure mathematics leads Manin to speculate that

Today at least some of us are again nurturing an ancient Platonic feeling that mathematical ideas are somehow predestined to describe the physical world, however remote from reality their origins seem to be. ([29], p. 293)

My claim is that just such interplay grounds the adherence of string theorists to their programme. One can find quotations from prominent string theorists that seem to bear this out. For example, David Olive writes that “the physical ideas [in string theory] have gained support from the startling and successful repercussions they have had in pure mathematics in terms of conceptual breakthroughs” ([35], p. 3). This looks like a case of mathematical support of a physical theory. But, as Roger Penrose has stated it: “Are we entitled to infer from the undoubted insights into mathematics that [string theory] must also have a deep physical correctness?” There are ways of making a case for a positive answer to Penrose’s question, though it is clearly, on the surface, unlikely thesis. I begin with three potential objections.

6.2 The Mathematical Fertility of False Theories

Lee Smolin ([48], pp. 34–35) discusses the origins of knot theory in Thomson’s (i.e. Lord Kelvin’s) study of classical electromagnetism [50], developing from the idea that atoms were knots in magnetic field lines (making them vortex tubes of ether). It was actually P.G. Tait who abstracted from this the mathematical theory of knots, amounting to an in depth classification of possible knots.

Smolin argues that if mathematical fertility could be an indicator of truth, then we ought to take the success of knot theory as evidence for the idea that atoms are indeed knotted bits of ether. Hence, we have an apparent reductio ad absurdum of the

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23 Gabriele Veneziano is likewise adamant that the mathematical successes are reason enough to continue to pursue string theory, but does not view this as having physical relevance. He writes that “[t]he last 14 years of (super)string theory are an example of a perfect marriage between theoretical physics and mathematics … never before have we witnessed such a cross-fertilization between these two fields of human knowledge, … If string theory were to turn out to have nothing to do with Nature (which I refuse to accept) it would still go down in history as a gigantic achievement in mathematical physics” ([53], p. 187).
idea that I am arguing for in this paper, that mathematical fertility—such as the string
theoretic conjecture of mirror symmetry and the resolution of difficult problems in
enumerative geometry—might lead us to believe more strongly in a theory.

There are several points we can make in response. Firstly, I am not arguing that
mathematical fertility weighs as heavily as a good empirical confirmation (or empiri-
cal disconfirmation) in our evaluations of physical theories. But here, as I pointed out
in Sect. 2, we are in unfamiliar evidential territory: the scales at which experiments
would be able to test novel features of string theory are out of bounds for the fore-
seeable future. Good empirical confirmations are, then, likely to be very thin on the
ground for some time to come. Secondly, that Kelvin’s theory was eventually discon-
firmed does not mean that it was a bad theory—after all, it was discussed and studied
as a serious theory for some 20 years. It was precisely the fact that it was taken se-
riously as a physical theory that led to the development of knot theory. That it was
taken seriously for 20 years means that, structurally at least, the theory was ‘on to
something’: it got something right, just as Newton’s theory of gravitation, though in-
correct at certain scales and energies, still gets something right. Thirdly, and related to
the second point, the physics of knots did not vanish forever after this failed episode.
Rather, it forms an integral part of modern physics, especially in condensed matter
physics, quantum field theory, and quantum gravity (Smolin, more than most, knows
this well, of course). Moreover, the basic idea that one can account for particulate
objects using singularities of fields also remains in various guises, and knot theory
still provides the appropriate mathematical toolkit for representing such systems.

Hence, what Kelvin latched on to was some widely applicable piece of repre-
sentational machinery. I think we can reasonably say that the development of knot
theory ought to have given reason to believe in Kelvin’s theory in the absence of a
better confirmed theory or weightier disconfirming evidence.24 Having an hypothesis
that naturally generates a consistent and widely applicable mathematical framework
ought, I argue, to increase our credence in that hypothesis, if only in a relatively small
way. In the absence of alternative sources of evidence, then even so small an increase
in the credence given to a theory is not insignificant.

6.3 The Causal Isolation of Mathematics

The argument from mathematical predictions appears to fall foul of the ‘concrete ver-
sus abstract’ division separating physics from mathematics. Mathematics is causally
isolated: it’s objects are non-spatiotemporal. So how can it be that mathematical re-
results can have any impact on physical facts? This immediately presupposes that Pla-
tonism is involved. This is not a necessary consequence. We could, conceivably, adopt

24It is possible, too, that there is in any case a crucial disanalogy between the development of knot theory
and the mathematical results I have been discussing in the context of string theory. Knot theory had an
earlier birth with the work of Gauss and Listing—Gauss discovered the knot-invariant known as the linking
integral, involving the number of windings of pairs of knots in 3-space. This invariant was then carried
into physics by Maxwell, who interpreted it in terms of energy needed to move a charge through a wire
containing knots. Hence, though the subject certainly developed considerable impetus as a result of it, knot
theory had entered physics before Kelvin’s work. See [2] for a brief history of the relationship between
knot theory and electromagnetism.
a conventionalist or some other viewpoint. J.S. Mill, for example, espoused a curious empiricism about mathematical truths, viewing them as extremely general laws of nature. For example, the laws of addition will be satisfied by physical objects. Frege argued that this position conflated mathematics with its application. However, Lakatos later resuscitated Mill’s idea. He argued that mathematical theories, just like physical theories, were fallible.

Elliot Sober [49] has argued that there could never be the kind of relationship between physical and mathematical that I have been arguing for on the grounds that, while we would be willing to drop a claim about the physical world on the basis of empirical evidence, we would not do the same with mathematical claims. Mathematical truth is, as Mark Colyvan puts it, “never placed on the line” ([11], p. 114). Mathematics does not simultaneously get ‘tested’ by experiments that test some physical hypothesis. But if this is the case, then if mathematics gets to join in the success of physical theories, then it should also suffer the failures. Of course if a theory fails we don’t assume the mathematical theory used to describe it was wrong, but simply that it was wrongly applied.

There is a response we can make here. Provided that the physical theory is mathematically consistent, we can in fact run a similar line about the failure of physical theories, namely that they were merely wrongly applied. The idea here is that if only the universe were put together in the ‘right manner’, the physical theory would be perfectly applicable. In other words, there is nothing preventing us from understanding the falsity of a physical theory (say Newtonian mechanics) in terms of its inapplicability. Note also that there has recently been a spate of arguments in the philosophy of mathematics literature arguing that there can be purely mathematical explanations of physical phenomena—see e.g. [3]. If these arguments are correct then there can be crossings here too. However, this argument has come under fire precisely on the grounds that it doesn’t close the causal gap from mathematical to physical facts ([5], p. 19).

Is there a way even for those who espouse predictivism (that a theory gains more support from evidence that it was not designed to fit) to grant sound scientific status to string theory, and perhaps rank it more highly than other approaches to quantum gravity? I believe I have given one example already: the case of mathematical predictions that were not expected and that did not enter into the construction of the theory. It is true that this example, involving mirror symmetry, is not the kind of phenomenon that could be tested in the laboratory. However, as already mentioned above, such predictions can be tested and have been tested (and found to be correct) using computer simulations. In other words, string theory leads to mathematical predictions (about what are essentially represented spacetime structures, and the dynamics of strings in such spaces) that are testable using computers. In high energy contexts, or impractical situations, this is a perfectly legitimate methodology. A not inconsiderable part of what we know about QCD and, indeed, general relativity, is exactly of this kind.

A slightly different element of the causal isolation objection is that all that has been done in the mirror symmetry based solution of the enumerative problem is a rather standard application of pure mathematics, only clothed in physical ideas: one is doing nothing more exciting than deriving mathematics from mathematics. After all, if we ‘look under the hood’, as it were, what we find is nothing but the solution
of Picard-Fuchs equations relating different aspects of Calabi-Yau spaces and their mirrors, where the coefficients in the expansion of such solutions are related to the desired number of rational curves (for arbitrary degree). It is true that there are good mathematical reasons underlying the formula of Candelas et al., however, the real issue is how these underlying mathematical principles were targeted by philosophical principles. The mathematical framework underlying the initial conjectures that made for physical reasons came later, following much hard labour. That a theory has been able (on numerous occasions—the example given here is but one of many) to sniff out deep mathematics using physical principles says something about both the principles and the theory incorporating them. A theory could so easily lead to nonsense, and presumably the theories that make sensible, applicable mathematical predictions are dwarfed by those theories that do not.

It is a big leap from this targeting of interesting mathematical structures and results to ‘real physical truth’, of course. I don’t think this would constitute a defensible position framed so starkly. However, I have been arguing for a weaker position, involving the idea that if a physical theory enables one to make strides in mathematics, and come up with interesting, highly applicable results, then this ought to cause up to up our degree of belief in the theory. I am not so concerned with the truth of the theory per se. In any case, philosophers of science have a hard time unpacking the notion of ‘truth’. Truth could mean many things, from correspondence with some ‘facts in the world’ to coherence with a background web of beliefs about the world. So long as string theory faces the experimental distance problem introduced in Sect. 2, and so long as the theory is able to reproduce known facts (i.e. is empirically adequate), the we can presumably defend the idea that string theory is also true, and the targeting of mathematical structures that I have been referring to could also be seen as offering support to the truth of string theory too.

6.4 The Social Isolation of Mathematics

An alternative line of objection comes from Penelope Maddy, who argues that if there really were the kind of interaction between mathematics and physics that I have been proposing, then we ought to see mathematicians taking a vital interest in physics:

If this were correct, one would expect set theorists to be vitally interested in the implications of renormalization in quantum field theories, in developments in quantum gravity, in assessments of the literalness of other applications of continuum mathematics in natural science, for the propriety of their very methods would hang in the balance. ([28], p. 159)

But, she notes, set theorists couldn’t care a less: they are socially isolated from physics. Firstly, this might be true of some mathematicians, but it certainly isn’t true for all, or (I expect) even a large portion. I can think of mathematicians with interests in physics who are interested in the physical ramifications for category theory, for example. Geometry received an enormous impulse from the interactions with physics.

Secondly, she argues that the supposed indifference comes from the fact that there would be a practical indifference in the way set theorists would work: the methods
would be unchanged. But again, this doesn’t strike me as correct. Physical applications often have back-reaction on mathematics. Geometry again offers a counterexample. Yang-Mills theory has provided many new tools for mathematicians. Moreover, what the mirror symmetry example has shown is that if methods in the natural sciences are able to deliver results, then mathematicians will take note, and it could well infect their methods. Maddy’s argument rests, then, on too restricted a view of mathematics and mathematical physics.

6.5 Quinean Holism and the Indispensability Argument

A stronger link between the mathematical and the physical comes from the so-called indispensability argument originating with Willard Quine. Quine argued for holism about knowledge: belief in some hypothesis or theory is justified if the hypothesis or theory forms part of, or coheres with our overall knowledge. His own capsule formulation of this idea is expressed as follows: “our statements about the external world face the tribunal of sense experience not individually but only as a corporate body” ([40], p. 41). Furthermore, since our best physical theories are dependent on the truth of the mathematics than one uses to formulate them, empirical confirmation of the physical theory is just as much an instance of empirical confirmation of the mathematics. Mathematical objects are theoretical entities, just like electrons and quarks!25

This has direct consequences for the ‘abstract’ versus ‘concrete’ division: physical experiments that successfully confirm some theoretical prediction, where that theory is (indispensably) linked to some piece of representational machinery, likewise confirm the mathematics. If we think once again in terms of the no miracles argument, then if the successful prediction gives us some reason to believe in the existence of the entities that the theory uses to make the prediction, then (by parity of reasoning) it also gives us reason to believe in the existence of mathematical entities. This holistic view of confirmation quickly leads, then, to what is known as the “indispensability argument”: mathematics used in the construction of a theory receives empirical support, just as much as the theoretical entities used.26

But now what is stop us inverting this argument, and arguing instead that mathematical confirmations (derived from some ‘physical’ theory) can act as confirmations of the physical theory? In this inverted Quinean argument, then, we use successful mathematical predictions as support for a physical theory. There is no asymmetry in the direction of support; at least it is hard to see what could account for such an asymmetry if we are willing, as Quine does, to allow empirical confirmations of mathematical truths. If we can speak of physical evidence for mathematical objects and truths, then there ought to be room for mathematical evidence of physical facts. There is an obvious sense in which this is perfectly true: blatant mathematical inconsistency will enable us to infer that the physical world will not be able to instantiate it.

25I am grateful to David Armstrong for bringing the potential relevance of Quine’s position to my attention.
26Naturally the negative responses to this thesis have tended to argue that mathematics is not indispensable to science, and our usage of it in this context is nothing but a matter of convenience (see, e.g. [18]).
This possibility is made somewhat more palatable by the fact that the mathematical structures in question, in the case of string theory, are isolated by physical principles. String theory is, as Polchinski puts it, “a mathematical structure . . . deeply grounded in physics” ([36], p. 429). The fact that this structure is able to generate so many mathematical discoveries tells us something about the physical theory too. And as Putnam puts it: “if we were really just writing down strings of symbols at random, or even by trial and error, what are the chances that our theory would be consistent, let alone mathematically fertile?” ([39], p. 73). This is to recapitulate what I already argued above; namely that the ability of a theory to produce interesting, deep, applicable results outside of the domain for which it was devised can and should be seen as offering limited evidential support to that theory.

7 Accommodation versus Prediction

Let us now turn back to the issue of testability that has been laying fallow until now, for those who defend something like the view defended by Smolin may still have doubts about the rationality of adopting a physical theory that has made no directly testable predictions. The crux of the issue for string theorists is the debate between accommodationism (or, somewhat stronger, ‘explanationism’) and predictivism. The traditional view is that the ability of a theory to make predictions of novel phenomena (not used to guide the construction of the theory) weighs more heavily than its ability to explain old phenomena. However, the historian of physics Stephen Brush has marshalled several case studies that show this not always to be the case in practice. There are episodes in which the explanation of previously known but puzzling phenomena weighed more heavily than novelty of predictions.27

As Peter Achinstein [1] notes, this brings in some interesting historical elements into theory evaluation: notions of degree of support, and related notions, are not time-independent. There are several possibilities. For example, given evidence $E$ and theory $T$:

- $E$ offers evidential support for $T$ iff $E$ was not known before $T$ [33]
- $E$ offers evidential support for $T$ iff $T$ was not devised to explain $E$ [58]
- $E$ offers evidential support for $T$ iff $E$ was not explainable by other theories before $T$ [33].

I don’t wish to get involved in the difficult debate over whether evidence is or should be seen as historical or not. It is clearly true that those who take issue with string theory’s claim to ‘retrodict’ certain facts adopt a historicist position.

In a series of articles [6–8], Brush defends the view that novelty in predictions as a matter of historical fact do not play a greater role in theory evaluation than explanation and accommodation of old, yet puzzling data. His central case study throughout has been the acceptance of general relativity. The standard story tells how scientists and the public were instantly converted by the confirmation of general relativity’s

27 Above I argued that some of the ‘unforeseen predictions’ can be essentially mathematical, rather than physical. However, I will put this aside for the purposes of the discussion in this section.
light bending prediction. However, as Brush argues, general relativity was widely accepted before this test, and was done so on the basis of its ability to get the perihelion of Mercury correct. This was so, argues Brush, despite the fact that this data was guiding the very construction of Einstein’s theory, acting as a phenomenological target. General relativity’s retrodiction of Mercury’s (up until then, anomalous) perihelion, though not a novel prediction, was novel in the sense that it was the only theory able to do so. In this case, then, it seems that uniqueness of retrodiction is playing a crucial role.

Turing now to string theory. Many known aspects of particle physics are inexplicable using currently established theories (or unestablished theories for that matter). For example: why are there three families of particles? Why are the particle’s interactions governed by these particular symmetry groups? Why do we find the symmetries broken at these particular scales? In the context of string theory, these are delivered through the topology of $K_{6}$. As are (via the Yukawa couplings), the particle lifetimes and masses. Though certainly not perfect, string theory does deliver a landscape (i.e. an ensemble of theories) with regions that correspond to something like the standard model. The topological features of the worldsheets combined with those of the background enable the generation of low energy physics with the right gauge groups, and the right number of generations, and more). As Schellekens notes ([43], p. 11), though this is often seen as a trivial victory (given the vast size of the landscape) it isn’t all that trivial since infinitely many other gauge theories are simply ruled at as physically impossibilities. So string theory has a case that though it is only an instance of accommodation rather than prediction, it is at least a case of unique accommodation.

Peter Lipton ([27], p. 21) has objected to Brush’s account, noting how Halley’s theory of comets made three distinct accommodations of known cometary trajectories before the prediction of the returning comet that bears his name was confirmed. A single novel prediction massively outweighed the three accommodations. We can ask two questions about this: (1) is this generic? (2) is it rational? Lipton does find another case, involving the prediction of as of then unknown elements using the period table. He argues, again, that these were worth more than the accommodation of all the other elements. Stephen Brush ([7], p. 139) has taken Lipton to task for the lack of historical evidence for his claim. The burden of proof is on the predictivist to demonstrate that the confirmation of the prediction swayed scientists’ opinions. But, as with general relativity, the theory was already accepted by the time the novel prediction was confirmed. This is not to say that novel predictions could never and do never play a crucial role in the evaluation of theories. But it does show that the story is not simple. A cursory inspection of the history of quantum gravity research shows quite clearly that novel predictions are not always involved in the acceptance and rejection of theories.

I mentioned that the uniqueness of retrodiction or accommodated facts might also be playing a vital role in the credence assigned to string theory. Richard Dawid has argued for string theory on the basis that it is unique simpliciter [14]. It is, he argues, the only possible theory that does the job of unifying the forces. Let us suppose, for the sake of argument, that this was indeed true, that string theory is the only possible way to bring together the forces of nature. Would this, in itself, make the theory ‘true’? If we are certain that there are just these four forces then it looks like it might
have to be the case. But there is no definitive reason, still, that the forces must be unified. If there were, we could rule out any program in quantum gravity that seems only to quantize gravity (i.e. independently from the other interactions). Furthermore, I see no reason why even a theory of everything (for which, *per impossibile*, we are certain that it gives a complete description of reality) we must suppose that it must be unique. There is no reason why there could not be multiple distinct frameworks for describing the same picture, even when we are dealing with ‘theories of everything’.

Aside from this, the central problem with this suggestion (that uniqueness can be an indicator of truth) is that it amounts to a claim without support from the theory—cf. [23], Sect. 4, for a more general discussion of the problems with the uniqueness argument. String theory originated from Geoff Chew’s bootstrap approach, and it was thought to provide, in its early stages, a unique bootstrap. This was responsible for much of the excitement that gathered around the theory. However, the uniqueness quickly degenerated in several ways. Firstly in the several different types of string theory, and then in the number of possible ways of compactifying them. It is often said by string theorists that uniqueness is achieved by the duality symmetries that connect these theories, or that the various theories are ground states of one and the same theory, but this is wishful thinking: there is no internal reason to adopt this viewpoint. Indeed, duality symmetries are generally taken to relate distinct theories, making them distinct from gauge redundancies.

8 String Theory’s Consilience of Evidence

Taken individually, string theory’s instances of confirmation are admittedly relatively weak. We can enumerate at least five distinct categories:

1. Unification (‘accommodation’)
2. Universal structure
3. Simulations
4. QG Targets
5. Fertility.

Since string theory scores highly when we combine these diverse categories, then it scores highly overall *given the absence of a competing theory that has made a well-confirmed experimental prediction*. William Whewell gave such numerosity of evidence (in the sense of how much a hypothesis explains) a central role in his approach to theory evaluation, labelling the feature “consilience” ([54], p. 65). One can use the notion to compare competing theories, even in cases where there is no experimental evidence. One chooses the ‘more consilient’ theory—though of course, one would have to factor in some kind of quality control on the kinds of facts that are explained, to rule out trivialities and such like.

Since we have, in previous episodes in science, always had the availability of experimental tests we have never really had to weigh these alternative theoretical

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28One might follow Dirac [15] and add ‘beauty’ to this list of evaluative factors. See [31] for a thorough analysis (and defence) of the role of aesthetics in the evaluation of physical theories. For an analysis of the problems with taking beauty seriously in this way, see [16].
virtues. However, we can find some such instances. General relativity had no competitors when it came into being. Those other theories of gravitation that existed were known to be empirically inadequate. In this case, even before the classic tests of the theory it was considered to be well-confirmed on the basis of old evidence.

Interestingly, Charles Darwin argued for the theory of evolution by natural selection using such a consilience of evidence, and faced much the same objections as string theorists face today. Darwin staunchly defend the method, writing in the 6th edition of *The Origin of Species*:

> It can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection, the several large classes of facts above specified. It has recently been objected that this is an unsafe method of arguing; but it is a method used in judging the common events of life, and has often been used by the greatest natural philosophers. ([13], p. 476)

More interestingly, Karl Popper was one of those who would dispute the scientific status of Darwin’s theory. He viewed it as a metaphysical system (though not an unworthy one).

In a recent appraisal of string theory, Nancy Cartwright and Roman Frigg draw attention to the range of factors other than testability that can play role in our evaluation of theories. They also explain how string theory does well in some of these other “dimensions”. But they still come down negatively on the status of string theory, arguing that:

> A research programme that progresses only in some dimensions, while being by and large stagnant in the others, surely does not count as being progressive. Contrasting string theory with Maxwell’s unification of electricity and magnetism, for example, we can see that the latter was genuinely progressing and eventually successful in every dimension. It used the new and powerful concept of a field, which made the theory simple and elegant, while at the same time giving rise to a whole set of new phenomena that led to new predictions. ([10], p. 15)

The conclusion Cartwright and Frigg draw from their analysis strikes me as a *non sequiter*. After pointing out several ways in which string theory is progressive, they claim that, nonetheless, the theory is in fact degenerative or stagnant! Of course, as with other theories of quantum gravity, with string theory there is something genuinely novel about its experimental status, and to compare it to Maxwell’s theory, which made predictions well within energy capabilities of the day, is not helpful.

In the final analysis, Cartwright and Frigg defend, more or less, the traditional view of scientific method:

> The question of how progressive string theory is then becomes one of truth, and this brings us back to predictions. The more numerous, varied, precise and novel a theory’s successful predictions are, the more confidence we can have that the theory is true, or at least approximately true (see box). That a theory describes the world correctly wherever we have checked provides good reason to expect that it will describe the world correctly where we have not checked. String theory’s failure to make testable predictions therefore leaves us with little reason to believe that it gives us a true picture. (ibid.)
As I mentioned at the outset of this paper, string theory (and quantum gravity research in general) simply cannot be bound to these same constraints. Inasmuch as it can, it is along much more indirect channels, such as it’s performance in simulations, it’s ability to be applicable beyond its intended domain of application, and its history of generating mathematical results. But this is not sufficient for Cartwright and Frigg:

Although string theory has progressed along the dimensions of unifying and explanatory power, this in itself is not sufficient to believe that it gives us a true picture of the world. Hence, as it stands, string theory is not yet progressive because it has made progress only along a few of the many dimensions that matter to a research programme’s success. (ibid.)

The problem this passage exposes here is that Cartwright and Frigg slide from the evaluation of theories (not whether they are necessarily true), to talk of truth. I should point out that I am nowhere saying that the various virtues exhibited by string theory warrant belief in its absolute truth. What I am suggesting is that they warrant an increase in the credibility of the theory. They make it perfectly rational to pursue string theory, and yes, perhaps fund string theory more than its competitors, in spite of the lack of direct experimental support. I know that many string theorists do not think of their theory as ‘definitely true’ but simply as the best available approach. In this article I have attempted to show that this is a perfectly reasonable position to adopt.

Note that Cartwright and Frigg are writing from a Lakatosian perspective, according to which research programmes that are able to make novel predictions are considered progressive and those that don’t are considered degenerative. On this account it is not enough to fit a body of evidence, however varied and variegated that body might be. But this tags as degenerative virtually all quantum gravity research, including those programmes that have had ‘success’ in mathematics and other areas, such as computing. Hence, if the Lakatosian approach has this implication, then I would suggest that the approach itself is at fault: it is too restrictive.

9 Conclusion

String theory has not yet been able to make contact with experiments that would give us strong reasons to accept it as the ‘sure winner’ in the race to construct a theory of quantum gravity. However, though experiment can often function as a decisive arbiter in situations where there are several competing theories, there are many more theoretical virtues that play a role in our evaluation of theories. Taking these extra-experimental factors into account, string theory is very virtuous indeed. Not only is it able to unify a whole swathe of old data, and offer the prospect of a consistent theory of quantum gravity (in itself, no mean feat!), it is arguably the most mathematically fertile theory of the past century or so. Though a novel, quantitative physical prediction might perhaps be ‘worth more’ than the combined network of confirming evidence of string theory, until this comes about (within string theory or a competitor) string theory stands ahead of the competition. I would go further and say that no direct experiment is likely to ever come about (other than ones that could be explained
by multiple approaches), so we can assume that non-experimental factors will have to be relied upon more strongly in our assessments of future research in fundamental physics.

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