CHAPTER 13

#EpicFail? Criticisms of String Theory

Toddlers and Taleban alike know that it is always easier to knock down than to build up. Criticism is cheap and criticism – of anything – is easy. The critic gets to pick his point and time of attack. The critic does not have to be fair, and the critic does not have to provide either a solution to the issues he raises or any alternative proposal to deal with them.

For a topic whose subject matter is at almost maximal remove from daily concerns, string theory attracts a surprising amount of emotion. At the time of writing, the first three options on the google autocomplete of 'String theory is' are 'dead', 'wrong' and 'bullshit'. It is a topic on which people have real and polarised opinions. Having last encountered any science or mathematics amidst the fraught years of adolescence is no obstacle at all to holding strong opinions on string theory's relative merits compared to other ideas for quantum gravity.[1]

Of course, criticism has many positive features. The concept of the loyal opposition is a glorious feature of parliamentary democracy. The good faith exchange of contradictory views can sharpen vague ideas into blades that cut. False pretensions and claims are blown away by a need to provide clear answers to clear questions. Critical debate can bring the point of disagreement into focus, thereby showing where hidden assumptions are entering in. Every scientist grumbles about the referee reports they get back on the papers they submit for publication, but few would deny that on the whole these reports lead to improvements in the quality and readability of the papers.

There are many criticisms that have been levelled at string theory, and the aim of this chapter is to provide responses to these criticisms. In doing so

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[1] This passion for abstract topics cannot however compete with fourth-century Byzantium under the emperor Theodosius, when fishmonger and carpenter would passionately debate in the marketplace the relative merits of the homoi-ousian and the homo-ousian nature of Christ.

I have tried to be fair and to do my best by arguments even when I disagree with them. In my formulations, I have attempted to capture the spirit of these criticisms and to put them in their most convincing form. Sometimes they involve unspoken assumptions. Whenever possible I have sought to allow these assumptions and to meet the criticisms on their own ground.

However, I also want to make clear that what I address in this chapter are criticisms of string theory, and not the promotion of any individual alternative theory. This chapter deals with the arguments made that string theory is wrong or misguided, and not with arguments that some other theory is right. This is a case to be made by those who believe in it, and I have provided some references in the bibliography for those who want to pursue their arguments. For this reason this chapter will not deal with any criticisms of the form 'But string theory is so much worse at frying burgers than my theory'.

I will give partial consideration to such arguments in the next chapter, which is the counterpoint to this one. The next chapter deals not with why string theory is wrong, but with why string theory is right. It gives the positive case for why string theory has been so much more successful than any other proposed theory of quantum gravity. In doing so, it will in part address these unfavourable comparisons of string theory to other theories, as well as including some brief comments on these proposals. These comments will be brief – this book is not primarily about quantum gravity and it is certainly not about all theories of quantum gravity ever proposed.

So, what are the reasons put forward that string theory is *ex operibus diaboli*?

> CRITICISM: The attractiveness of string theory comes from its claim to solve the high-energy (sometimes called ultraviolet) problems of supergravity by making the divergences finite. However, there is no actual proof that string theory is finite. The calculations only hold at the lowest orders in perturbation theory and have not been extended further. Beyond these, finiteness is simply a conjecture, but not one that has actually been proven. As such, the finiteness of string theory might be an interesting idea, but one should not place too more store by it.

The strength and weakness of this point lies entirely in the word 'proof'. 'Proof' is a heavily loaded word. It is also a word that has more in common with mathematics than physics. The interesting structures of physics are too complicated to be described at the level of mathematical detail that can happily accommodate the mathematicians' notion of proof.

In 1984, one of the main selling points of string theory was that it offered a possible answer to apparently insuperable problems with the supergravity theories. This answer was partly conceptual and partly calculational. There were indeed calculations that gave finite answers where supergravity gave infinite answers. However, and at least as importantly, there were also conceptual arguments, through which the extended nature of the string provided a reason why these problems should be absent in string theory.

One aspect of this argument was that strings are extended objects and so tend to smear out infinities associated to point particles with no spatial extent. Another more technical point was that the structure of string theory offered a way to reinterpret any high energy problems of supergravity as low energy questions. On this way of thinking, the short-distance infinities of supergravity could be re-understood as long-distance effects. However, divergences associated to long-distance effects were already well understood through studies of quantum field theory and were known to be harmless. These arguments made string theory attractive in 1984: it proposed a new way to solve an old problem, and wherever the new ideas could be tested, they worked.

At that time, the question of whether all these ideas really did work as they appeared was a good and interesting question. Were the cancellations that had been found in the superstring merely a lucky coincidence? The arguments for finiteness worked neatly in the simplest settings – at the lowest orders in perturbation theory. However, even if they were saying something, there could still be something more that had been missed. Was superstring theory really consistent? This is ultimately what the criticism above asks – is string theory actually a theory that makes sense?

Superstring theory in 1984 was a relatively new and poorly understood structure focussed on the particular problem of quantum gravity. In this context, any proof of finiteness would have been very welcome. Such a proof would have, by necessity, automatically greatly extended the technical tools available in string theory. The techniques then used for describing fermions became prohibitively complicated beyond the lowest orders of perturbation theory, and of necessity any proof would have had to include methods for working at all orders in perturbation theory. Any general proof would have greatly extended the relatively few calculations that existed and would have offered clues to how the theory should be developed.

However, by the time we reach 2015 string theory has produced so much more of interest that this question of 'proof' is far less interesting. The number of positive, surprising and correct results produced by string theory is now so large that there can be no reasonable doubt that string theory as known today does represent a consistent mathematical something, even if it is not possible to define exactly what that something is. As one example, we saw in chapter 8 one of the highly intricate formulae that string theory reproduces in the AdS/CFT duality. It is beyond reasonable doubt that these agreements are not simply a fluke.

The technical argument for the finiteness of string theory is that the structure of the theory always allows potentially dangerous high-energy divergences to be reinterpreted as harmless low-energy divergences. In any place where it can be tested – including for far more complicated perturbative calculations than could be performed in 1985 – this principle has continued to hold over the last thirty years.

We still do not know fully how string theory works or what its most fundamental principles are. However, it by now requires something approaching dishonesty for a professional to doubt that it exists as a consistent theory of something. A Victorian engineer confronted with the latest mobile phone would be totally baffled as to how it works. He would have no possible conception at all as to the nature of the internal circuitry – the transistor would not be invented until long after his death – but he would also have no doubt that this circuitry worked.

It may still be said: that may be so, but why cannot someone still just take a few months to write down a proof? The answer is that physics is not easily amenable to proofs, and proofs cannot be found even for topics far simpler than string theory. As mentioned in chapter 8, there is a one million dollar prize available from the Clay Mathematics Foundation for proving one of the basic features of the Standard Model: the presence of a mass gap in the strong force. This is the statement that there are no massless particles charged under the strong force – there is a 'gap' to the first allowed mass. Compared to questions involving quantum gravity, this is a baby problem. The Standard Model is much simpler than quantum gravity. The techniques are far more understood. This question is also accessible to experimental study. There is also a *one million dollar incentive* – and yet there is still no proof.

> CRITICISM: String theory comes in so many forms that it is impossible to make any predictions. There are an almost infinite number of ways to compactify down from ten dimensions to four. Each way represents a different string theory, and each will lead to entirely different physics. String theorists themselves say that there are 10^{500} such possibilities, and so if you can get 10^{500} different theories you can get anything you want out. A theory that can predict anything is a theory that predicts nothing. A theory that makes no predictions and is not falsifiable is not science.

This criticism contains several errors and exaggerations, which I will address below. The criticism also contains an attitude to falsifiability characteristic of Popperians of the strict observance, which I note but shall not challenge.

The first main error is that it conflates the questions of 'What is science?' and 'What is the state of current technology?'. It is clear that the ability to test any idea experimentally is a function of the technology of the time. Nuclear physics was just as true in the stone age as it is today,[2] and it will remain just as true if we are returned thither through some war or catastrophe. Today, nuclear physics and the associated quantum mechanics is testable. In the past they were not, and in the future they may not be either. Their scientific truth, however, endures.

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[2] And earlier – around two billion years ago, a natural nuclear reactor operated at Oklo in Gabon in central Africa, fissioning much of the uranium present via a chain reaction.

That said, it is always better – not least for the scientists involved! – when ideas can be tested within a few years of their proposal, or at most within the lifetimes of the scientists. It would have taken the heart of a Vulcan not to rejoice in Peter Higgs's pleasure in living to see the discovery of the Higgs boson in 2012, at the age of eighty-three and almost fifty years after his paper on the topic. No one enters science for the money, but that does not confer immunity from the human desires for recognition and acclaim.

Science is also healthiest when the interchange between theory and experiment is rapid. Wrong ideas, like aggressive weeds, are best killed quickly, and experiment is the best killer of them. Science moves fastest when theoretical ideas are closely coupled to experiment. However, *sub specie aeternitatis* it is ultimately irrelevant whether bridging the technological gap required to test a theory takes ten years, a hundred years – or longer. Democritus was no less right that the world is made from atoms for having died over two thousand years before the construction of the periodic table.

It is clear that the natural scale of string theory is not the scale of atoms and is not the scale of the Large Hadron Collider. It is the scale of quantum gravity, and whatever that may be precisely, we certainly know it is far smaller than any distance scale we can currently access. Our inability to access this scale is technological, but not a question of principle. Given magnets large enough and long enough, we know how to accelerate protons to quantum gravity energies.

However, the Large Hadron Collider currently represents the best that we can do. If money were no object, we could do better; but as seen in chapter 11 even then there is no open path to studying physics directly at the Planck scale. All current technologies fail long before we reach these scales. While history teaches us to be exceedingly modest when attempting to constrain future ingenuity, it is clear that predictions for the Planck scale are for the moment a question of principle rather than practice.

Nonetheless, what are the predictions of string theory at these quantum gravity scales? In brief, they are extra dimensions, extended objects and soft scattering. As we have seen in chapter 10, from a four-dimensional perspective extra dimensions manifest themselves as additional particles: ten-dimensional gravity has many more internal degrees of freedom than four-dimensional gravity. This statement remains true whether the extra dimensions are classical geometric dimensions or quantum stringy dimensions with no easy classical interpretation.

Likewise, strings are characterised by an enormously rapid – an exponentially rapid – growth of the number of harmonics with energy, corresponding to the many possible directions in which a string can vibrate. As we have also seen, the scattering of strings (or any other extended object) at high energy furthermore has the distinctive feature of soft scattering – colliding objects have minimal tendency to go off at right angles from the collision axis.

These predictions are not hard to test. Once you have a microscope that is capable of resolving sufficiently small lengths, there is no mystery about how to test the relative claims that the electron is a particle or the electron is a string. You use the microscope, and you go and look. Indeed, no philosophical agonising about falsifiability occurred when string theory in its original incarnation was proposed as a theory of the strong force, and the characteristic length of strings was thought to be a femtometre. The reason string theory was originally ruled out as an account of the strong force was precisely because, as more experimental data arrived, its predictions totally and spectacularly failed to accord with this data.

If you can look at the quantum gravity scale, string theory is then not hard to test. At this point a rider is sometimes added to this objection: what about M-theory? The different string theories are all meant to be different limits of M-theory, but the equations of M-theory are unknown. If you cannot say fully what string theory really *is*, how can you say it is testable? How can you make any statements about predictions without a full definition of what is meant by the theory?

There are two answers to this. The first, conservative, one is to say simply that the above statements about testability apply only to all the work done on string theory in the last thirty years. In that string theory is a topic that has absorbed real people's time, it is testable in this sense, and these statements certainly apply to all the work that caused anyone to be interested in string theory in the first case.

The stronger, but still reasonable, response is that extra dimensions and extended objects are always present in string theory, and extra dimensions do carry physical meaning. As we go to higher energies, these extra dimensions become apparent and the number of particle-like states grow enormously. Likewise, the presence of spatially extended objects – whether strings or the branes of M-theory – is something that one can always look for once sufficiently high energies are attained.

Returning to the original criticism, the second inaccuracy is that it is not true that an almost infinite number of ways to go from ten dimensions to four dimensions implies an almost infinite number of possibilities for four dimensional physics. The number 10^{500} sounds large – and it is. However, as discussed in chapter 6 it is dwarfed by the number of genetic permutations that can arise when mummy and daddy get jiggy and make a unique human being. Despite this, observation of siblings and their parents belie the notion that infinite variation is therefore possible – and while there may be far more than 10^{500} possible human genomes, we can predict with good accuracy the number of fingers someone has.

Specifically, we have seen in chapter 10 that theories with extra dimensions leave characteristic legacies in lower dimensions. Almost always, there are additional light particles with very weak interactions: moduli, additional hidden forces or axion-like particles. These particles are simply a feature of extra dimensions and are present in any theory with extra dimensions. Their existence is therefore insensitive to the many different ways of moving from ten to four dimensions. It is not a theorem, but I am trying and failing to think of any counterexamples.

As we have also seen in chapter 10, there are many ways to look for such particles experimentally. These searches are not easy, and success is not guaranteed, but this situation is hardly unique to string theory.

It is certainly true that, as a fundamental theory of nature, string theory is hard to test. Of course, it would be undeniably nice to have an experiment with existing technology that was capable of giving a definitive answer about whether string theory – or anything else – was a correct description of physics at scales fifteen orders of magnitude smaller than those we are able to probe directly. To which the only response is: yes, it would be nice.

> CRITICISM: Modern physics, of which string theory is an example, ignores philosophy and does so at its peril. It is not reflective, but instead attempts to develop the subject following the 'shut up and calculate' tradition. In doing so it cuts off the hand that feeds it; it believes it can answer foundational questions while ignoring foundational thinking. The development of relativity required input of philosophical ideas such as Mach's principle; there is no reason to suppose the much harder problem of quantum gravity should be any different.

The essence of this criticism is that many of the deepest problems in physics are philosophical in nature. What is the nature of space? What is the nature of time? What are the basic principles that any quantum theory of gravity must satisfy? The argument made is that blind calculation is not enough – these questions cannot be answered without philosophical reflection, and that this process has been systematically rejected. The particle physicists of the 1960s and 1970s, flush with data, could get away with rejecting philosophy. However for problems without abundant data, this attitude is presumptuous at best and idiotic at worse.

Where this objection chiefly fails is in a conflation between the concept of 'philosophy' and 'what those calling themselves philosophers do in the philosophy department'. Nature does not divide itself by university department. Up until the nineteenth century, what we now call science used to be called natural philosophy. Isaac Newton's most famous work is called '*Mathematical Principles of Natural Philosophy*'. In the title, he makes the statement that natural philosophy is best done with the language of mathematics – while also gently alluding to Descartes' non-mathematical 1644 work *Principles of Philosophy.* 'Science' at that time was just natural philosophy – the philosophy of nature.

While the name has changed, the essence of the subject has not. For example, Richard Feynman was famously disparaging about philosophy – 'low level baloney' was one of his more polite comments. But, Feynman was also the person who reformulated quantum mechanics as a sum over all possible histories of a system. If you want to know what is the quantum mechanical probability for a particle to go from A to B, Feynman said, then you can do it by adding up contributions from all the possible paths there are from A to B.[3] Which way did the particle go? It went every which way. All paths contribute, and we do not and cannot say more. This is a deep truth about nature, and it a deep truth that deals with the same branch of knowledge that Aristotle's *Physics* did.

An example more relevant to string theory is the case of the holographic principle. This is the statement that the physics of a gravitational system in D dimensions can be captured by the physics of a non-gravitational system in $(D-1)$ dimensions. This is a statement that is made sharp in the AdS/CFT correspondence, which gives a precise mathematical formulation of it. This is one of the major components of string theory research in the last twenty years, and no criticism of string theory can simply excise this topic from consideration. But – how can the holographic principle *not* be regarded as philosophy? In any way that philosophy is worthy of the name, how can such a deep statement about nature not be called philosophical? It is every bit as deep as any of the ideas that fed into the development of relativity, and the sharpness of the calculational tests of it can only be a virtue and not a vice.

AdS/CFT is an example of a duality. There are other dualities that provide similar examples. In chapter 5 we encountered T-duality, which is essentially the statement that in string theory very small spaces are indistinguishable from very large spaces. Despite its surprising identification between two very different geometries, T-duality is still one of the best understood dualities in string theory. The mathematical subject of mirror symmetry that we encountered in chapter 9 can be seen as a generalisation of it. How can T-duality not be regarded as a philosophical statement about what space really is? Furthermore, it is a result backed by precise calculations. Just as in the time of Newton, a statement should not be seen as less philosophical merely because it is backed by mathematical evidence.

My general response to this criticism is then that on any historic reading of what counts as philosophy, or on any self-respecting notion of what philosophy encompasses, string theory does not ignore philosophy. It is instead part of (natural) philosophy.

The narrower statement that string theory is deficient as a theory of quantum gravity because it pays insufficient attention to what is going on in the philosophy department is simply weak (or, as Feynman might say, baloney). It is the same sort of baloney as the argument that your plumber might not be able to fix the drains because she is not an expert on the Victorian novel. It may be true that a plumber would be a better plumber for a wider knowledge of literature, but it is hardly the crucial aspect of the job. One part of my employment involves teaching physics at New College in Oxford, and one of the many pleasures of working in an Oxford college is a greater-than-average

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[3] More precisely, probability in quantum mechanics is the square of an amplitude. Feynman's prescription was to first sum the individual amplitudes for each path and then to square this sum.

exposure to professional philosophers – who are intelligent and sensible people who do not make this sort of silly argument.

> CRITICISM: String theory is too mathematical and has lost touch with actual physics. Physics advances through experiment, and it is extremely dangerous to believe it is possible just to think one's way to the answer without any input from observation. Practitioners of string theory have become obsessed with mathematical beauty and regard it as a reliable guide to truth. However their idea of 'beauty' may be false, and other people may find different ideas beautiful. Furthermore, it is not mathematical beauty that is relevant in evaluating a physical theory, but success in explaining experimental data. The 'beauty' beloved of string theorists leads them to ten spacetime dimensions; this is in manifest contradiction with observation.

This criticism certainly contains elements of truth. There are many who work on string theory who are entirely uninterested in either observational input or output. It is not what motivates them. They are interested in the formal structure of theories or in mathematical applications of them. The prospect of explaining experimental data is not what gets them out of bed in the morning.

There is nothing wrong with this. Mathematics is a worthy subject, and it is not less important because it does not involve experiment. Studying string theory for its mathematical applications is an entirely sensible reason to study it. There is a valid question, which I have sympathy with, as to whether *too many* people are currently working on the subject for reasons only tangentially related to physics. This is a legitimate question about distribution of funding, effort and resources, but it is a question of a different kind.

What is not defensible is the idea that string theory cannot be relevant for physics *because* many aspects of it involve advanced mathematics – where 'advanced' means significantly more mathematics than was needed for the formulation of either the Standard Model or general relativity. Mathematics is certainly not the only guide to truth, but it is historically true that advances in mathematics and advances in physics have fitted together hand in glove.

Furthermore, what precisely is meant by 'too mathematical'? Difficult mathematics has been encountered in physics before. This is what Max Born, one of the founders of quantum mechanics and winner of the 1954 Nobel Prize, had to say about the start of quantum mechanics:

> By observation of known examples solved by guess-work [Heisenberg] found this rule and applied it successfully to simple examples . . .
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> I could not take my mind off Heisenberg's multiplication rule, and after a week of intensive thought and trial I suddenly remembered an algebraic theory which I had learned from my teacher,

> Professor Rosanes, in Breslau. Such square arrays are well known to mathematicians and, in conjunction with a specific rule for multiplication, are called matrices.

Matrices are now the type of diddy topic that are taught in school and professional physicists end up unable to remember not knowing. Looking further back, Cartesian coordinates – labelling graphs with an x and a y axis – were also at their time a shocking innovation. However useful, obvious and natural they may seem to us, the hard truth is that their discovery eluded Greek, Arabic, Chinese and mediaeval science and mathematics.

The problem with 'too much mathematics' as an objection is that it appears to be shorthand for 'there is too much unfamiliar mathematics compared to the mathematics I learnt as a student'. It is clear from history that advances in physics have very often required mathematics that was unfamiliar and that initially appeared bizarre. Mathematics that is necessary becomes familiar, and mathematics that becomes familiar becomes easier.

The complaint that whatever progress has been made in string theory has not been through explaining experimental data is a true one. It is also a slightly unfair one. The book started with an account of the unreasonable success of the Standard Model, a theory that has been far more successful in explaining experimental data than it ever deserved to be. All data in particle physics is consistent with the Standard Model. So far, all searches for qualitatively new physics have been without success – and if anything is to 'blame' for this fact, it is the laws of nature.

Null results do give (some) information, but they are nowhere near as informative as discoveries. It is almost a tautology that if there is any progress that can be currently made about physics at quantum gravity scales, this progress will require more than just experimental data – and mathematics will play some role in it.

> CRITICISM: One of the major features of Einstein's theory of general relativity is that it is background independent. Its formulation does not depend on a choice of coordinates. All that really exists are relations between objects, and any fundamental formulation of physics must be done in a way that does not depend on any particular choice of coordinates. In particular, any correct theory of quantum gravity must be background independent.
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> However, string theory is not background independent. The standard formulation of string theory is in terms of an expansion in terms of small perturbations about a particular spatial background. String theory therefore always depends on a choice of a background. Its physics is not background independent, and consequently string theory is not a theory of quantum gravity.

To my mind, the problem with this view is that it is based on a rather fixed ideological belief concerning what quantum gravity *must* be. The criticism is founded on the notion that one can first guess or deduce the principles underlying fundamental physics, and then construct the theory according to the principles. It expresses an overconfidence in the ability to know how everything will turn out, independent of input from either experiment or calculation. I am reminded of the (possibly apocryphal) response of Niels Bohr to Albert Einstein when he expressed his doubts about quantum mechanics:

> Einstein: God does not play dice.
> Bohr: Don't tell God what to do!

Let me make two more detailed responses. First, geometry is not fixed but manifestly dynamical in string theory. The fields that describe spacetime are not static. They have equations of motion, and these equations of motion cause them to change. In that string theory is an expansion about a fixed background, it is also a background that changes dynamically according to Einstein's equations. Small changes build up to large changes, and large changes can be as large as one wishes.

There is also an important distinction between the statement that the physics must be background independent, and the statement that the formulation of the physics must be background dependent. This may seem unclear. While quantum gravity may be esoteric, there is a more familiar topic in which one can re-express this same issue: cartography and the making of maps.

How do we describe the geometry of the earth? There are two ways, one 'background independent' and one 'background dependent'. The background dependent way is through an atlas of charts. If you purchase an atlas, on every page you will find a map of a different part of the earth. Depending on the purpose of the atlas, these maps can be of varied quality with varying levels of detail. They contain cities, towns and villages. They contains the contours of the land and the depths of the sea. They contain ship wrecks and sandbanks, castles and churches. Each chart only describes a small part of the overall picture: a ship sailing to Archangel will have little use for a map of Cape Horn. The charts also depend entirely on coordinates, as they have latitude and longitude lines stretched across them. Patched together however, the charts describe the surface of the entire earth: they are good for any purpose.

There is also a 'background independent' way of describing the earth. This is through a globe. A globe provides a visualisation of the full geometry of the earth. With a globe there is no requirement of labels for latitude and longitude, or indeed any other choice of coordinates. Globes preserve perspective and area in a way that is not possible to do with an atlas, and they are excellent educational tools.[4] However – it is not reasonable to argue that globes are 'right' and atlases are 'wrong'. An atlas – which uses particular choices and

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[4] Maps in an atlas can either preserve area and violate angles, or preserve angles and violate areas. The most familiar atlas projection of the globe is the Mercator projection. This preserves angles but does not preserve area – making the relative size of Europe compared to Africa appear far larger than it actually is.

charts to label every part of the globe – is a precise and correct description of the globe. In fact, the mathematical definition of any geometric space, technically called a *manifold*, is done precisely in terms of an atlas of charts. So there is nothing 'wrong' with the choice of coordinates – it is the choice to use an atlas of charts rather than a globe.

The second objection is that 'background independent' is a slogan, and a hollow slogan without deep content unless further accompanied by a notion of what *a* background is. To claim to be independent of all backgrounds, it is first necessary to say what these individual backgrounds actually are.

The simplest possible background is flat spacetime: a background that was already present in special relativity. Slightly more complicated backgrounds are the curved but classical geometries that arise in general relativity. These solutions are still, however, well approximated by (generalisations of) Einstein's theory of gravity.

What about more complex backgrounds than these classical, weakly curved spaces? In string theory, it has required many centuries of work to determine the large variety of possible different backgrounds that are permitted in the subject, and 'background' in string theory is a much richer concept than in general relativity. As seen in chapter 11, it must enlarge to include geometries that are of different topology. 'Topology' refers to the properties of objects that remain unaltered under any smooth change. The shape of a bagel cannot be deformed into the shape of a tennis ball no matter how much you knead it – you have to tear it. Any such geometric transition is entirely impossible in Einstein's theory of gravity, as you cannot tear space. As also seen in chapter 11, string theory contains controlled examples in which the space changes topology. You can smoothly change the topology of spacetime in string theory without anything funny happening.

Secondly, the concept of a background must also include examples where the background smoothly deforms, in a calculable fashion, from the classical picture of Einstein into a form of 'quantum geometry'. In quantum geometry the background no longer admits an interpretation in terms of classical notions of space. Coordinates no longer have any meaning – these represent an idea that sensibly applies only for the classical geometries your grandparents grew up with.

As a final illustration, the range of backgrounds must also include geometric spaces of different dimensionality. As we saw in chapter 5, one of the most surprising results from the mid-1990s was that string theory taken as a whole has limits in which it is either a ten-dimensional theory *or* an eleven-dimensional theory – and it is possible to interpolate between the two.

While 'background' in string theory is mostly a richer concept than in general relativity, it is also in some ways poorer. There are backgrounds that look very different in a classical theory of gravity, but that are identical in string theory. In string theory, T-duality implies very big spaces and very small spaces are the same. As backgrounds, they are absolutely identical. They are one and the same. This is not at all obvious at the outset, and it can only be seen by looking at the actual equations of string theory on an actual background.

All these results were found through hard calculation, by looking at particular backgrounds in detail and understanding what happens as small changes are made near those backgrounds. None of these results would have been easy to guess in advance.

The danger with the assertion that background independence is a guiding principle of quantum gravity is that it tends towards an impoverished view of what is possible. Real content comes from knowing what all the possible 'backgrounds' can be. Once you know what all the possible backgrounds can be, you are a long way towards knowing what quantum gravity is.

What the criticism does correctly capture is the fact that in an ideal world, you would have a formulation of string theory that gave you a view from which all these surprising results become 'obvious'. From the right perspective, crazy relationships just become simple consequences of general principles. When this is attained, it produces one of these glorious moments of scientific ecstasy when understanding brushes aside confusion.

This perspective does not yet fully exist for string theory. However the fact that such a formulation does not yet fully exist does not make string theory wrong – it just makes it a topic of research. It is like saying that because no-one can yet prove the elementary (and apparently true) statement that all even numbers can be expressed as the sum of two primes, we have no theory of prime numbers.

In the end, 'background independence' is a rallying call. If someone believes that the only way to make progress is by following this principle and writing down, in one go, the full theory of quantum gravity, then that is what they believe, and no amount of result or calculation will convince them otherwise. In this respect, extensive argument with proponents of this view becomes like a discussion with either committed Marxists or members of the Chicago school of economics, where independent of the question the answer is either 'dialectical materialism' or 'monetarism and free trade'.

> CRITICISM: String theory receives too high a fraction of the available resources for fundamental physics. String theory has promise, and it is reasonable that some people are interested in the subject and work on it. However, there are many approaches to quantum gravity and this should not be to the exclusion of other methods. In the same way that retirement savings should not be entirely invested in a single stock, resources in physics should be far more equitably distributed so that similar levels of attention can be paid to each of the different approaches to quantum gravity.

While it superficially sounds entirely reasonable, this criticism contains two implicit assumptions. First, it assumes that there is actually a lot of money spent on string theory. Second, it assumes that this money comes from a large pot of soft goodies, which is jealously guarded to prevent it being shared out. In this world, there should be more than enough money to go round, with some to spare. The only reason it is not is because of bad behaviour by string theorists, who hoard these resources, keeping them for themselves and their friends.

The fault with this criticism is that it supposes an idyllic world entirely disconnected from the practicalities of funding. In the real world, scientists almost entirely get money to do research by asking funding bodies for grants. If I want to get money to do research on string theory (as I do, and as I have done), I do not do so by asking a committee full of my chums. Instead, I have to make my case to a panel from many different specialisms. The vast majority of this panel will have never worked on anything even tangentially related to either string theory or quantum gravity, and indeed may not even be working on particle physics. I have to convince this panel both that I, as an individual, am worth funding and also that the topic I propose to work on deserves public money. For the largest grant, by cash terms, that I have been awarded, the relevant committee involved sixteen people, of whom a total of one – precisely one – was in even the broadest and most generous interpretation of 'my area'.

Success in such grant applications depends on both tangible and intangible factors. The tangible factors involve both past history and publication record: the papers you have written and the number of times they have been cited. Past performance is no guarantee of future success, but it certainly helps in a grant application. There are also the intangibles – the fluency of a presentation and the ability to make a research proposal convincing and comprehensible to those outside the subject, all mixed with the individual perversities and predilections of the interview panel and its members.

The people making the decisions to spend money on string theory, then, are not string theorists. Grants are hard to get and grant applications are competitive. The success rate for the major grants that launch independent careers can be smaller than ten per cent, and the large majority of applications fail. Scientific funding is not a Care Bears' tea party. Panels have to decide where limited resources can be most productively spent, and every penny obtained is obtained by convincing those outside your field that what you do is worthwhile and deserves to be funded. There is no soft pot of money available for those who would like a good salary to develop their own theory of quantum gravity at the taxpayers' expense.

Why has string theory been successful in this endeavour? The long answer has been given throughout this book. The short answer is that, as seen in chapters 8 to 11, string theory has proven to be so much more than just quantum gravity – and by doing so it has become attractive to large numbers of scientists.

In the next and final chapter I summarise this positive case for string theory, and also explain why string theory has in fact been preferred over other alternative theories of quantum gravity.