Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 1 of 55
Go Back
Full Screen
Close
Quit

String Theory and the Landscape

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Abstract

A brief survey of the status of string theory: why it is a good candidate for a complete theory of fundamental physics; recent developments which strongly suggest that it has a large "landscape" of solutions; and the implications for observable physics.

1. Introduction

Fundamental physics is the search for laws which govern all physical observations (within some class) to date. Thus it has evolved with time:

- 1800 – celestial mechanics, Newton's equations
- 1865 – electromagnetism, Maxwell's equations
- 1900 – statistical mechanics, the Boltzmann distribution
- 1915 – general relativity.
- 1930 – quantum mechanics, the Schrödinger equation
- 1970 – quantum field theory, the Standard Model of quarks, leptons and their interactions.

With one notable exception (GR), this progress came with the study of shorter distances, from the Solar System, through the phenomena which govern the everyday world, to the atomic and then subatomic.

Many expected this to be a progression from complexity to simplicity, that the complexity of the everyday world would follow from simple underlying laws.

But, as we will review shortly, there is abundant evidence that this is not so. Now the Maxwell equations,

$$\partial^\mu F_{\mu\nu} = 0$$

the Schrödinger equation of atomic physics,

$$i\hbar \frac{\partial}{\partial t}\Psi = H\Psi = \left(-\sum_i \hbar^2 \frac{\partial^2}{\partial \vec{x}_i^2} + \sum_{i<j} \frac{q_i q_j}{|x_i - x_j|} \right) \Psi$$

and the equations of general relativity,

$$R_{\mu\nu} - \frac{1}{2} R g_{\mu\nu} = 8\pi G_N T_{\mu\nu}$$

are beautifully simple.

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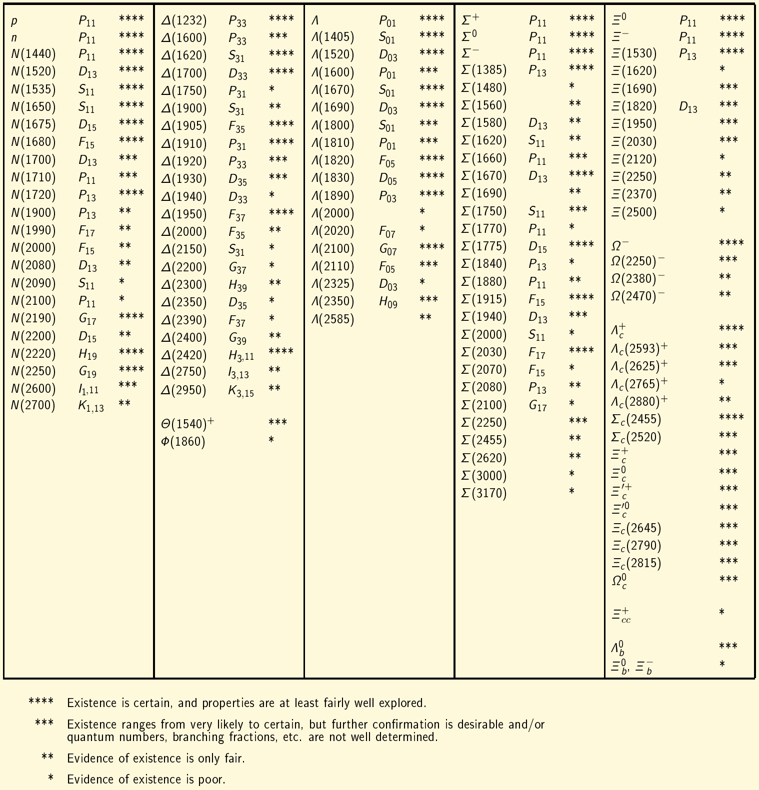
are beautifully simple.

But this simplicity started to evaporate in particle physics experiments of the 1930's. The muon is an analog of the electron with the same charge, but 207 times its mass. Its discovery was a complete surprise; when the physicist I. I. Rabi heard, he famously commented, "Who ordered that?" Einstein was only the most prominent of the physicists who turned away from such complicated and arbitrary seeming discoveries. Nature was supposed to be simple.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 3 of 55
Go Back
Full Screen
Close
Quit

Things were to get far worse before they would get better:



p	P_{11}	****
n	P_{11}	****
$N(1440)$	P_{11}	****
$N(1520)$	D_{13}	****
$N(1535)$	S_{11}	****
$N(1650)$	S_{11}	****
$N(1675)$	D_{15}	****
$N(1680)$	F_{15}	****
$N(1700)$	D_{13}	***
$N(1710)$	P_{11}	***
$N(1720)$	P_{13}	****
$N(1900)$	P_{13}	**
$N(1990)$	F_{17}	**
$N(2000)$	F_{15}	**
$N(2080)$	D_{13}	**
$N(2090)$	S_{11}	*
$N(2100)$	P_{11}	*
$N(2190)$	G_{17}	****
$N(2200)$	D_{15}	**
$N(2220)$	H_{19}	****
$N(2250)$	G_{19}	****
$N(2600)$	$I_{1,11}$	***
$N(2700)$	$K_{1,13}$	**

$\Delta(1232)$	P_{33}	****
$\Delta(1600)$	P_{33}	***
$\Delta(1620)$	S_{31}	****
$\Delta(1700)$	D_{33}	****
$\Delta(1750)$	P_{31}	*
$\Delta(1900)$	S_{31}	**
$\Delta(1905)$	F_{35}	****
$\Delta(1910)$	P_{31}	****
$\Delta(1920)$	P_{33}	***
$\Delta(1930)$	D_{35}	***
$\Delta(1940)$	D_{33}	*
$\Delta(1950)$	F_{37}	****
$\Delta(2000)$	F_{35}	**
$\Delta(2150)$	S_{31}	*
$\Delta(2200)$	G_{37}	*
$\Delta(2300)$	H_{39}	**
$\Delta(2350)$	D_{35}	*
$\Delta(2390)$	F_{37}	*
$\Delta(2400)$	G_{39}	**
$\Delta(2420)$	$H_{3,11}$	****
$\Delta(2750)$	$I_{3,13}$	**
$\Delta(2950)$	$K_{3,15}$	**
$\Theta(1540)^+$		***
$\Phi(1860)$		*

Λ	P_{01}	****
$\Lambda(1405)$	S_{01}	****
$\Lambda(1520)$	D_{03}	****
$\Lambda(1600)$	P_{01}	***
$\Lambda(1670)$	S_{01}	****
$\Lambda(1690)$	D_{03}	****
$\Lambda(1800)$	S_{01}	***
$\Lambda(1810)$	P_{01}	***
$\Lambda(1820)$	F_{05}	****
$\Lambda(1830)$	D_{05}	****
$\Lambda(1890)$	P_{03}	****
$\Lambda(2000)$		*
$\Lambda(2020)$	F_{07}	*
$\Lambda(2100)$	G_{07}	****
$\Lambda(2110)$	F_{05}	***
$\Lambda(2325)$	D_{03}	*
$\Lambda(2350)$	H_{09}	***
$\Lambda(2585)$		**

Σ^+	P_{11}	****
Σ^0	P_{11}	****
Σ^-	P_{11}	****
$\Sigma(1385)$	P_{13}	****
$\Sigma(1480)$		*
$\Sigma(1560)$		**
$\Sigma(1580)$	D_{13}	**
$\Sigma(1620)$	S_{11}	**
$\Sigma(1660)$	P_{11}	***
$\Sigma(1670)$	D_{13}	****
$\Sigma(1690)$		**
$\Sigma(1750)$	S_{11}	***
$\Sigma(1770)$	P_{11}	*
$\Sigma(1775)$	D_{15}	****
$\Sigma(1840)$	P_{13}	*
$\Sigma(1880)$	P_{11}	**
$\Sigma(1915)$	F_{15}	****
$\Sigma(1940)$	D_{13}	***
$\Sigma(2000)$	S_{11}	*
$\Sigma(2030)$	F_{17}	****
$\Sigma(2070)$	F_{15}	*
$\Sigma(2080)$	P_{13}	**
$\Sigma(2100)$	G_{17}	*
$\Sigma(2250)$		***
$\Sigma(2455)$		**
$\Sigma(2620)$		**
$\Sigma(3000)$		*
$\Sigma(3170)$		*

Ξ^0	P_{11}	****
Ξ^-	P_{11}	****
$\Xi(1530)$	P_{13}	****
$\Xi(1620)$		*
$\Xi(1690)$		***
$\Xi(1820)$	D_{13}	***
$\Xi(1950)$		***
$\Xi(2030)$		***
$\Xi(2120)$		*
$\Xi(2250)$		**
$\Xi(2370)$		**
$\Xi(2500)$		*
Ω^-		****
$\Omega(2250)^-$		***
$\Omega(2380)^-$		**
$\Omega(2470)^-$		**
Λ_c^+		****
$\Lambda_c(2593)^+$		***
$\Lambda_c(2625)^+$		***
$\Lambda_c(2765)^+$		*
$\Lambda_c(2880)^+$		**
$\Sigma_c(2455)$		****
$\Sigma_c(2520)$		***
Ξ_c^+		***
Ξ_c^0		***
$\Xi_c^{\prime +}$		***
$\Xi_c^{\prime 0}$		***
$\Xi_c(2645)$		***
$\Xi_c(2790)$		***
$\Xi_c(2815)$		***
Ω_c^0		***
Ξ_{cc}^+		*
Λ_b^0		***
Ξ_b^0, Ξ_b^-		*

****	Existence is certain, and properties are at least fairly well explored.
***	Existence ranges from very likely to certain, but further confirmation is desirable and/or quantum numbers, branching fractions, etc. are not well determined.
**	Evidence of existence is only fair.
*	Evidence of existence is poor.

Baryon summary table, from the Particle Data Book.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions
Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

But they did get better. The Standard Model, formulated from 1967–73, provided a beautiful and simple explanation of a wealth of data,

But for a fundamental theory, it is surprisingly complicated.

Its equations of motion, comparable to the Maxwell or Schrödinger equations, take about a page to write out.

It involves 19 seemingly arbitrary parameters, such as

$$\alpha_1^{-1} = 137.03599911(46)$$

(the inverse fine structure constant), and about 100 species of particle (counting color and flavor) – most completely irrelevant to physics on earth, and even in most astrophysics.

Of course, this theory is still very simple compared with the emergent complexity of chemistry or biology. Nevertheless, it is evidently not the simplest theory in its class; we will quantify its complexity later in this talk. Where does this complexity come from?

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There are two ways we might try to answer this question:

1. The complexity of the SM emerges from some simpler underlying framework.

2. The complexity of the SM is required in order to produce a universe sufficiently complex to support structure and life.

Or, we might need to call on both ideas.

2. String/M theory

The best clue we have towards an underlying framework is the difficulty of reconciling quantum field theory and general relativity. While this was long mysterious, it is now believed that this is not a deep issue of principle (analogous to passing from classical to quantum mechanics), but rather a problem which can be addressed using the known concepts of quantum theory. One piece of evidence for this is the existence of field theories of gravity in 2 and 3 dimensions.

In 4 and higher dimensions, treating the Einstein action in perturbation theory leads to nonrenormalizable divergences. So far the only accepted cure for this is to go to string theory. As two strings approach to within the string length, all forces, including gravity, become weak, avoiding the problem of nonrenormalizability. String theory has been shown to lead to finite graviton scattering amplitudes to all orders in perturbation theory, and sensible nonperturbative completions of these amplitudes seem to exist (Veneziano; Gross-Mende; Banks-Fischler).

The problem is solved, but at a price: strings only contain gravity if space-time has ten dimensions.

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 6 of 55
Go Back
Full Screen
Close
Quit

While when first discovered this fact seemed to make the theory utterly useless, by now we are familiar with the idea (first proposed by Kaluza and Klein in 1925!) that extra dimensions of space (not of time) might not be inconsistent with observation, if they are very small, of diameter $R << 10^{-17}$ cm.

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If we are brave and continue, we soon realize that this provides a possible explanation for the structure we see in the Standard Model. We begin by postulating that space-time is the direct product of four-dimensional Minkowski space-time with a six dimensional manifold K. To make this work, we need to find K which solves Einstein's equations. We then study the small fluctuations of the Einstein and other wave operators on this space-time, and find that each eigenvalue of a Laplacian on K, is the energy (squared) required to create a particular particle.

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 7 of 55
Go Back
Full Screen
Close
Quit

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Now all particles observed to date are light and must correspond to small eigenvalues, much less than $1/R^2$. In general, the scalar Laplacian has only one of these. But Laplacians acting on tensors, spinors and so forth can have more. These generally appear for **topological** reasons. For example, the Laplacian on differential p-forms,

$$\Delta\omega^{(p)} = -(d * d * + * d * d)\omega^{(p)},$$

will have $b^p(K)$ zero modes, where $b^p(K) = \dim H^p(M, \mathbb{R})$ is the p'th Betti number of K.

Thus, the multiplicity of particles in the SM, arises because of the non-trivial topology of K. In the original analysis of the heterotic string, it was found that the number of generations of quarks and leptons was determined by the Euler number of K,

$$N_{gen} = \frac{1}{2}\chi(K).$$

Thus the observed three generations would follow if $\chi(K) = 6$.

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Thus the observed three generations would follow if $\chi(K) = 6$.

But are there any six-dimensional manifolds K which solve Einstein's equations? The simplest candidate, the torus, has $\chi = 0$ and is too simple. But it turns out that a wide variety of solutions can be found by methods of algebraic geometry. As conjectured by E. Calabi and proven by S.-T. Yau, for any complex Kähler manifold with zero first Chern class, there exists a Ricci-flat metric. The simplest example is the quintic hypersurface in $\mathbb{CP}^4$,

$$0 = z_1^5 + z_2^5 + z_3^5 + z_4^5 + z_5^5.$$

While this one has $\chi = 200$, a few exist with $\chi = 6$.

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

When this was realized in 1985, physicists became very excited. Indeed, an (in)famous quote in the Argonne conference proceedings from that year, reads,

> Thus, it appears that there is no obstacle in principle to deriving all of known physics from the $E_8 \times E_8$ heterotic string.

3. The Λ problem

So why haven't we done it? Of course various problems soon arose. But the deepest of these goes back to Einstein's work on cosmology. and his discovery that his equations predicted that a static universe is unstable – it will tend to either expand or contract. To prevent this, he added another term, the cosmological constant (or "c.c.") Λ:

$$R_{ij} - \frac{1}{2} R g_{ij} = 8\pi G_N \; T_{ij} + \Lambda g_{ij}.$$

When Hubble discovered the red shift of distant galaxies, and thus the expansion of the universe, Einstein realized that he had missed making one of the greatest predictions of all time – his "greatest blunder."

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But to present-day thinking, Einstein did not make a blunder. In quantum theory, the vacuum has energy, coming from the zero-point fluctuations of all of the fields. There is no *a priori* reason this energy density, which is Λ, should be zero. Indeed, we can observe vacuum energy in the lab, in the form of the Casimir force between conducting plates. So we need to parameterize it, and ultimately compute it from the underlying fundamental theory.

Can we estimate this energy density? There are various estimates:

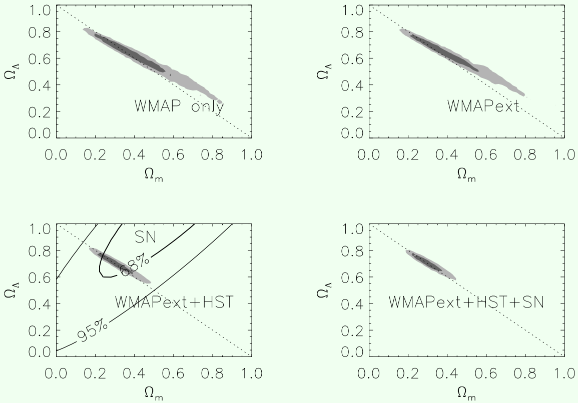
- In a generic theory of quantum gravity, we expect about one Planck energy per Planck length 10^{-33} cm cubed.
- String theory might substitute the "string length" for the Planck length. However, since we have not observed strings, this length must be shorter than 10^{-18} cm
- Exact supersymmetry would guarantee that the vacuum energy from bosons is cancelled by the fermions, so would be exactly zero. However we do not have exact supersymmetry; it must be broken at distances less than 10^{-17} cm, and these effects also create vacuum energy.

All of these estimates are far above even the weak limit $|\Lambda| < (10^{-2}\ \mathrm{cm})^{-4}$ which follows from the fact that the universe is more than ten billion years old and has not blown apart or recollapsed. And, nobody has ever thought of a way around them, which would make the expected value zero.

Still, most physicists thought the correct theory should provide such a way. Since string theory had not, either it was wrong, or at best we did not understand it well enough to have much confidence in its other predictions.

This changed in the late 1990's thanks to astronomical observation, more specifically the study of distant supernovae, which established that the expansion of the universe is slowly accelerating, in a way which is most simply explained by assuming that $\Lambda > 0$.

This was dramatically confirmed in 2003 by the WMAP satellite, whose precision measurements of the microwave background allowed fitting cosmological parameters with unprecedented accuracy. We now believe that $\Lambda = (0.71 \pm 0.02)\Omega$, where Ω is the "critical density" beyond which the universe would eventually recollapse.



From D.N.Spergel, L.Verde et al, Astrophys.J. 148 (2003).

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions
Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

Evidently, there is no hidden mechanism or symmetry in the correct theory which sets $\Lambda = 0$. Rather, we face the problem of explaining a non-zero but incredibly small value,

$$\Lambda \sim 10^{-122} M_{Planck}^4 \sim 10^{-60} M_{susy}^4.$$

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What determines the value of Λ? In string theory, this is tied up with the problem of determining all of the coupling constants: the fine structure constant, the masses of the particles, and so on, as none of these constants appear in the underlying ten-dimensional theory.

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The key idea is that all of the other constants are determined by the expectation values of fields, analogous to the electric and magnetic field (though these fields are scalars in space-time, so more analogous to the electric potential, temperature, etc.) While the Standard Model contains only one scalar field, the Higgs field corresponding to the (still unobserved) Higgs particle, string compactifications typically contain hundreds of scalar fields, for example those parameterizing the size and shape of the Calabi-Yau. A configuration in which this size and shape changes with time, is described by taking these fields to vary in time.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page

Title Page

◀◀ ▶▶

◀ ▶

Page 13 of 55

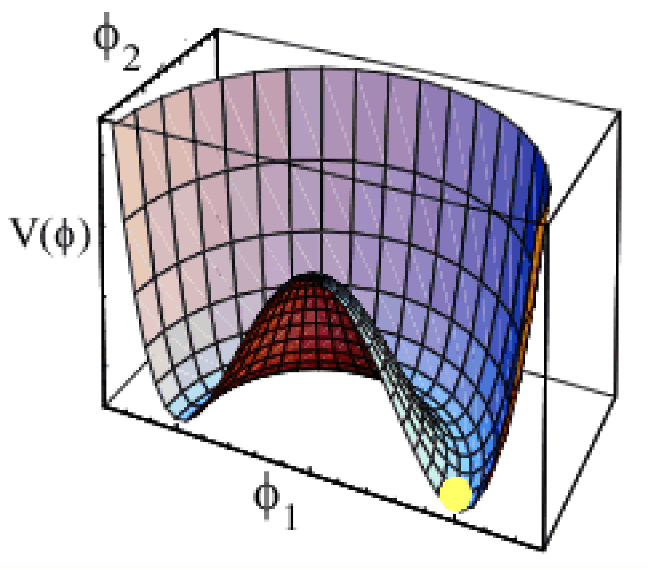
Go Back

Full Screen

Close

Quit

Now, the energy of a configuration depends on the fields, through a function called the "effective potential," denote this $V(\vec{\phi})$.



A vacuum configuration is then a local minimum of V. This is because if V is not at a local minimum, there is potential energy available to turn into radiation, which is favored entropically. The value of V at the minimum is the vacuum energy Λ.

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

Now, in a realistic compactification, the vacuum energy must depend on all of the scalar fields. We know this because any scalar field which did not affect the vacuum energy, would be free to vary (in particle physics terms, it would be massless), and would lead to long-range corrections to the gravitational inverse square law, which are not observed.

Thus, an important element in the problem of showing that a string vacuum can describe the real world, is to show that all the fields enter into in the vacuum energy (the "moduli stabilization problem"), and to find their vacuum expectation values.

The first step in this problem is to compute V, after which the problem of finding vacua becomes an optimization problem.

So what does the effective potential V look like in a typical string compactification? Around 2000, people started to find simple and tractable models for this. The simplest models assume that within the Calabi-Yau manifold, we have a "generalized magnetic field," and the vacuum energy is simply the B^2 energy of this magnetic field. (The generalization is simply that the usual two-form magnetic field of Maxwell's equations becomes a p-form, as is possible in higher dimensions).

Now, on a manifold K with non-trivial topology, there is a law of flux quantization due to Dirac: the (generalized) magnetic field B must satisfy

$$N = \int_\Sigma B \sim \mathrm{Vol}(\Sigma) B \in \mathbb{Z},$$

where Σ is a cycle K (in Dirac's original argument, K was a two-sphere surrounding a magnetic monopole).

This law implies that as the volume of a cycle $\int_\Sigma \sqrt{g}$ grows, the magnetic field per unit volume must decrease inversely. On the other hand, the energy in the magnetic field is

$$V = \int_\Sigma B^2 \sim \mathrm{Vol}(\Sigma) B^2 \sim \frac{N^2}{\mathrm{Vol}(\Sigma)}.$$

Thus, the magnetic field energy depends on the size of the Calabi-Yau. By taking combinations of fluxes, one can get a combination of terms in V such that V depends on all of the fields,

$$V = \sum_{i,j} c_{ij}(\vec{\phi}) N^i N^j - V_0,$$

generalizing the expression $B^2 \sim N^2$ to the case of many components (many non-trivial cycles). We also added a constant term V_0, which often appears in examples.

Such potentials typically have one or several minima, and thus we have a candidate solution to the moduli stabilization problem. Over the last few years, it has been convincingly argued that this solution works in string theory, most notably by Kachru et al (KKLT) 2003.

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But what about the cosmological constant problem? There is no reason for the negative V_0 term to cancel the positive terms to produce a total $V = 0$, and in examples it does not. The point is that, by the time we add all the features of a realistic model, we have so many contributions to V_0, that there is no hope of such a cancellation.

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

The currently accepted resolution of this problem is strange and troubling. Around 1988, Steven Weinberg suggested that the cosmological constant problem might be solved by the following "anthropic" argument.

Suppose there were a large number of vacua $\mathcal{N}$, all candidates to describe our universe, but with different values of Λ, roughly uniformly distributed over a range $(-\Lambda_{max}, \Lambda_{max})$. Since we have a finite set, we do not realize all values of Λ, but it is reasonable to expect to find vacua which realize values as small as $2\Lambda_{max}/\mathcal{N}$.

Thus, even if $\Lambda_{max} \sim M^4_{Planck}$, as long as $\mathcal{N} > 10^{122}$, it is not unreasonable to expect that a vacuum exists with the observed small value $\Lambda \sim 10^{-122} M^4_{Planck}$.

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Now this might not sound like an explanation as one might naturally ask: why do we find ourselves in a vacuum with small Λ, when the overwhelming majority of vacua have large Λ ?

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 18 of 55
Go Back
Full Screen
Close
Quit

This is where the anthropic argument comes in. The "anthropic principle" is simply the statement that, even if some parameter of nature could in principle have taken a different value, it is not necessarily the case that an observer could have seen this other value. Clearly, if a value X were incompatible with the existence of that observer, X would never be observed.

This is where the anthropic argument comes in. The "anthropic principle" is simply the statement that, even if some parameter of nature could in principle have taken a different value, it is not necessarily the case that an observer could have seen this other value. Clearly, if a value X were incompatible with the existence of that observer, X would never be observed.

This near-tautology attains significance in the present context as it is not hard to come up with modifications of the laws of nature which would prevent the existence of life as we know it, and even chemistry, stars, or other structure. One basic example is that the stability of the proton depends on the relation

$$\mathrm{mass(proton)} < \mathrm{mass(neutron)} + \mathrm{mass(positron)} + \mathrm{mass(neutrino)}$$

as otherwise the proton would decay into the three particles on the right. This relation follows fairly directly from the relation

$$\mathrm{mass(up\ quark)} < \mathrm{mass(down\ quark)}.$$

However, in the Standard Model, the quark masses are free parameters, and nothing requires this relation to hold. Suppose we found a microscopic theory which gave rise to a SM-like theory, but in which this relation failed. Clearly this theory would not fit the data; the anthropic principle makes the further statement that one needs no other explanation for the fact that we do not observe this world instead of "our" SM.

Now, granting the anthropic principle, the key point, which requires a bit of astrophysics to check, is that, if Λ were much larger than what we observe (say $\Lambda > 100\Omega$), the accelerated expansion of the universe would start so early in its history, that galaxies, stars and planets would not form. This seems clearly incompatible with the existence of observers and thus we need no further explanation for the fact that we do not live in one of the multitude of universes with large Λ. Indeed, some take the agreement of the bound here (up to a small factor of 100) with the observed value, as support for this explanation.

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The anthropic principle smacks of metaphysics and another attitude one can take (now that Λ has been measured) is that our job as scientists is simply to find out whether our theory can fit the data we observe, in this case small Λ. Either way, the upshot is, in a theory with more than $\mathcal{N} \sim 10^{122}$ different candidate vacua with uniformly distributed cosmological constant, there is no cosmological constant problem.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

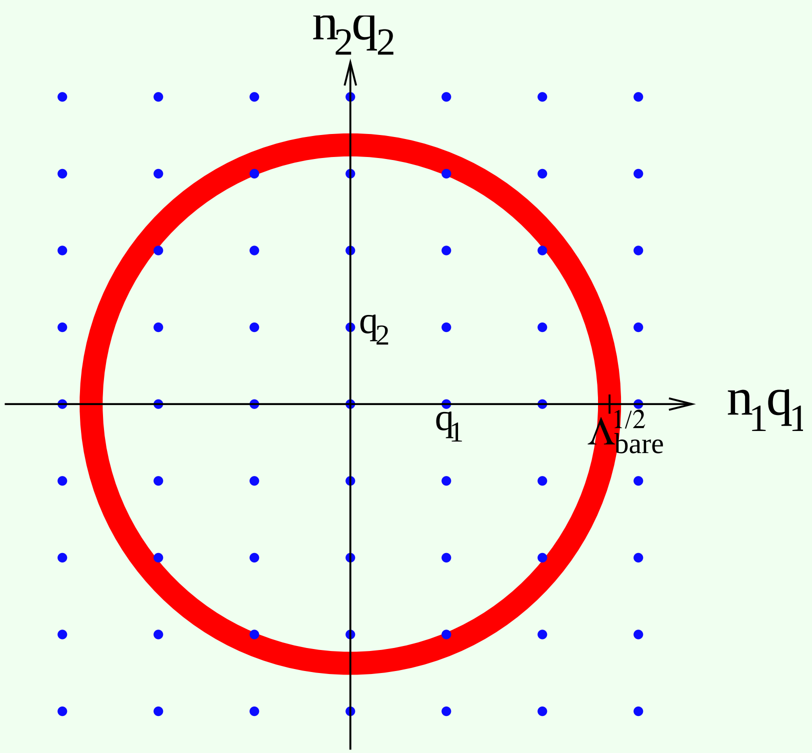
Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 20 of 55
Go Back
Full Screen
Close
Quit

So what about string theory? It was first realized by Bousso and Polchinski (2000) that this argument could be applied here. They argued that, by turning on flux in the extra dimensions, one would find an effective potential of the form we discussed,

$$V = \sum_{i,j=1}^{k} c_{ij} N^i N^j - V_0.$$

They furthermore simplified the problem by taking c_{ij} a generic constant matrix with $O(1)$ coefficients, independent of the fields.

Thus, the set of flux vacua they considered is parameterized by a vector of integers N^i, $1 \leq i \leq k$. What is the set of possible cosmological constants?, Clearly it will be discrete – but does it contain values near zero?



Yes, if there are enough flux vacua. In this figure (from Bousso and Polchinski 2000), the lattice points represent quantized values of the flux, while the red circle is the region with $\Lambda \sim 0$.

$$V = \sum_{i,j=1}^{k} c_{ij} N^i N^j - V_0.$$

The basic estimate for the spacing between values of V near $V \sim 0$ is

$$\Delta V \sim \frac{1}{\mathrm{Vol}(S^{k-1}) V_0^{(k-1)/2} \det c}$$

from the intuition that a shell around zero of volume $\det c \sim 1$ will be expected to contain one vacuum. The dominant factor here is simply V_0. and (taking into account the factor $\mathrm{Vol} S^{k-1}$) for $V_0 \sim 10$ and $k \sim 100$ we find $\Delta V \sim 10^{-122}$ as desired.

Here k is the number of distinct cycles which can carry flux, a Betti number of the Calabi-Yau. As we discussed, this can range up to 960 in the known examples, and $k > 100$ is fairly common. Thus string theory can solve the cosmological constant problem.

4. The landscape

While Bousso and Polchinski's arguments somewhat oversimplified the real situation, this type of analysis was carried out for the real problem of flux vacua in string theory in a series of my papers with Sujay Ashok, Frederik Denef, Bernard Shiffman and Steve Zelditch. The results justified the claim that there are many flux vacua, of order c^k where k is a Betti number. The final numbers which came out of our analyses are more like 10^{300}–10^{500}. The good news is that this is clearly larger than 10^{122}, while the bad news is ...

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While at first a number like 10^{500} seems totally outlandish, it is not totally out of keeping with the underlying nature of the problem. We should compare this to the inverse of the probability that a randomly chosen effective field theory would match the Standard Model, to experimental accuracy. If we draw the 19 parameters from uniform distributions (on $(-1,1)$ in dimensionless terms), we find a volume of around 10^{-80}. Combining this with the c.c., we have the "volume of the Standard Model," about 10^{-200}.

Defining natural distributions for the discrete data (number of generations, rank and factors of the gauge group, etc.) to judge how common is 3 generations and $SU(3) \times SU(2) \times U(1)$ (etc.) requires more detailed string theory input, *i.e.* a rough picture of the actual origins of this structure from string theory.

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The starting point for many (not all) string constructions is a choice of a three complex dimensional Calabi-Yau manifold, such as the quintic. What are the choices here?

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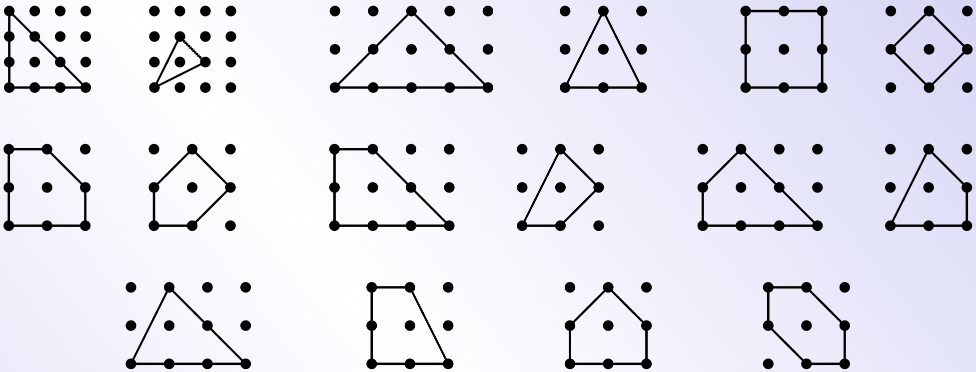
The starting point for many (not all) string constructions is a choice of a three complex dimensional Calabi-Yau manifold, such as the quintic. What are the choices here?

Although the general classification is not known, a significant part of it, the classification of hypersurfaces in toric varieties, has been reduced to a combinatorial problem, and solved. The basic result (Batyrev 1993) is that a class of such d-dimensional manifolds can be defined given a reflexive polytope in $d+1$ dimensions, *i.e.* a lattice polytope L such that L and its dual $\check{L}$ each contain one interior point.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 25 of 55
Go Back
Full Screen
Close
Quit

The reflexive polyhedra in two dimensions:



Note that all of these fall into dual pairs, or are self-dual. Mathematically, such a pair describes a pair of mirror Calabi-Yau manifolds. This mirror symmetry is of central importance in the mathematical and physical analysis and was in part discovered by computer-aided classification of examples.

Kreuzer and Skarke carried out this classification for $d+1=4$ and found 4×10^8 reflexive polyhedra, providing an upper bound on the number of toric hypersurface threefolds. A lower bound of $30,000$ is given by the number of distinct pairs of Betti numbers $(b_{1,1}, b_{2,1})$ which appear.

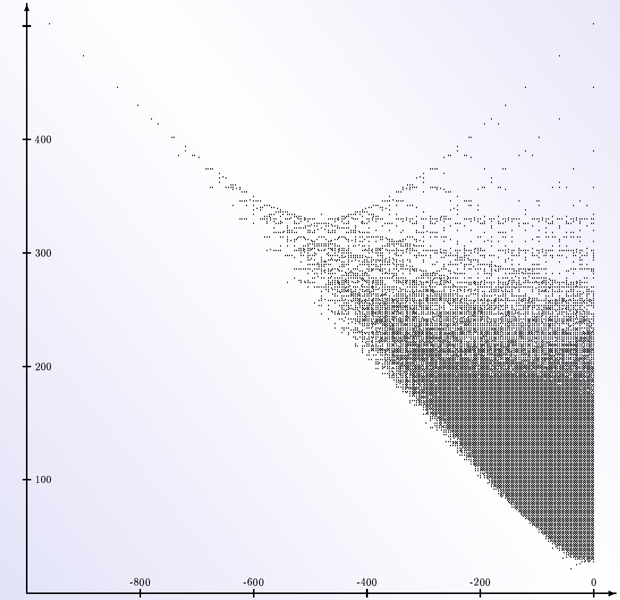


Fig. 1: $h_{11}+h_{12}$ vs. Euler number $\chi=2(h_{11}-h_{12})$ for all pairs (h_{11},h_{12}) with $h_{11}\leq h_{12}$.

From Kreutzer and Skarke, hep-th/0002240.

Listing CY's is just a first step. The subsequent steps are

1. combinatorial – enumerating bundles and brane configurations on the CY manifolds, satisfying various physical constraints.
2. semi-numerical – deriving PDE's (Picard-Fuchs equations, GKZ systems, etc.) for CY periods and solving them, and the same for similar nonlinear recursion relations defining instanton coefficients, etc.
3. joint combinatorial/numerical optimization – finding vacua which minimize the effective potential and satisfy other required properties.

As an example of (2), the potential for compactification on the mirror quintic is derived from solutions of the PDE (Candelas et al 1990)

$$\left(z\frac{d}{dz}\right)^4\Phi - 5^5 z\left(z\frac{d}{dz}+\frac{1}{5}\right)\left(z\frac{d}{dz}+\frac{2}{5}\right)\left(z\frac{d}{dz}+\frac{3}{5}\right)\left(z\frac{d}{dz}+\frac{4}{5}\right)\Phi = 0.$$

In a more general example, this would be a linear matrix PDE in $b_3/2 - 1 \sim 100$ variables. While a straightforward numerical approach is hopeless, to some extent exact solutions for these equations can be written down (they are hypergeometric functions with integral representations) and qualitative understanding of these is a reasonable short term goal.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page

Title Page

◀◀ ▶▶

◀ ▶

Page 28 of 55

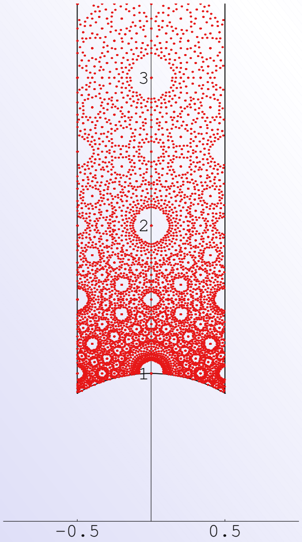
Go Back

Full Screen

Close

Quit

Here is the distribution of flux vacua in a simple (one parameter) model problem (the rigid CY):



This graph was obtained by enumerating one solution of $a_1 b_2 - a_2 b_1 = L$ in each $SL(2,\mathbb{Z})$ orbit, taking the solution $\tau = -(b_1 - ib_2)/(a_1 - ia_2)$ and mapping it back to the fundamental region.
The total number of vacua is $N = 2\sigma(L)$, where $\sigma(L)$ is the sum of the divisors of L. Its large L asymptotics are $N \sim \pi^2 L/6$.
From Denef and Douglas, 2004.

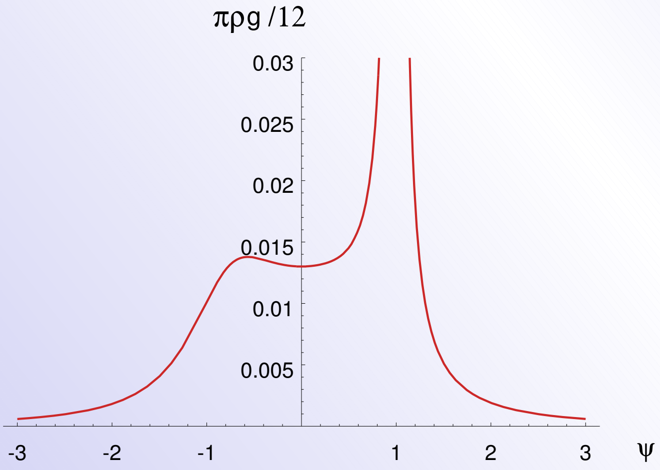
A similar enumeration for a Calabi-Yau with n complex structure moduli, would produce a similar plot in $n+1$ complex dimensions, the distribution of flux vacua. It could in principle be mapped into the distribution of possible values of coupling constants in a physical theory.

One can get simple asymptotic formulae for such distributions, in terms of the geometry of the underlying configuration space, for example the "index density" formula

$$\rho_I(z,\tau) = \frac{(2\pi L)^{b_3}}{b_3!\,\pi^{n+1}} \det(-R - \omega \cdot 1).$$

Here ω is the Kähler form and R is the matrix of curvature two-forms.

Here is a one dimensional slice of the resulting distribution in a two-parameter example (the mirror quintic, from Denef and Douglas 2004):



Note the singularity at $z = 1$. While in this case and in great generality, the distribution is integrable and the total number of vacua is finite, this is not known in general, and is clearly an outstanding question. It is only true if certain "cuts" are placed eliminating obviously non-physical vacua (say, with extra dimensions visible to the naked eye), but in all examples considered so far is true under this definition.

Granting this, to continue our discussion of numbers, the combination of such results suggests that the Standard Model spectrum appears in 10^{-10}–10^{-20} of the vacua, while other structure required to obtain the Standard Model (supersymmetry breaking, realistic inflationary cosmology) is similarly constraining.

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

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There are many other consistency conditions which have to do with stability of the vacuum, physics yet to be seen such as dark matter, and early cosmology. In certain cosmological frameworks, one also obtains a "measure factor" which can weigh different vacua with a "probability" or "amplitude." It is not known how constraining any of these factors are.

Suppose they were constraining on (say) the 10^{-200} level, then even 10^{400} vacua would not be enough to make it *a priori* likely that string theory reproduced the data, unless the distribution has more structure. Thus, the theory will be testable in principle: even if we do not find direct evidence for strings, extra dimensions or other truly novel physics, just matching the data will be significant evidence.

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Of course all this is a drastic oversimplification of the real problem, but is intended to make the point that these seemingly outlandish numbers are not in immediate conflict with the claim that string theory will someday produce concrete and falsifiable predictions. This could have failed, say if there were 10^{5000} instead of 10^{500} candidate vacua.

The upshot is that string theory seems to have more than enough complexity to lead to theories like the SM, indeed a bit too much. Of course, it is far less than the complexity which emerges from Schrödinger's equations – besides atoms, one has molecules, crystals, chemistry, etc. For example, the molecular dynamics of a 20 residue amino acid chain is described by a potential landscape with ~ 500 variables, about the same number. And this would be a small protein, accessible to computer simulation. But the idea that any comparable degree of complexity is emerging from a fundamental theory, is astonishing.

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

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Of course, the situation might again turn out analogous to the periodic table before quantum mechanics, particle physics before the SM, etc. We might reasonably hope and search for a far simpler description or mathematical framework from which this complexity emerges. But, according to string theory as we understand it now, these distinct vacua would remain possibilities within the theory, and we would still face the problem of finding the right one(s).

5. Issues in flux vacua

Of course, the existence of 10^{500} vacua within string theory is hardly proven, even to physics standards. There are various points in the present arguments which one might examine critically (*e.g.* see my RMP review with Kachru):

- The effective potential makes sense. In particular, we can add up terms and corrections from various sources (fluxes, quantum corrections, instantons) and get a sensible controlled result.
- Any sufficiently long-lived local minimum is a candidate vacuum.
- Most of the flux vacua are very long lived $(T_{decay} >> 10\ \mathrm{Gyr})$.
- We need not worry about singularity formation in the distant future, or other acausal or nonlocal consistency conditions.
- Most of the flux vacua can be created by early cosmology.
- The measure factor (more on this later) is "reasonably uniform," *i.e.* not overwhelmingly concentrated on a few vacua.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

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While there is no clear idea as to what the "nonperturbative consistency conditions" might be, a concrete idea along these lines is to look at flux vacua with negative cosmological constant. These are compactifications to AdS_4 and as such should have dual gauge theories. On the other hand, despite attempts (Silverstein 0308175, Aharony et al 0801.3326, and others), these theories have been hard to find explicitly, raising the possibility that some or all of them actually do not exist.

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What makes these theories strange from the AdS/CFT point of view, is that we are asking for the (negative) cosmological constant to be very small compared to the KK and all other scales in the problem, because of cancellations: we have

$$\Lambda = V = -3|W|^2 = -3|\sum_i N^i \Pi_i|^2$$

where Π_i is a basis of periods (in IIb CY flux compactification, periods of the holomorphic 3-form). Generally the individual $|\Pi_i| \sim 1$, and $|W|$ is small in a few vacua because of cancellations. On changing a single flux $N_i \to N_i \pm 1$, it will typically vary wildly.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 35 of 55
Go Back
Full Screen
Close
Quit

Then, the AdS radius R determines the number of degrees of freedom (central charge) of the dual gauge theory, in AdS_4 as

$$c \sim R^2 M_{pl}^2.$$

Thus the number of degrees of freedom varies wildly as well.

This is by no means impossible. Presumably the fluxes N_i enter into the bare Lagrangian, but the CFT is obtained from this by an RG flow. The resulting number of degrees of freedom might indeed vary wildly – indeed this is the case in duality cascades, which end up with gauge theories with $N \sim \gcd(N_1, N_2, \ldots)$. But it is a bit strange.

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The case of IIa flux vacua is somewhat better as there are series of vacua, running off to large volume and weak coupling, in which the relation between flux and Λ is simpler. In IIa on $T^6/\mathbb{Z}_2$ orientifold, taking all fluxes $O(N)$, one sees that (Aharony et al)

$$c \sim N^{9/2}.$$

Again, while not impossible, for this to come out of a gauge theory requires either very large gauge groups (say $N_{eff} \sim N^3$), or very high rank matter representations.

Furthermore, in AdS/CFT, the AdS radius sets the scale of the relation between masses of bulk fields, and operator dimensions in gauge theory. We are particularly interested in vacua in which $\Lambda << M_{KK}$ and all other scales. This translates into a large gap in the spectrum of operator dimensions: a few operators with dimension $\Delta = d = 3$ such as the stress-energy tensor and conserved currents, and then no operators until $\Delta \sim c$.

This is much like the extremal 2d CFT's recently suggested by Witten to describe 3d pure gravity. In the case of 2d $(2,2)$ SCFT, there are strong constraints from modular invariance of the partition function, which actually seem to forbid the existence of these extremal CFT's except for fairly small c (see Gaberdiel et al 0805.4216). On the other hand "near-extremal CFTs" with $\Delta \geq 7/8c$ seem to be possible; also the 3d flux vacua of interest would correspond to $(1,1)$ SCFT.

To summarize, there is no clear reason the dual gauge theories cannot exist, especially since we only expect these properties in a small "tuned" subset of theories. Furthermore, the flux vacua achieve small Λ by precise cancellations in a classical large volume regime, and this might be expected to be very difficult to see in dual gauge theory. Still this deserves study.

6. Measure factor

Could the vacuum be determined by early (Planck time) cosmology?

It is clear that this must be described by a theory of quantum gravity. Thus one expects a theory to determine, not a specific classical initial condition, but rather a wave function. According to the standard interpretation of quantum mechanics this will lead to a probability distribution over vacua, the measure factor.

At first such a thing sounds unobservable, indeed more or less crazy. After all we live in one universe and even if our theory predicts that our particular universe is “low probability,” it is not clear that that falsifies the theory.

On the other hand it does seem reasonable that if two vacua V_1 and V_2 both fit present data, $P(V_1) << P(V_2)$, and we then do an experiment fit by V_1 and not V_2, this should count as evidence against the theory (including the prescription which determined the measure factor). So, leaving aside philosophical considerations about what this means and how significant a given probability might be, the measure factor seems worth studying.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

A lot of work has been done on the measure factor and at present there is no candidate prescription which is generally agreed to be sensible. See work of Linde, Vilenkin, Bousso among others.

The general idea is that there is some sort of dynamics according to which regions of space containing a particular vacuum configuration are created (say by inflation), make transitions (say by tunnelling), or are destroyed. We then postulate $N(i)$, a "number of regions of vacuum i," and write an equation like

$$\frac{d}{dt}N(i) = \sum_j P(j \to i)N(j) + \text{source} - \text{sink}.$$

Of course the appearance of "time" t here is problematic and one tries to eliminate this in favor of a discrete dynamics. One then defines the probabilities as

$$P(i) = \frac{N(i)}{\sum_j N(j)}.$$

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We avoid details and instead discuss a few of the most salient differences between proposals.

- Finite vs infinite.
  If the $N(i)$ are infinite, the ratio defining $P(i)$ must be defined as a limit. In practice this is usually terribly ambiguous. It is tempting to fix this by saying that "complementarity" makes only one causal region observable, and the number of vacua in such a region is finite. However this has not been understood.

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- Uniform vs highly peaked.
  This can be quantified by the Shannon entropy

$$S = \sum_i P(i) \log \frac{1}{P(i)}.$$

Clearly peaking improves predictivity. On the other hand, the anthropic solution to the c.c. problem only works if the distribution contains of order $1/\Lambda \sim 10^{120}$ probable quasi-realistic vacua, so it cannot be too peaked.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions
Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

- Equilibrium vs non-equilibrium.
  It is very tempting to postulate that the distribution $N(i)$ reaches an equilibrium state, $dN/dt = 0$. After all it has "infinite time" to do so.
  However this turns out to be very problematic as it is hard to avoid conclusions like "$P(i)$ is proportional to the number of microstates which correspond to the vacuum i." This tends to lead to distributions which are very highly peaked on obviously unrealistic vacua, and thus falsified.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions
Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 41 of 55
Go Back
Full Screen
Close
Quit

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  One simple way to get a non-equilibrium distribution is to have many sinks. For example one can claim that any AdS vacuum is a local endpoint for the dynamics and thus a sink. This leads however to dependence on the initial conditions (see recent work of Bousso) and it is not clear that there is any preferred initial condition.

The anthropic argument for the c.c. suggests that the measure is roughly uniform, at least in the c.c.

It is also very plausible that the measure is decorrelated from almost all details of low energy physics, such as the gauge group, matter content and couplings of the Standard Model. This is because the dynamics which determines it is taking place at very high scales (it must happen before the observed period of inflation) at which the structure of the SM is irrelevant.

If so, this is a good argument to consider the measure as effectively uniform in (independent of) all of these parameters. Even if it varies, this variation will be effectively random and will drop out when we sum over many vacua.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 42 of 55
Go Back
Full Screen
Close
Quit

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If so, this is a good argument to consider the measure as effectively uniform in (independent of) all of these parameters. Even if it varies, this variation will be effectively random and will drop out when we sum over many vacua.

On the other hand, it would be reasonable to expect the measure factor to depend on the properties which are important in early cosmology, such as the scale of inflation and the volume of the extra dimensions.

7. An example: the scale of supersymmetry breaking

To simplify, let us focus on two parameters, the supersymmetry breaking scale F, and the scale of electroweak symmetry breaking $M_{EW} \sim 100$ GeV.

Imagine that we are about to do an experiment which will detect superpartners if $F < F_{exp} = 1$ TeV. Then, the probability with which we expect to discover supersymmetry would be

$$P_{susy} = \sum_{F_i \leq F_{exp}, M_{EW,i}=100\ \mathrm{GeV}} P(i). \tag{1}$$

If this probability were high, we would have derived a top-down prediction of TeV scale supersymmetry.

But, from what we know about string theory, do we know it will be high? Might it instead be low, so that the discovery of TeV scale supersymmetry would in some sense be evidence against string theory?

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 43 of 55
Go Back
Full Screen
Close
Quit

Let us grant that the measure factor $P(i)$ is independent of the scale of supersymmetry breaking, so $P(i) = 1/N$ for each of the N vacua in the landscape. This may or may not be true. But, if the dynamics determining $P(i)$ takes place at scales far higher than that of supersymmetry breaking, it is plausible.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions
Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 44 of 55
Go Back
Full Screen
Close
Quit

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Now, let us rephrase the usual argument from naturalness in this language. We focus attention on the subset of string/M theory vacua which, while realizing all the other properties of the Standard model, may have a different value for the electroweak scale M_{EW}. Since this is quadratically renormalized, in the absence of any other mechanism, we expect that the fraction of theories with $M_{EW} < M_{EW,max}$ should be roughly

$$\frac{M^2_{EW,max}}{M^2_{cutoff}} \sim 10^{-30}$$

taking $M_{cutoff} \sim M_{GUT}$ for definiteness. While small, of course given enough vacua, we will find vacua in which the hierarchy is a result of fine tuning.

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Let us now grant that we have some subset of the string theory vacua in which the Higgs mass is determined by supersymmetry breaking as above. Then one might expect an order one fraction of these models to work.

Now, the naturalness argument is the claim that, since most of the TeV scale supersymmetry vacua work (fit the data), while only 10^{-30} of the fine tuned vacua work, we should expect to live in a universe with TeV scale supersymmetry, or at least prefer this alternative to the fine-tuned models.

The gap in this argument is evident. It is that, even though the fraction of fine-tuned vacua which work is relatively small, if their number is large, we might find in the end that far more of these vacua work than the supersymmetric vacua. Given our hypothesis, string theory would then predict that we should *not* see supersymmetry at the TeV scale.

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Is this what we expect or not? Before taking a position, one should realize that the additional structures being postulated in the supersymmetric models – the scale of susy breaking, a solution to the μ problem, a mediation mechanism in which FCNC and the other problems of generic supersymmetric models are solved, and so on – each come with a definite cost, not in terms of some subjective measure of the complexity or beauty of the theory, but in terms of what fraction of the actual string/M theory vacua contain these features. Is this cost greater than 10^{-30} or not?

While this may someday lead to a top-down prediction, present arguments are rather exploratory.

First, it seems clear that there is a large supply of flux vacua which break supersymmetry just because there are "random minima" in the effective potential. A simple argument (Denef and Douglas, Dine, O'Neil and Sun) shows that the number distribution of these vacua is

$$dN(M_{susy}) \sim d(M_{susy}^{12}),$$

simply because 3 complex parameters (of mass dimension 2) must be tuned in the superpotential to get this.

This distribution far outweighs the advantage of supersymmetry in solving the hierarchy problem, and thus these vacua disfavor low energy susy.

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On the other hand, there are clearly gauge theories which dynamically break supersymmetry at an exponentially small scale. Indeed, recent work (Intriligator, Seiberg and Shih 2006; Ooguri et al, Giveon and Kutasov) shows that if we allow metastable vacua, there are many such theories. Perhaps they are an "order one fraction" in the distribution of theories produced by string compactification.

On the other hand, the ISS model and other models found to date, do not work unless one postulates an additional small parameter. In ISS for example, one needs the quark mass $m << \Lambda$. Thus one has not escaped the need for tuning.

Models have been proposed in which this is done by "retrofitting," *i.e.* a second gauge sector is introduced and coupled so that $m \sim \Lambda_2$ which can be exponentially small. However getting this to work requires discrete symmetries, in particular discrete R symmetry. And this also seems to be highly nongeneric in flux compactification (Dine and Sun).

So at present there is no clear justification in the string theory landscape for low energy supersymmetry, however perhaps models will be found.

The other phenomenological problems (μ problem, little hierarchy problem) are also serious. It is hard to escape the conclusion that either

- The tuned Standard Model is a very simple realization of the hierarchy and could well be preferred in the landscape, or
- There is some other yet to be discovered solution, simpler than the existing supersymmetric and other models.

8. Finding the right vacuum

Let us grant that the picture remains as we have described and ask: suppose we had an algorithm for enumerating and computing the exact predictions of each vacuum. We just outlined an analysis of testability in principle – but can we ever carry this out in practice?

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page

Title Page

◀◀ ▶▶

◀ ▶

Go Back

Full Screen

Close

Quit

8. Finding the right vacuum

Let us grant that the picture remains as we have described and ask: suppose we had an algorithm for enumerating and computing the exact predictions of each vacuum. We just outlined an analysis of testability in principle – but can we ever carry this out in practice?

While we do not have an exact formulation to work from, let us consider our present-day approximations to the problem, and see what they suggest.

As we discussed, a basic element of the problem of describing our universe, is to match the small observed value of the cosmological constant, roughly $\Lambda = 10^{-122}$ in Planck units. Our present belief is that this can be done in string theory, only by finding a particular vacuum with this energy, *i.e.* a minimum of the effective potential V at which $V = \Lambda$. Thus, to identify candidate vacua and test the theory, we should try to find such vacua.

Of course, matching the precise value we observe is perhaps asking too much, but bounding the cosmological constant above by $100\Omega \sim 10^{-120}$ (and below by -5Ω or so), is a requirement for any reasonable vacuum.

Now, let us consider the Bousso-Polchinski model for the effective potential,

$$V = \sum_{i,j=1}^{k} c_{ij} N^i N^j - V_0,$$

where $N^i \in \mathbb{Z}^k$, c_{ij} is a symmetric real matrix with known generic order one coefficients, and V_0 is a known positive number of order $100k$. (In the real problem, these coefficients are computed by minimizing solutions of the Picard-Fuchs PDE's, summing instanton expansions, and similar computations).

Earlier, we gave a simple statistical argument that for $k \geq 100$, it is plausible that a satisfactory vacuum should exist. But suppose we wanted to prove this statement, and go on to find such a vacuum?

A naive approach would be sequential search: try each candidate vacuum (in some order), compute its vacuum energy, and check. While the problem is finite, we need to search through of order 10^{120} candidates to make it likely to find one which works, so clearly this is infeasible. So is there a better way?

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 49 of 55
Go Back
Full Screen
Close
Quit

Using techniques from computational complexity theory, Frederik Denef and I have shown that this is an NP complete problem. This means that, if we consider arbitrarily large problem instances, and allow ourselves to freely choose the coefficients, we can encode arbitrary NP problems in this problem. Thus, the worst case behavior is exponential in k; indeed for many problem instances, even the best algorithms are not much better than exhaustive search.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit

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Similar problems arise in other toy models of the landscape. For example, in a model suggested by Arkani-Hamed, Dimopoulos and Kachru (2005), matching the cosmological constant reduces to the knapsack problem: given a list of numbers α_i, find a subset which adds to 1 (within some error bound). This problem is also NP complete, as are some of the combinatorial problems which arise at earlier stages of the classification of vacua.

Indeed, the NP hardness of the problem of finding the minima of realistic potential energy functionals is well-known. It is a central aspect of the theory of spin glasses, materials in which randomness in the lattice structure leads to a complicated, quasi-random arrangement of interactions. A standard model of this type is the Sherrington-Kirkpatrick model,

$$V = \sum_{1 \le ij \le N} J_{ab} \sigma_a \sigma_b$$

with $\sigma_a = \pm 1$, and interaction strengths J_{ab} drawn from a random distribution which contains both signs. Note the similarity to Bousso-Polchinski. This model has long been known to have an exponentially large number of local minima (under flipping single spins), making the problem of finding the global minimum (for a chosen J_{ab}) is extremely difficult.

The basic reason for this is frustration. Given two spins, there is always a favored (lower energy) pattern, say $\sigma_1 = -\mathrm{sgn}\ J_{12}\sigma_2$. But given three spins $\sigma_{1,2,3}$, if

$$\mathrm{sgn}\ J_{12}J_{23} \ne \mathrm{sgn}\ J_{13},$$

then their mutual interactions compete, and it is not possible to minimize all of these energies simultaneously.

Thus, if the anthropic solution to the *c.c.* problem is right, we may never be able to find the configuration which describes our vacuum. This sounds like a disaster – is it?

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Not necessarily. We will probably never get direct experimental evidence for the flux sector. What we really care about is testing the observable predictions of string theory. In other words, we must find that the matter sector agrees with the SM at low energy, and may contain some interesting extension of it at higher energies such as supersymmetry, large extra dimensions, or other BSM physics. The relation between this problem and the c.c. is very indirect.

Introduction
String/M theory
The Λ problem
The landscape
Issues in flux vacua
Measure factor
An example: the . . .
Finding the right . . .
Conclusions

Home Page
Title Page
◀◀ ▶▶
◀ ▶
Page 52 of 55
Go Back
Full Screen
Close
Quit

Indeed, consideration of the structure of flux vacua makes it very plausible that the distribution of c.c.'s is decorrelated from the matter sector and other observed physics. In other words, the number of flux vacua is roughly independent of the gauge group and other potentially observable parameters, and the c.c. is always uniformly distributed. This follows from formula such as

$$V = V_{obs} + \sum V_{hidden,i}$$

or

$$V = \sum_i |D_i W|^2 + \sum_\alpha (D^\alpha)^2 - 3|W|^2.$$

according to which the total vacuum energy is a sum of observable and many uncorrelated "hidden sector" contributions.

Thus we do not need to know the details of how hidden sector physics tunes the cosmological constant. However this structure might make a big difference in the number of vacua with different properties or their measure factor. Fortunately this type of question can be computationally tractable.

Introduction

String/M theory

The Λ problem

The landscape

Issues in flux vacua

Measure factor

An example: the . . .

Finding the right . . .

Conclusions

Home Page

Title Page

◀◀ ▶▶

◀ ▶

Page 53 of 55

Go Back

Full Screen

Close

Quit

9. Conclusions

The search for the fundamental laws of physics draws on three sources: high energy physics, cosmology, and theoretical physics. In the last decade, it has been cosmology which provided the most dramatic new insights, with the confirmation of the ΛCDM paradigm, and the discovery of dark energy.

9. Conclusions

The search for the fundamental laws of physics draws on three sources: high energy physics, cosmology, and theoretical physics. In the last decade, it has been cosmology which provided the most dramatic new insights, with the confirmation of the ΛCDM paradigm, and the discovery of dark energy.

Now the field stands at a crossroads. With the imminent turn-on of the Large Hadron Collider at Cern, we can hope to open a new frontier in high energy physics, perhaps discovering supersymmetry, or something even more dramatic.

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Now the field stands at a crossroads. With the imminent turn-on of the Large Hadron Collider at Cern, we can hope to open a new frontier in high energy physics, perhaps discovering supersymmetry, or something even more dramatic.

From the point of view of string theory and theoretical physics, we can imagine three general outcomes:

- Dramatic discoveries producing significant evidence for extra dimensions, or other features of string theory.
- Unexpected discoveries which cannot be fit into any existing theoretical paradigm.
- New discoveries which can be fit using four-dimensional effective field theory, such as supersymmetry. In this case, we will need to think very hard about the landscape and predictivity, in order to make contact with string theory.

Home Page
Title Page
◀◀ ▶▶
◀ ▶

Go Back
Full Screen
Close
Quit