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The Emperor's Last Clothes?

Overlooking the String Theory Landscape[1]

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Abstract

We are in the middle of a remarkable paradigm shift in particle physics, a shift of opinion that occurred so slowly that some even try to deny that they changed their minds at all. It concerns a very basic question: can we expect to derive the laws of particle physics from a fundamental theory? The Standard Model of particle physics as well as the 1984 string theory revolution provided ample food for thought about this. The reason this was ignored for so long can be traced back to an old fallacy: a misguided idea about our own importance.

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1 Introduction

In the last five years, a big debate has erupted concerning a very basic question: are the laws of physics we observe unique, or are there various distinct possibilities? Understanding this requires a definition of what is meant by "the laws of physics" and what could be non-unique about them, and we will get to that later. But even before going into detail it should be obvious that the answer to this question is of fundamental importance. It would have interested Einstein very much, judging from his famous phrase *"I wonder if God had any choice when he made our universe."* The answer to this question touches on deep existential questions that have troubled mankind for centuries. If different laws of physics are possible, we – or anything else of comparable intelligence – might not exist in some of those different circumstances.

Physicist tend to get nervous about this sort of statement, and not without reason. Ideas of this kind go by the generic name "anthropic principle", but there are many different formulations, ranging from tautological to just plain ridiculous. But advances in string theory in the past three decades have moved these questions from the area of philosophy into physics and mathematics. String theory suggests a very concrete answer to this question: it suggests that the laws of physics (and in particular the properties of fundamental particles and interactions) are highly non-unique, but in a precise and quantifiable way. Whether this is indeed a correct statement about string theory can be decided without invoking philosophy. It is a matter of using the traditional methods of physics: finding controllable approximations, performing calculations, and making use of the powerful machinery of mathematics.

While it is more and more widely accepted that string theory does indeed have this property, it remains to be demonstrated that string theory is indeed the fundamental theory of all interactions that it is hoped to be, and of course the answer a theory gives is irrelevant if that theory is wrong. But it already deserves credit for forcing us to ask the question about uniqueness of the laws of physics in a much more precise way. This was never done before, because we never had a candidate theory that was so ambitious.

Initially, when string theory was touted as the "theory of everything" around 1984, there were hopes it would lead to exactly the opposite: a unique derivation of all the laws of physics. Evidence that quite the opposite was true started emerging almost immediately after 1984, but most people chose to ignore it. In 2003, after important additional evidence had been found, Leonard Susskind published a paper [2] entitled "The Anthropic Landscape of String Theory", which finally started a debate that should have started fifteen years earlier. What is at stake in this debate is not only the uniqueness of our universe, but also the fate of string theory as a fundamental theory of all interactions.

In my opinion string theory gives the right answer, and the fact that it does adds to the evidence in its favour. I can say this without being accused of trying to put a positive spin on the recent developments, because I actually wrote in 1998 [3] that I hoped string theory would ultimately lead to a huge number of possible choices for the laws of physics, a point of view I have been advocating since the late eighties. I reached that conclusion after having been involved in one of the first papers [4] pointing out that the number of

possibilities was humongous. Observing that the number of possibilities was huge, in the approximation used at the time, was a relatively simple matter. But it was not obvious what to conclude from that. Most people blamed on our lack of understanding of the theory. Indeed, there were, and still are, many serious problems to be addressed.

My own conclusion was that we were in the process of uncovering a fundamental property of nature. String theory suggested a fantastic solution to a deep mystery: why the laws of physics we observe seem engineered especially for us to exist, even though theoretically there seem to be many other possibilities. We all hope to live during a time when big things are happening in our field, and I have never doubted that this is one of those things. I have spent the last twenty years trying to convey my sense of excitement to my colleagues, but with little success. But in the last few years I have been delighted to see more evidence coming in supporting this point of view, so that the mood has started to change. I hope this is the right time to make one more attempt.

Since I have been defending this point of view for a long time already, I knew it was controversial. Therefore I was not surprised that many people rejected Susskind's conclusion or called it "premature". This is a reaction I can understand. Scientific debates about the validity of approximations or of the validity of a theory are normal. However, there was another kind of reaction to Susskind's paper that inspired me to choose the title of this paper. Some string theorists claimed that they had reached the same conclusion already a long time ago, but did not find it worth mentioning. Apparently they did not find it worthwhile to point out that many great physicists had the wrong expectations about fundamental physics. They do no think it is of any interest to know that our universe is just one of a huge number of theoretically possible ones, and that it is fundamentally impossible to derive it in full detail from a fundamental theory. They do not seem to care about the existential consequences of this large number of possibilities. They do not find it important that a theory they have invested years of their life in has lead us to such a startling conclusion, and they do not seem to care about the implications for the correctness of that theory. Or do they secretly hope that the conclusion will turn out to be wrong after all? Sadly, since nobody found it worthwhile to speak or write about this, we will never know what they really thought.

String theorists who say that the conclusion is premature could simply be right. String theorists who claim that they think it is correct, but that they "knew this already" and did not find it worth mentioning have missed one of the most fundamental scientific discoveries of the last decades. Unless it is all wrong, but that seems hardly an attractive alternative for a string theorist.

The inspiration for the title of this paper is the well-known fairy tale about the emperor and his new clothes, advertised to be invisible only to stupid and incompetent people. Since nobody wanted to belong to that category, all claimed to be able to see the clothes, until a small child said "but he is not wearing anything". Then suddenly everyone claimed to have noticed this already.

1.1 Unification versus uniqueness

But there is another moral that fits the recent events. Two concepts that played an important role in the history of physics are *unification* and *uniqueness*. Sometimes these concepts seem to be inadvertently confused.

Unification means reducing the laws of physics to fewer and fewer fundamental principles. This has been a fantastic success. Many breakthroughs in physics can be viewed in terms of unification. The gravitational force acting on object on the surface of the earth turned out to be the same as the ones governing the motion of planets; inertial mass the same as gravitational mass; electricity was found to be related to magnetism, and the unified theory of both forces is responsible for such diverse phenomena as light and atomic physics, chemistry and the structure of matter. Furthermore, in the second half of last century we learned that two other forces of nature, the weak force (responsible for radioactive decays) and the strong force (binding quarks into nucleons), are based on the same principles as electromagnetism, the principle of local gauge invariance. If string theory is correct the success of unification continues in a spectacular way. It unifies all interactions with gravity, and all matter with interactions.

Uniqueness concerns the question if the laws of physics we observe in our universe are the only ones possible. Historically, whenever alternatives were imaginable, the hypothesis of uniqueness has almost systematically been a failure. The origin of this fallacy can often be traced back to an – explicit or implicit – belief that there is something special about us, or about our planet, or our solar system. For centuries it was believed that the entire universe was a unique structure with the earth in its center. This was corrected by Copernicus, who put the sun in the center of the solar system. But he also put the sun in the center of the universe, a similar fallacy. The idea that the solar system was something special is evident in the work of Kepler, who tried to derive the sizes of the planetary orbits from a mathematical principle. He was even moderately successful in identifying the ratios of these sizes for the first five planets using nested platonic solids. The fact that he tried this implies that he was thinking of the solar system in terms of a mathematically unique system. Giordano Bruno speculated about other solar systems with other kinds of life in them, and paid dearly for that (although not only for that). This particular issue is of course still undecided, but it illustrates the strong sentiments that arguments against our uniqueness evoke. Fortunately they have become a bit less emotional more recently. In 1920 there was the so-called "Great Debate" (it got the name because it concerned the scale of the universe) between the astronomers Shapley and Curtis. The former defended the point of view that our galaxy was the entire universe, but that our solar system was not in its center, while the latter claimed that the then observed "spiral nebulae" were actually other galaxies. However, Curtis also defended the point of view that the Milky Way was centered around our solar system. Each defended an anthropocentric statement, and was fully aware of that. Each of them later turned out to be wrong on that statement. The mistaken concept of human uniqueness not only plagues physics and astronomy, but also biology, and is at least to some extent the origin of the strong resistance against evolution, which took the human being from the pedestal where it once placed itself.

The debate that is the subject of this article fits nicely in the foregoing list. Although it was triggered by string theory, it could and should have started independently. It is the structure of the Standard Model itself that raises the question. String theory suggest an answer that would fit perfectly in the brief history presented above.

Of course a historical analogy can never be used as a proof. However, it can be used to understand the importance of a conclusion, and the resistance against it. In the course of history, ideas of this kind have never been received with much enthusiasm, as Galileo and Giordano Bruno found out. Unification sells much better than non-uniqueness.

This is the second moral: the emperor is us, and the beautiful clothes are the strange ideas we have about our place in the universe. Repeatedly we have been shown to be naked, and I am convinced that in the last decades this has been happening again.

Let me make it clear that I am not trying to portray great scientists like Ptolemy, Copernicus, Kepler and others as fools. My point is quite the opposite: to show that even the greatest minds of their time can make the obvious anthropocentric mistake of thinking that what we see around us is all there is. I might be making that mistake myself somewhere in this article, and I will try to point out where that could be the case.

1.2   Contents

This article has two parts, devoted to two essential ingredients of the debate. The first part follows a "bottom-up" way of thinking. It contains a brief introduction to the Standard Model of particle physics, which leads us naturally and inevitably to the question how it might come out of a fundamental theory, and whether we can expect this to happen in a unique way. I will explain in particular why the Standard Model forces us to think about that question much more seriously than any of its predecessors. The second part takes a "top-down" point of view. I will consider string theory as an example of what a fundamental theory of quantum gravity might have to offer as an answer to this question. In between these two topics I will discuss a concept that inevitably comes up in connection with these discussions, and that tends to confuse the issue and stir up strong emotions: The "anthropic principle".

The main focus of this article is on the Standard Model of particle physics and the anthropic implications of its embedding in string theory. Other essential ingredients in the history of anthropic arguments, in particular from cosmology, will only be mentioned briefly.

The assumed background knowledge is quantum mechanics, some basic group theory and some facts about elementary particle physics. A few parts on the latter subject are perhaps a bit more advanced, but they can be skipped without missing the main points.

This article summarizes twenty years of passionate and vigorous discussions. It contains answers to most of the questions that came up during these debates or afterwards. I wish to thank everyone who contributed their point of view and thereby influenced mine.

2 The Standard Model

2.1 Towards the Standard Model

In the beginning of last century, after Einstein formulated his theory of gravity, General Relativity, it may have been natural to assume that a unique and unified theory might be emerging. Both Maxwell's theory of electromagnetism and General Relativity have an air of uniqueness about them. Could the laws of physics be anything else than this? The quest for a unified theory dominated the rest of Einstein's scientific career, and it is plausible that he also hoped or expected the result to be unique.

But then the experimental facts started pointing in a different direction. New interactions were discovered that did not seem to have an equally simple and elegant description: the strong and the weak interaction. New particles were discovered that nobody needed, such as the muon. And new parameters emerged that did not seem to have a simple explanation, such as the masses of all these particles. The resistance against new particles was so strong that when Dirac discovered the positron he was so reluctant to introduce a new particle that he tried to identify it with the proton, even though he knew that the positron and the proton had totally different masses.

An important theoretical development also put the idea of uniqueness into doubt. Yang-Mills theory, a generalization of electrodynamics, was discovered. Electrodynamics is a "gauge theory" based on the group $U(1)$. For readers not familiar with the concept of a gauge theory the only important point to note is that $U(1)$ is merely an example of a group that can be used in their construction. But there are many other possibilities that work equally well from a theoretical point of view: for example $SU(N)$ and $SO(N)$ for any integer N. It just so happens that the first example of a gauge theory we encountered in nature was based on $U(1)$, but that does not make it unique (actually, the physics of the other possibilities is more complicated, making them much harder to observe directly).

Indeed, nature does make use of these other possibilities: $SU(3)$ for the strong interactions and $SU(2)$ for the weak interactions. This result emerged during the sixties and seventies, and put all the interactions once again on a common conceptual footing: all turned out to be based on the principle of "local gauge invariance", which is a close relative of the concept of general coordinate invariance that underlies General Relativity. This unification of concepts generated some new dreams of unification and uniqueness, but on the other hand it was clear that the Standard Model did not really look like something mathematically unique.

2.2 The Standard Model

Let us have a closer look at the Standard Model. The only feature that I want to emphasis is the choices involved in its definition. We have already seen that the first choice is a gauge group, which is $SU(3) \times SU(2) \times U(1)$. There is no theoretical justification for this choice. It is just what we observe, albeit rather indirectly and through tremendous experimental efforts. Each factor implies the existence of an interaction, which may be

thought of as a kind of generalization of electrodynamics. Indeed, the first factor is responsible for the theory of the strong interactions, quantum chromodynamics (QCD), whereas the other two combine to yield the weak and electromagnetic interactions.

Next we must specify matter for these interactions to couple to. This is done simply by specifying a spin and a set of charges. The charges, more generally "representations" of the Standard Model gauge group $SU(3) \times SU(2) \times U(1)$, specify how the corresponding particle couples to the three fundamental interactions. All fundamental matter particles observed so far have spin $\frac{1}{2}$. They fall into two classes: those that have QCD interactions (the quarks) and those that do not (the leptons). Mathematically speaking there are infinitely many possible choices for the charges/representations, and just one of them agrees with experimental observation. It is usually written as follows[2]

$$(\mathbf{3}, \mathbf{2}, \frac{1}{6})_L + (\mathbf{3}, \mathbf{1}, \frac{2}{3})_R + (\mathbf{3}, \mathbf{1}, -\frac{1}{3})_R + (\mathbf{1}, \mathbf{2}, -\frac{1}{2})_L + (\mathbf{1}, \mathbf{1}, -1)_R \ , \tag{1}$$

where the three entries indicate the coupling to $SU(3)$, $SU(2)$ and $U(1)$ respectively, and L and R denote left- and right-handed. Although I am showing these representations in detail, I do not expect all readers to understand what the notation means. I only want to emphasize that this is just a choice. Indeed many other choices are possible; one may change all the numbers appearing in (1), subject to a few consistency conditions. These conditions allow an infinity of possibilities, and there is nothing in the formulation of the Standard Model that specifies this particular choice. Furthermore, for unknown reasons, this entire set is repeated three times: the matter particles belong to three "families". Each of these families has identical couplings to the Standard Model interactions; each consists of two quarks and two leptons.

We may add particles of spin 0 (scalar particles), and they may also have all kinds of charges. So far no such particle has been observed, but one is expected: the famous Higgs particle. To play the rôle of the Higgs particle it must have a very definite set of charges, but mathematically there is once again an infinity of possibilities. The gauge group itself implies the existence of particles of spin 1; these are completely fixed and correspond to the photon (responsible for all electromagnetic phenomena), the gluons (particles that bind quarks together to form protons and neutrons) and the W and Z particles (vector bosons with a mass of about eighty times the proton mass that are responsible for weak decays).

There is one final set of ingredients to be specified. The spins and charges of the particles determine precisely how they can couple to each other, up to some numerical constants, the so-called coupling constants. These are real numbers, and any value is in principle allowed. The Standard Model has about[3] 28 of them. Only a few of them really play a rôle in the rest of the story, but for completeness I will list all of them:

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[2]Right-handed neutrinos have been ignored here.

[3]The precise number depends on assumptions regarding the number of right-handed neutrinos and the existence of Majorana masses.The most common set of assumptions (three right-handed neutrinos and the existence of both Dirac and Majorana mass components) gives three masses and six mixing angles for neutrinos, and a total of 28 parameters.

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- Three gauge couplings g_1, g_2 and g_3.

- The nine masses of the quarks u, d, c, s, t, b and the charged leptons e, μ, τ.

- Four weak interaction mixing angles of the quarks.

- The Higgs boson mass parameter μ^2 and the Higgs self-coupling λ.

- The strong interaction CP-violation parameter θ.

- Neutrino mass and leptonic weak interaction mixing parameters.

Apart from the neutrino sector, where a lot is still unknown, the only parameter that has not been measured yet is a combination of μ^2 and λ. We do know the value of the combination $v = 2\sqrt{-\mu^2/\lambda}$, known as the "vacuum expectation value of the Higgs field". It has the dimension of a mass, and in the conventional units of particle physics its value is 246 GeV[4] If and when the Higgs boson is discovered we will know its mass, which is given by $\sqrt{-2\mu^2}$ (note that $\mu^2 < 0$, although the notation might suggest otherwise), and then of course we also know λ.

The quark and charged leptons owe their masses to their coupling to the Higgs bosons. These are called "Yukawa couplings" and are in fact dimensionless 3 x 3 matrices mixing the particles of equal charge in the three families. Not all parameters in these matrices are observable, but their eigenvalues are. Denoting these as λ_x we may write the mass m_x of particle x as $\lambda_x v$.

A combination of g_1 and g_2, $e = g_1 g_2 / \sqrt{g_1^2 + g_2^2}$, yields the famous fine-structure constant $\alpha = \frac{e^2}{4\pi}$, with a value of about $\frac{1}{137.04}$. Most of the remaining parameters are responsible (via the Higgs mechanism) for the masses of the quarks and leptons.

It should be emphasized that the number of parameters of the Standard Model is amazingly small in comparison to its achievements. Most physical systems have an infinite number of parameters (even though in practice only a finite number is relevant). For example any function specifying the shape of objects has an infinite number of parameters. Nevertheless most people think 28 is too many, although few people have expressed themselves very explicitly on their ultimate expectations. One often encounters phrases like the following one from the Wikipedia item on the Standard Model:

However, the Standard Model falls short of being a complete theory of fundamental interactions, primarily because of its lack of inclusion of gravity, the fourth known fundamental interaction, but also because of the large number of numerical parameters (such as masses and coupling constants) that must be put "by hand" into the theory (rather than being derived from first principles).

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[4]I will follow the standard conventions of particle physics and express all energies in electron-Volts (eV). Furthermore all masses and lengths are also converted to eV by inserting appropriate powers of $\hbar$ and c. For comparison: the proton mass is about one GeV (938 MeV) in these units.

No-one would disagree with the first part of that statement.[5] Most people would also agree that it is unacceptable (although not for very concrete reasons) that parameter values must be put in by hand. But it is the last, parenthetical remark that will be especially important: should we expect that their values can be "derived from first principles", and what exactly would that mean?

Feynman said the following about one of the 28 parameters, the fine-structure constant $\alpha \sim 1/137.04$:

There is a most profound and beautiful question associated with the observed coupling constant, e, the amplitude for a real electron to emit or absorb a real photon. It is a simple number that has been experimentally determined [....]. It has been a mystery ever since it was discovered more than fifty years ago, and all good theoretical physicists put this number up on their wall and worry about it. Immediately you would like to know where this number for a coupling comes from: is it related to π or perhaps to the base of natural logarithms? Nobody knows. It's one of the greatest damn mysteries of physics: a magic number that comes to us with no understanding by man. You might say the "hand of God" wrote that number, and "we don't know how He pushed his pencil."

Indeed, some theorists did put the numerical value of $\frac{1}{\alpha}$ on their office door. Others produced surprisingly accurate formulas for this value. A quick search on the internet easily produces a handful (the fact that there is more than one undermines the significance of each of them). There are similar attempts for other quantities, such as the the proton/electron mass ratio, or quark or lepton mass ratio's.

In fact, our current understanding of the Standard Model makes it very unlikely that a parameter like α (as well as the other Standard Model parameters) has a mathematically defined numerical value. We know that all these parameters are in fact functions of energy (see section 2.4). In particle collisions where the incoming particle beams have energies of order 100 GeV, the value of α is about $\frac{1}{128}$. Towards lower energies, the function α would tend to zero, except that its decrease is stopped by the lowest value that its argument can have, the energy corresponding to the electron mass (the lightest charged particle). It would be very surprising if this value could be written as a simple formula in terms of e or π. However, it is still possible that the *function* is itself unique, and that it has a nice mathematical value at some special, presumably high, energy.

2.3 Beyond the Standard Model: Two paradigms

After a breathtaking century of discoveries, to which I did insufficient justice above, our understanding of the fundamental laws of nature has advanced to a remarkable level. We now have a theory of all known non-gravitational interactions that seems almost perfect. This theory is called, not very glamorously, the "Standard Model". One might also call it "The theory of almost everything" [5]. The story of last century concerned the exploration

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[5]Except that there are other ways in which the Standard Model falls short of being complete, most notably the absence of particles that can account for the large amount of dark, non-standard-model matter that appears to be present in our universe.

of matter at ever shorter distances, from atoms to nuclei, protons and neutrons and finally quarks. How will this story continue to unfold in the present century?

There are basically two points of view regarding physics at shorter distances. One is that it is like peeling away the layers of an onion. In this picture new physics should be expected whenever we go up several orders of magnitude in energy, or down several orders of magnitude in distance. This is certainly what happened during last century, and it is *a priori* not unreasonable to expect the story to continue that way. Most ideas in this direction focus on the Higgs boson, because it is the least understood part of the Standard Model, and it is responsible for most of its parameters.

However, during the last decades of last century a different point of view has emerged: perhaps we can peel off the last layer of the onion in one bold step. If that is the case, one would expect this bold step to solve also the longstanding problem of consistently combining General Relativity and quantum mechanics. Assuming that our universe has three spatial dimension at all lengths scales, this puts the energy scale of the new physics at an inconceivably large value of 1.2×10^{19} GeV, the Planck energy[6], corresponding to a length scale of 1.6×10^{-33} cm.

There are several arguments in favor of this point of view. The first one is purely practical (and not very convincing). Looking deeper and deeper into the structure of matter becomes more and more difficult and expensive, and if there are too many onion shells still ahead of us, the "final theory" may elude us forever. There is, of course, no scientific reason why we should be able to find such a theory, assuming it exists. It seems almost ridiculous to believe that we, primitive beings on a speck of dust in the universe, should be able to figure out all the laws of physics with our severely limited equipment and mental abilities. There may be crucial information that we are simply unable to get. However, the structure of the Standard Model itself *does* give us some reason to hope that such a final bold step may be possible. It looks like it is more than simply the next step in a sequence: periodic system, nuclear physics and hadronic physics. I will discuss this in the next section. There is a third reason. More or less by coincidence we have stumbled on a theory with magical properties called "string theory", which looks like a perfect candidate for the center of the onion, the final theory. This will be the subject of the second half of this paper.

When one phrases the problem in this way it seems rather natural to expect the center of the onion to be something unique. One starts dreaming of a "theory of everything" from which all known physics can be derived, at least in principle. Indeed, we have always been able to move backward in the hierarchy of onion shells. For example we can derive the spectrum and interactions of protons and neutrons from quarks and quantum-chromodynamics, the fundamental theory of the strong interactions. We can derive nuclear physics from the strong force mediated by pions (themselves quark-anti-

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[6]The Planck energy is given by $E_{\mathrm{Planck}} = \sqrt{\frac{\hbar c^5}{G_N}}$, where G_N is Newton's constant. For comparison: the most powerful particle accelerator currently under construction, the LHC (CERN, Geneva), will collide protons with each other with energies of a mere 7 TeV. I will not consider the possibility of large but unobservable extra dimensions, which effectively lower the scale of quantum gravity. This does not affect the main points.

quark bound states), atomic physics from the electromagnetic force between nuclei and electrons. In practice we can do these computations only in a few simple cases and with sometimes brutal approximations, but the principle is not in doubt.

The question regarding uniqueness of the Standard Model becomes unavoidable if one adopts the second paradigm. This is not really an issue if one just thinks in terms of moving to the next shell of the onion. But if one is aiming for the "fundamental theory of all interactions", this would be the last chance. There is, by definition, nothing beyond such a theory. Should we really expect that all the parameter values of the Standard Model can be "derived from from first principles", as the Wikipedia text suggests?

2.4 Parameters in Quantum Field Theory

A fundamental feature of Quantum Mechanics is that one must sum over all possibilities, weighted with an exponential factor. Here "all possibilities" includes literally everything, including physics that we know nothing about. It includes the possibility that particles are created and annihilate again, at arbitrarily short time and length scales. These particles may be quarks and leptons, but also any particle we have not yet observed. Even for a finite set of particles there is an infinite set of possibilities: many particles may be created and they may interact and recombine in any possible way. How can we possibly get precise answers out of such a theory?

The reason is that in certain "good" theories, including the Standard Model, all these unknowns can be lumped together in a finite number of parameters. These parameters are not specified by the theory, indeed, they are "infinitely unknown", but that does not matter. We only have to measure each of them once and we can compute any quantity of interest.

One can distinguish three kinds of parameters: dimensionless ones, parameters with a positive mass dimension and parameters with a negative mass dimension. The latter become more and more relevant at high energies, since they contribute to physical quantities with powers of the dimensionless quantity ξE^n, where ξ is a parameter of mass dimension $-n$ and E the energy of the process of interest. In every quantum field theory one can write down an infinity of such parameters, and as soon as one is known to be non-zero, one should expect an infinite number of others to be non-zero as well, because of incalculable quantum corrections that contribute to them. Consequently, for energies larger than the lowest mass scale set by the negative dimension parameters the predictive power of a theory breaks down completely. Positive dimension parameters are irrelevant at sufficiently high energies, but important at low energies. Dimensionless parameters are relevant at all energy scales. A given quantum field theory has a finite number of parameters of positive or zero dimension. The Standard Model has one parameter of positive mass dimension (the mass of the still to be discovered Higgs particle) and the remaining 27 are dimensionless. No parameters of negative mass dimension are known.

The feature that unknown quantum corrections can be lumped together in a finite number of parameters goes by the name of "renormalizability". It is a theoretical fact (for which 't Hooft and Veltman were awarded the Nobel prize in 1999) that the Standard

Model has this feature, but it will always remain an experimental question whether this really corresponds to nature. Indeed, it is possible that the next generation of experiments, and in particular the LHC (Large Hadron Collider, currently in the last phase of construction at CERN, Geneva), finds evidence of new parameters of negative mass dimension. In this case the Standard Model looses its validity at the corresponding energy scale, and the extrapolation towards the Planck scale that is required to make contact with a fundamental theory of quantum gravity is unjustifiable. If such parameter is ever discovered, it would essentially invalidate the argument presented in the next chapter, or at best postpone the discussion by many decades.[7]

There are two important features of renormalizable gauge theories that are important in the present context. The first is that they retain their consistency for a large range of parameter choices other than those that we observe in our universe. The second important feature is the fact that the properties of the theory are independent of the details of short distance physics.

The meaning of "consistency" in the foregoing paragraph is that we can carry out computations of quantum corrections with unlimited precision without running into infinities and without needing new information that was not present in the original definition of the theory. In particular, no new parameters will be needed. This statement is most clearly correct for strong interaction ($SU(3)$) part of the Standard Model, Quantum Chromo Dynamics (QCD). Paradoxically, this is the part of the Standard Model where computations are *least* precise, bur this is due to practical limitations and not matters of principle.

The other two interactions are only slightly more problematic. They are also renormalizable, but even for renormalizable theories there is something else that could go wrong. It turns out that the dimensionless parameters also give rise to energy dependence, although not by powers of energy but only logarithmically. This is known as "running" of coupling constants, although "crawling" would perhaps be a better word. The energy dependence is typically as follows (to lowest order in the expansion in quantum corrections)

$$g^n(E) = \frac{g^n(E_0)}{1 - b_0 \ln(E/E0)} \ . \tag{2}$$

Here g is some coupling, E the energy, E_0 some reference energy scale, an n can be 1 or 2 depending on the kind of coupling considered. The coefficient b_0 is a numerical constant that depends on the particle content of the theory, and that can be straightforwardly computed. The sign of b_0 is important. It turns out to be negative in QCD, and this implies that the coupling becomes smaller at higher energies, eventually approaching zero. This phenomenon is called "asymptotic freedom", and Gross, Wilczek and Politzer received the 2004 Nobel prize for its discovery. If b_0 is positive the corresponding coupling becomes infinite at the energy E_0 if we extrapolate it to higher energy (this is known as a

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[7]This does not mean that any kind of "new physics" at shorter distance scales necessarily undermines the argument. New phenomena that can be consistently extrapolated to the Planck scale themselves, or that only affect the Standard Model in a marginal way, or involve entirely new particles are not relevant. Anything that ruins the extrapolation of gauge couplings or quark and lepton masses would however seriously weaken the argument.

"Landau pole"). In the Standard Model this happens for the coupling of the $U(1)$ factor, and for the self-coupling of the Higgs bosons. The latter can only be determined if and when the Higgs boson is found experimentally. In both cases, the Landau pole limits the precision of the theory to effects of order E/E_0. For the $U(1)$ coupling the pole is far beyond the Planck scale, and for the Higgs self-coupling it depends on the still unknown mass of the Higgs particle: if that mass is less than about 180 GeV the Higgs Landau pole is also beyond the Planck scale.

Obviously this is therefore another issue where LHC results could change the entire picture. The entire justification for assuming that we can go directly to a fundamental theory of all interactions without passing through intermediate "onion shells" would break down if the Higgs boson were too heavy. This would imply that we will encounter a pole in the Higgs self-coupling before reaching the Planck scale, and the existence of that pole would provide strong evidence of an intermediate onion shell.

Because of their Landau poles and the still unknown Higgs sector the other two Standard Model interactions are on a less strong theoretical footing than QCD, but in reality neither of the three parts of the Standard Model can be expected to be infinitely precise. The reason is that they all couple to quantum gravity, which will lead to presently incalculable corrections of order E/E_{Planck}. Quantum gravity is precisely the problem that the has to be solved anyway by the fundamental theory we are looking for, and therefore one might say that from this point of view all three Standard Model interactions are on comparable solid footing.

More importantly, we can write down an infinity of other theories that are on equally solid footing theoretically. This puts us in a unique situation in the history of quantum physics. Nobody would make such a statement about the theory of nuclear physics or hadronic physics. Both of them have plenty of parameters (particle masses, hadronic for factors, nucleon-nucleon potentials etc.) but it would be foolish to suggest that one still obtains a consistent theory for arbitrary changes of these parameters. Even for the parameters we observe in our own universe the concept of high precision quantum computations is senseless in these theories. Hence they contain no clue about how they could possibly be modified. We *can* give a meaning to alternatives to nuclear and hadronic physics now that we know how to obtain these theories from gauge theories. This tells us precisely what we can vary (in particular the quark masses and the strength of the couplings) without affecting their implicit consistency.

This notion of what can be modified in the laws of physics we observe plays an essential rôle in the following. Gauge theory defines a huge set of possibilities which I am tempted to call the "Gauge Theory Landscape", but this would be an inappropriate name for two reasons. First of all it may be confused with the "String Theory Landscape" to be discussed later, and secondly it lacks an essential feature for a true landscape: a height function. So instead I will refer to it as the "Gauge Theory Plane". String theory will provide a height function on that plane to turn it into a landscape.

The fact that thirty-five years ago we seem to have landed in a point of the Gauge Theory Plane that extrapolates consistently to the Planck scale is remarkable, but not a guarantee that this is the last onion-shell before the Planck scale. However, the principles

on which gauge theories are based are so powerful that it seems reasonable to assume that eventually we will have to land somewhere in that plane. Indeed, practically all new ideas of physics "beyond the Standard Model" are based on other points in the Gauge Theory Plane. This also addresses another obvious objection one might have, namely the fact that it only seems to describe less than a quarter of all the matter in the universe. The vast majority of matter in the universe does not interact with photons, and a large fraction of this "Dark Matter" is believed not to consist of the Standard Model particles we know. We are in obvious danger of making another anthropocentric mistake if we assume that the matter we have not observed yet has properties similar to the small percentage of matter that we are made of. Nevertheless, I have enough confidence in the theoretical underpinnings of gauge theory to assume that those interactions are probably eventually described by gauge theories as well.

So if we have to land eventually somewhere in the Gauge Theory Plane, we can only hope that nature is kind to us and puts us directly in the right point, rather than confusing us with a theory that just mimics a well-behaved gauge theory for no apparent reason. This is an important point, because it can be tested experimentally, and we do not have to wait very long for this to happen. Experiments at the LHC (Large Hadron Collider), which is scheduled to start running this year at CERN, Geneva, may force us to conclude that indeed nature has been confusing us, and that there is no well-behaved extrapolation of currently known physics to the Planck scale. Then everything written here is, at best, premature.

3   Two "gedanken" calculations

So is our beloved Standard Model derivable from first principles, *i.e.* from pure mathematics? I was confronted with that question around 1987, when string theory started hinting at an answer. I arrived at a conclusion that I started explaining to my colleagues, scribbling pictures like the one below on napkins in the CERN cafeteria. The gist of the argument was that for most alternatives we would not exist, and neither would anything else of interest, but it is worthwhile to make that a bit more precise. Since string theory will be discussed later, this presentation is not in chronological order. The reason for doing it like this is that I am convinced that someone should have arrived at this conclusion even before string theory forced us to think about it.

3.1   Physics in the Gauge Theory Plane

Consider all theories we can think about theoretically, with different kinds of interactions, quarks and leptons, and arbitrary masses for all these particles, in other words the "Gauge Theory Plane" mentioned above. This is a huge space, with discrete and continuous parameters. The important point is that all of these make as much sense theoretically as the Standard Model. We are going to consider two "gedanken calculations". These are calculations we cannot really do, but that we can at least think about without being

accused of transcending the boundaries of science.

The first computation is the possible existence of observers, or intelligent life, in a universe governed by a given gauge theory. This is a "gedanken" calculation because we are technically unable to do it. We certainly would not be able at present to derive the existence of observers from the fundamental properties and interactions of particles in our own universe. Even the definition of life or observers is a problem. However, we can certainly imagine quantities that are computable in principle and that are almost certainly necessary for the existence of observers.

The definition of such quantities is tricky because of the risk of making precisely the kind of anthropocentric mistakes I warned against in section (1.1). It is the danger of assuming too quickly that what we see in our own environment must be necessary for life in general. Indeed, arguments of this kind tend to be based on assumptions such as the necessity of Carbon, galaxies and stars, and some go as far as assuming the special physical properties of water as essential.

However, it seems reasonable to assume, for example, that some level of complexity of the spectrum is necessary for life, and on the other hand complexity is something that can be defined and computed, at least in principle. Furthermore it seems plausible that the building blocks of this complexity should be sufficiently abundant, at least locally, and that there should be sources of energy available.

So one can at least imagine drawing contours of some appropriately defined "observer function" on the space of all gauge theories. Ideally such a function might specify the average total number of observers that exist in a universe during its entire lifetime, and the contour may be drawn at the value 1. I will insist on working exclusively in the Gauge Theory Plane, because that is where parameter changes are certain to make sense. Any other variables, in particular cosmological ones, may simply be put at their optimal values. If it turns out that those values can never by attained, the contours in the Gauge Theory Plane can only be smaller than we first thought they were. The same is true if we use too liberal a definition of what constitutes an "observer".

Each such contour divides the space into two regions, one where observers might exist, and one where they most definitely do not exist. Both regions are non-empty: it is not hard to construct gauge theories that have completely uninteresting and hence "lifeless" spectra, and obviously the Standard Model provides an example within the contour. The computation would involve nuclear physics and chemistry for gauge theories that are different from ours, with different interactions, coupling to different kinds of particles. It is not doable in practice, but well-defined in principle. The figure below illustrates what such contours might look like in some slice of the Gauge Theory Plane. On the axes one may put some of the Standard Model parameters, such as quark or lepton masses. The small circle indicate the current experimental data. It lies entirely within an observer contour, because our own existence is a piece of experimental data. These contours are not based on real computations (one could in fact do a lot better than this), but are drawn like this to illustrate some important points and address a few common misconceptions that I will discuss later.
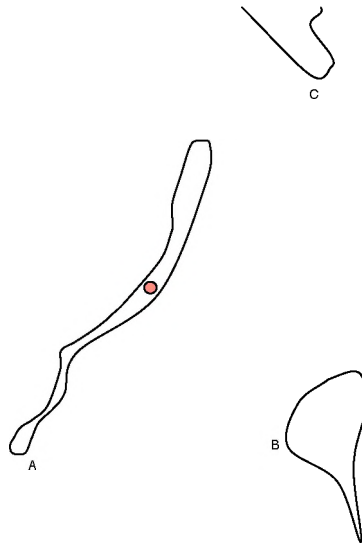
3.2 A unique gauge theory?

Now consider the second gedanken computation. It would be to compute the unique gauge theory resulting from some fundamental theory. This is a gedanken computation because no such theory is known. String theory was once hoped to be such a theory, but does not seem to have this "uniqueness" property. But let us imagine that one day we find some theory that does give a unique answer. Deriving that answer would involve some computation involving properties of new physical phenomena relevant to distances much shorter than what can be observed today, for example the Planck distance.

The problem is now that the unique result of the second computation has to fall within the narrow contour of the first, even though these are completely unrelated computations.

The two gedanken computations are unrelated precisely because gauge theories, as explained above, are insensitive to the physics of very short distances. So even if the second gedanken computation picks only one point as the only theory that is consistent for arbitrarily short distances, this does not invalidate the first gedanken computation. Nobody would doubt that we could repeat all the computations in nuclear and atomic physics and chemistry with different values of the electron and quark masses or the strength of the electromagnetic coupling, and that all the results are correct, even if such a theory would turn out to be inconsistent at distances the size of the Planck length.

There is an obvious way out of this puzzle. There would be no mystery about the special features of our own universe if the fundamental theory had a huge number of "solutions". I am using the world "solution" because I am thinking of some set of equations derived from the hypothetical fundamental theory. Equations can of course have more than on solution, even if the equations themselves are unique. In order to demystify the apparent fine-tunings we see in our environment, the fundamental theory should have, in addition to our own universe, a huge number of other solutions, densely covering the Gauge Theory Plane (or at least a large neighbourhood around us) with points.

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3.3 Our own neighbourhood

Although the complete computation of the contours in fig 1. is far beyond our technical capabilities, part of our own neighbourhood has been mapped out already. There exist several arguments showing that small modifications in some of the parameters of the Standard Model would be fatal for our kind of life and most likely any kind of life. There are many arguments of this kind, see for example [6], [7], [8], [9], [10] and [11]. A rather obvious one is the fact that the proton is slightly lighter than the neutron. If you turn that around, the proton would be unstable against decay to a neutron, a positron and a neutrino. In the Standard Model, we can tune that mass difference by increasing the mass of the up quark (a proton consist of two up quarks and a down quark, a neutron of two down quarks and an up quark; up and down quarks are also denoted as u and d, and have charges $\frac{2}{3}$ and $-\frac{1}{3}$ respectively), keeping everything else fixed. There is a small range in parameter space where a free proton would be unstable, but where it is stable in certain nuclei, just as the neutron in our universe. But if we continue to increase the up quark mass a bit more, we loose all charged stable nuclei.

If we increase the electron mass from its observed value of .511 MeV, keeping everything else fixed, pretty soon it becomes larger than the proton-neutron mass difference of 1.293 MeV. This would make the neutron stable. Then the hydrogen atom becomes unstable against electron capture and we get to a universe dominated by neutrons, with few protons and electrons. Another remarkable fact is the lightness of the up and the down quark. If we increase their masses, the pion become heavier. This in its turn reduces the range of the strong interactions, which fall off exponentially with the a length scale set by the inverse pion mass. Soon we start loosing the stable heavier nuclei, until eventually Carbon is not stable anymore. Similarly, if we increase the electromagnetic fine structure constant α, we increase the repulsion of protons in nuclei, and this would also destabilize heavier nuclei.

Opponents of this kind of argument will say that the burden of proof is on me to show that beyond these thresholds nothing resembling intelligent life could possibly exist. But instead of arguing about the precise position of the contours in the gauge theory plane, one should consider the opposite question: is it plausible that intelligent life is a dominant feature over most of the Gauge Theory Plane? If one looks at the entire picture, rather than zooming in on a particular part of the contour, it seems difficult to argue that life is generic. There may well be life elsewhere in the Gauge Theory plane, equally intrigued about the friendly features of their own environment as we are about ours. But on the other hand, one cannot simply dismiss the entire bulk of evidence in this way.

Scales

The most obvious peculiarity of the Standard Model that seems relevant for the existence of life is its mass scale. In fact, the Standard Model has *two* mass scales that are – to the best of our knowledge – independent. The first one is the QCD scale. It is the mass scale E_0 where the running QCD coupling constant, given by (2) with $b_0 < 0$, has a pole. This scale is a few hundred MeV, and it determines the masses of most hadrons, in

particular the proton. An important exception is the pion, whose mass – unlike that of the proton an neutron – goes to zero with the up and down quark masses: it is proportional to $\sqrt{m_{\mathrm{u}} + m_{\mathrm{d}}}$. The second mass scale we know of is the weak interaction scale $v = 246$ GeV. It determines the masses of the W and Z bosons as well as those of all the quarks and leptons, with the exception of the neutrino masses, which may well require a third mass-scale (this is usually regarded as "beyond the Standard Model physics" and hence not counted among the Standard Model mass scales). Both the QCD and the weak scale are much smaller than the Planck scale, and in both cases this is clearly essential for the existence of observers: If the proton or the electron had Planckian masses, gravity would be much too strong in comparison with the electromagnetic or hadronic and nuclear forces. Most people consider the problem of the smallness of the QCD scale "solved". I will comment on this in appendix C.

Discrete choices

The discrete choices in defining the Standard Model, the gauge groups and representations, are undoubtedly important for the existence of life. This quickly gets us into uncharted territory, because most cases are to complicated to work out. But if a few things are fairly obvious. Removing either the $SU(3)$ factor or the $U(1)$ factor removes the strong or electromagnetic interactions, which both seem essential, not just for our life but for any kind of complexity. The $SU(2)$ factor responsible for the weak force is less essential. Indeed, it has been argued that life could still exist if we remove it altogether [12], provided we make some other changes. However, in such a universe our world would look even more fine-tuned. The quark and lepton masses are dimensionless numbers time a natural scale of about 246 GeV, the weak interaction scale. Removing $SU(2)$ amounts to send the natural mass scale towards the Planck mass which replaces the miracle of a small weak scale by the bigger miracle of small up-quark, down-quark and electron masses. Nevertheless, the computations in [12] are a very nice example of a computation of part of the contours in the Gauge Theory Plane, slightly away from our own place. Although it is argued in [13] that in these weak-less parts of the Gauge Theory Plane we would be starved of oxygen, that argument strikes me as perhaps a little too anthropomorphic. Perhaps other kinds of life could exist in such a universe. If true, it would simply yield another contour in the Gauge Theory Plane.

I have drawn some examples of other contours in the figure. Their existence does not undermine the argument in any way. It does not even matter if there is another contour that looks "larger" than ours, because it does not make it less surprising that a unique solution managed to hit precisely our own, locally narrow, contour.

There are other changes in the discrete choices that have drastic consequences. Changing the number of colors from three to any even number turns the analog of protons and neutrons into bosons, eliminating the rôle of the Pauli exclusion principle in nuclear physics. Changing the choice of representations by choosing instead of Eqn. (1) something else from the myriad of possibilities has consequences that are hard to analyse, but most of them would undoubtedly yield something pretty boring.

Quark and lepton masses

Let us take a closer look at the quark and charged lepton masses. Measuring quark masses is a tricky business, because they cannot be liberated from their bound states. To first approximation the mass of the proton has nothing to do with the quark masses. The proton mass is determined mainly by the intrinsic energy scale of QCD. To observe the actual quark masses we have to probe the proton at high energies, where the quarks start approaching a free particle behaviour. I will use here the official estimate of the "Particle Data Group" [14]. For the lightest quark, the u-quark, they give 1.5 to 3 MeV. The next quark with the same electric charge $\frac{2}{3}$, the c quark, weighs 1.25 GeV, or at least 400 times more. The mass of the next quark of charge $\frac{2}{3}$, the top (t) quark is about 173 GeV, larger by a factor of 138. For the three quarks of charge $-\frac{1}{3}$ these masses are respectively 5 MeV, 95 MeV and 4.5 GeV. For the charged leptons we find .511, 105, and 1800 MeV, with ratios of 200 and 17 respectively.

In the Standard Model all quark and lepton masses are given by the Higgs vacuum expectation value of 246 GeV, multiplied by a dimensionless number, the Yukawa coupling. In the following I will keep the Higgs vacuum expectation value fixed, as well as the strength of the electromagnetic and strong interactions, and vary the Yukawa couplings; see the subsection "determining parameter values" below for a justification.

In terms of their natural units of 246 GeV, the up and down masses as well as the electron mass look ridiculously small; the electron mass is smaller by a factor half a million. Note that the mass-ratios of the charge 2/3 and charge -1/3 quark are inverted in the two heavier families, which makes especially the lightness of the up quark rather peculiar. The lightness of the up-quark is important for at least two reasons: to keep the pion light, and to make the proton lighter than the neutron. The mass difference between the latter is not only determined by the differences of the quark masses that they are made of, but also by electromagnetic effects. Although this is hard to compute, looking at the charges of the quarks it is easy to see that the electromagnetic contribution tends to make the neutron lighter than the proton. The pion mass is proportional to $\sqrt{m_u + m_d}$, whereas the neutron-proton mass difference contains a term $m_d - m_u$ and a negative electromagnetic contribution.

The optimal way to deal with these constraints would be to make the up-quark nearly massless, to make the down quark mass large enough to overcome the electromagnetic contribution, to hope that then the pion is still light enough, and then to choose the electron mass smaller than the neutron proton mass difference. To make this quantitative would require a lot more work[8] because we need to know precisely how heavy we can make the pion without destabilizing too many heavy nuclei. But qualitatively, it is hard to avoid the conclusion that the light quark and lepton masses have been pushed precisely

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[8]There are other bounds one may take into account. For example Hogan [9] takes into account that the conversion of two protons to deuterium, $p + p \rightarrow d + e^+ + \nu_e$ should be exothermic; Weinberg [16] points out that this may not be necessary, because the reaction $p + p + e^- \rightarrow d + \nu_e$ can take its place. It may not be easy to pin down the exact boundary of the inhabitable island in parameter space, but in any case it is true that the parameter values we observe are remarkably close to thresholds beyond which the necessary conditions for life change drastically, and need to be re-examined carefully.

to this corner of parameter space, away from any imaginable natural distribution.

What would such a "natural distribution" look like? If we were to assume that the Yukawa couplings are number of order 1, the amount of tuning required to get the right up-, down-quark and electron masses looks enormous. But the distribution of the other masses does not suggest that they are "randomly" chosen numbers of order 246 GeV. Indeed, there is a clear hierarchy, with masses of quarks and leptons going up by one or two orders of magnitude each time we move to the next family. This hierarchy (the ratios of subsequent quark masses of the same charge) is not very sharply defined, and does not look universal: it is considerably larger for the up quarks than for the down quarks. If we assume that such a hierarchical distribution of Yukawa couplings is fundamental, the amount of tuning is of course reduced. But even taking that into account, It is hard to feel comfortable with the idea that this entire structure, including the hierarchy itself, its non-universality and the large deviations from it, could be the unique solution to some mathematical equation, if our existence depends so crucially on it.

Not all continuous parameters of the Standard Model have serious implications for our existence. The most obvious ones are the up and down quark mass, the electron mass, and the strong and electromagnetic coupling constant. Other parameters may not be as irrelevant as they seem. Consider the top quark mass. The top quark is about 180 times heavier than the proton and has an extremely short life-time. It may nevertheless be important since the large mass implies a large Yukawa coupling, which in its turn implies that it makes important contributions to the running of all other couplings. In particular the Higgs self-coupling is strongly affected, and since all quarks and leptons owe their mass to the Higgs boson, one may expect drastic consequences if we modify the top-quark mass while keeping everything else fixed. Indeed, in the supersymmetric[9] extension of the Standard Model the large top quark coupling drives the symmetry breaking mechanism that produces the quark masses. We do not know enough yet about the Higgs boson to make general statements about the rôle of the top quark, but is remarkable that its mass of about 173 GeV is close to the theoretical upper limit for quark masses, about 230 GeV: if we push the mass beyond that point its Yukawa coupling will have a Landau pole below the Planck mass. Neutrino masses are also relevant for various features of our universe that are important for the existence of life, but that is a much more complicated story.

Secondary effects

The first gedanken computation would include not only the effect of direct variations of the gauge theory parameters, but also the secondary effect these variations have on the parameters of nuclear physics and chemistry. A good example is an excited state of the Carbon nucleus, correctly predicted by Fred Hoyle. He was worried how stars could have produced as much Carbon as we observe. The obvious way to produce Carbon is that

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[9]Supersymmetry is a hypothetical and at best approximate symmetry between fermions and bosons. Because fermions and bosons contribute with opposite signs to quantum corrections, it greatly helps in controlling those corrections. The upcoming LHC experiment may tell us whether it is merely a powerful theoretical tool, or if nature makes use of its power as well.

two α-particles fuse to form Beryllium, which in its turn fuses with another α particle to form Carbon. The trouble is that the first step fails, because Beryllium-8 is not stable. Hoyle then conjectured that there should exist an excited state of Carbon appearing as a resonance in the triple-α channel, so that the normally suppressed process fusing three α-particles to Carbon is strongly enhanced. He urged experimentalists to look for that excited state, and they found it. The energy of the excited Carbon state he conjectured is of course not a variable. But nuclear physics itself varies over the Gauge Theory Plane, mainly as a function of the up and down quark masses and the strength of the QCD coupling. These variations would affect the abundance contour, and end up reducing the size of the region where life can exist. According to Weinberg [16] the Hoyle resonance is not as remarkable as it may seem. He argues that it has the same origin as the instability of Beryllium, and can be understood entirely in terms of unstable bound states of α-particles. If Weinberg is right this would imply that the Hoyle resonance would persist over a large part of the Gauge Theory Plane, and hence does not contribute much to the reduction of the contours. But either way, it can be taken into account.

There are many other secondary effects. In particular [6] is a treasure trove of remarkable facts about nuclear physics and chemistry, relevant for our existence, such as the properties of water (the fact that ice is lighter than water) or the properties of the DNA molecule (supposedly very sensitive to the electron/proton mass ratio, a claim attributed in [6] to T. Regge). Many of these arguments are not terribly convincing since they focus on *our* kind of life instead of observers in general. For example, even if *our* DNA replication fails for a different proton/electron mass ratio, it would still be extremely hard to argue that no life could possibly exist for another value. The essential complexity of chemistry is unchanged if we change this ratio, and there may well exist entirely different molecules playing a similar rôle if the parameter values are different. We should not be too surprised to find that nuclear physics and chemistry are remarkably suitable to *our* kind of life, but that does not exclude the possibility of entirely different kinds of life, presumably equally puzzled about their own environment. This kind of remark is often used to dismiss *any* argument concerning apparent fine-tuning of the laws of physics. But that is not reasonable either. If we look at all the evidence without prejudice, it does look plausible that we live in a privileged place in the Gauge Theory Plane, but there is no reason to assume it is the only one. Indeed, if it were, that would raise another disturbing puzzle.

These secondary effects are a favorite target for critics. After all, we can compute them! We can compute the resonances of carbon, the properties of water, and the properties of DNA, at least in principle. Does this not show that it is senseless to worry about such apparent coincidences in our environment?

Although we can indeed compute them, the result depends on the parameters of the Standard Model, and as such contributes to the shape of the contours. If these secondary arguments turn out to be correct (*i.e.* if they hold for generic observers, rather than just us) the contours might become very small indeed. In some cases there is even a risk of over-determination, in the sense that several unrelated arguments constrain the same set of Standard Model parameters. If we could really do these computations, it seems more

likely to me that the contours would decrease is size in comparison to current guesses, than that they would increase.

Determining parameter values

To which extent do these arguments *determine* or *explain* the values of some of the Standard Model parameters? One has to be very careful in drawing such a conclusion. A common, and valid, criticism of arguments of this kind is that one cannot simply vary a parameter while keeping all others fixed. Consider the example of the electron mass. Can we conclude that it is so small because if it where three times heavier the neutron would be stable? Even in our own neighbourhood, one would first have to examine if there are ways to increase the up and down quark mass difference. The range is limited by the pion mass, which in its turn is limited by the required range of the strong interactions, which compete in nuclei with the electromagnetic interaction. So one would really have to consider the full variation of all these parameters to see if the electron mass can be pushed up significantly. But if we go further away from our own neighbourhood, there are other possibilities. For example in the "weakless universe" proposed in [12] the neutron is stable and the problem of a neutron dominated universe is avoided by having stable charged pions, which produce protons when colliding with neutrons. So then this particular limit on the electron mass disappears altogether.

Obviously the correct way of obtaining a true observer window for a single variable is to project the contours on a single axis (including contours for different discrete choices of groups and representations, as long as one can identify quarks and leptons). In practice, this will be very difficult.

But this is not what I am trying to do. I merely wish to argue that our own contour is remarkably narrow in certain directions. To show that, it suffices to scan it along a few axes, where an axis is defined by keeping all but one variable fixed. As fig 1. suggests, our own region can be narrow, but obviously its projection on the two axes can be large. If our contour is narrow in certain directions (essentially a lower-dimensional sub-manifold), it would indeed by a puzzle if a unique answer from a fundamental theory ended up precisely in that narrow strip, regardless of the fact that the strip might be very elongated in other directions.

3.4 Two questions

The advantage of presenting the arguments in terms of contours in the Gauge Theory Plane is that it cleanly separates the issue into two fundamental questions, one that we may be able to answer, and one for which more information is required.

The first is why non-empty contours exist at all: why is there life *anywhere* in the Gauge Theory Plane? The second question is how a first principle computation could ever end up within the contour, instead of in one of the far more abundant boring places in the Gauge Theory Plane. This is the question addressed here.

I do not have much to say about the first question. The problem becomes more

worrisome if our island turned out to be the only one. If all the arguments based solely on our *own* existence are taken seriously, one might be tempted to conclude that. But such an conclusion would fit perfectly in the series of anthropocentric mistakes listed in the first chapter. In any case, the answer to this question is clearly out of reach. Even in our own universe we have no idea what other forms of life might exist.

The most attractive way out would be if the contours themselves were abundant, so that also in this respect we are not unique. The most attractive way out of *both* questions would be that some form of life is generic in all gauge theories. But that seems very implausible, and would still leave us with the question why it was realized in such a complicated way in our own environment.

3.5   Boundaries and measures

In order to make all the foregoing mathematically precise, one would like to compute the relative size of the region in the Gauge Theory Plane that can support the existence of observers. If we could prove that this is true only in a portion of relative size ϵ, it would require the existence of at least $\frac{1}{\epsilon}$ "solutions" in a fundamental theory in order to demystify our existence. Unfortunately such a computation requires the definition of a boundary and of a measure, and the Gauge Theory Plane is not naturally equipped with such quantities. Indeed, it is of infinite size in many directions, although the requirement that a theory should remain consistent until the Planck scale cuts it down. There is also no natural measure, and this concept becomes even harder to define if we consider the discrete choices. For example, what is the relative weight of $SU(3) \times SU(2) \times U(1)$ with respect to other possible gauge groups?

On the other hand, if we choose any boundary and measure without prior knowledge of the Standard Model, we would undoubtedly find that the window for the existence of observers is small. Mathematically speaking, one can always deform any measure in such a way that the relative size gets arbitrarily close to 1, but only if the location of the observer window is already known.

A fundamental theory with a large number of discrete "solutions" would define a measure for us, namely the continuum limit of the distribution of those solutions. But once the correctness of such a theory has been established, there is no need anymore to argue against uniqueness. On the other hand, if a fundamental theory with a unique solution is found, we will undoubtedly be immensely puzzled how it could ever land precisely within the observer contour, but there is no way to make that puzzlement quantitative. This is not significantly different if we consider our planet or our solar system. Undoubtedly in that case nearly everyone would agree that our understanding of their very fortunate features is greatly enhanced by the fact that we know that there are huge numbers of other possibilities. But there is no way to quantify how fortunate we are without a theory of the formation of solar systems and planets (the analog of the "fundamental theory" above), and even then it is very hard. In the time of Kepler such a theory was not available, so if he had chosen to think of the analog of observer contours on the space of all possible solar systems or planets, he would have faced a similar measure problem.

To illustrate this, assume that some fundamental theory has solutions with a flat distribution between 0 and 1, for all dimensionless parameters and ratios of mass scales with the Planck scale. Under this assumption we can compute how many solutions it must have in order that, statistically, there is one within our contour. From the ratio of the QCD scale over the Planck scale we get 10^{-19}, the weak scale over the Planck scale gives an additional 10^{-17}. The u, d quark masses and the electron mass give a factors of about 10^{-4} to 10^{-5} each, when expressed in terms of their natural scale, the weak scale: $v = 246$ GeV. This does not yet include the value of the fine structure constant or the discrete choices of gauge groups and representations. It seems that we would need at least 10^{50} solutions. However, in at least one case it is not likely that the distribution will be as assumed here: instead of the ratio of the QCD scale over the Planck scale it would perhaps be more natural to use the strength of the QCD coupling constant g_3. The relation between these two is logarithmic, as in (2), with $g = g_3$ and $n = 2$. Clearly, if we assume a flat distribution for g_3 instead of the value of the pole, E_0, the relative size of the observer contour is not reduced by a factor 10^{-19} but by a factor of order 10^{-1}. Note that this argument does not by itself explain the small ratio, nor determine the measure, but it does illustrate the importance of understanding the correct parametrization of the variables (see also Appendix C). There may be other cases where Standard Model variables can be reparametrized in order to blow up the observer contour. The weak scale is a candidate, although no convincing mechanism exists in this case. The quark and lepton masses also do not look as if they originated from a flat distribution. Indeed, as discussed above, they display a clear, though not very precise hierarchy. All of these effects may conspire to make the Standard Model look less rare than 1 in 10^{50}. But on the other hand, the number $\frac{1}{\epsilon}$ is just a lower limit that would statistically give rise to just one point within our own contour. It would be yet another example of a potential anthropocentric mistake to assume that our universe is the only fundamentally allowed one within our contour. Furthermore, if the parameters that are relevant to us can take many values, it is reasonable that all the others can do so as well. It is not easy to scatter just a few hundreds of points over a a multi-dimensional space so that they are nicely spread around our neigbourhood without prior knowledge of its existence. We cannot expect the fundamental theory to produce a distribution that is just barely dense enough and just scattered in the minimal way to meet the requirement that we exist. That would be another serious puzzle, although a slightly less severe one.

3.6   Against uniqueness

One may look at the problem in the following way. We have at our disposal a discretely and continuously infinite set of possible theories, all making as much sense theoretically as any other: the Gauge Theory Plane. In our own universe we observe only one of those. Let us assume the existence of some fundamental theory that selects a number N of points in this infinite set. We would like to know the value of N.

I have presented above arguments indicating that we should expect N to be large. These arguments cannot, and will never be, mathematically rigorous. Even if we were

technically able to do all relevant gauge theory computations, we are still faced with the problem of the precise measure on this space, something that can only be determined once we already know the fundamental theory. So this is the best that can be done: a plausibility argument, not a mathematical proof.

But now let us look at the other side of the issue. What are the arguments for believing that $N = 1$? Well, there aren't any. There are a few empirical arguments in favor of relations among some Standard Model features, but on closer examination (see appendix B) even these do not really point towards uniqueness. In fact, the argument for uniqueness of the Standard Model is not better than Kepler's belief that the ratios of planetary orbits should be computable.

At this point some people will say that they never really believed in uniqueness anyway, but just hope that there will be "very few". But this reveals the true nature of their argument: it is nothing else than wishful thinking. The concept of "very-few-ness", unlike uniqueness, has no mathematical definition. All this says is that it would be nice if we could enumerate all the solutions. Indeed, that would be nice. But there is no reason to believe that it will be true.

Believers in uniqueness might appeal to a hope for some underlying, yet to be discovered principle of nature. Indeed, I admit that I am hoping for a principle of nature that fixes the underlying theory (as opposed to the "solutions" of that theory) uniquely. But as soon as one gives up on absolute uniqueness, there is no way of telling what the total number will be. In our universe we observe 1, and that is the maximum we are able to observe with foreseeable technology. All this says about the total is that it is larger than or equal to 1.

3.7 Entropic selection?

Let us assume that we find a fundamental theory that has a large, discrete set of solutions. Then we end up with some kind of statistical distribution, perhaps peaked in somewhere in the Gauge Theory Plane. There are two obvious ingredients to such distributions: the density of points in a certain region of interest, and the probability of ending up at a particular point. The former can at least in principle be obtained from the underlying fundamental theory. The second contribution depends on details of the model for populating the distribution, and may depend on initial conditions beyond our control. It is not even clear if the notion of probability makes any sense in the context, or how to define it.

But let us assume for the sake of the argument that this can all be defined and computed. Some people may hope that this will restore what seemed to be lost, namely the possibility to derive the Standard Model. Could it be the maximum of the probability distribution?

To me, that would seem to be even less attractive than a unique solution. The computation of the maximum of a statistical distribution is analogous to the second gedanken computation of section 3. It would be an equally incomprehensible mystery why such a computation would end up in the observer contour.

Furthermore, unlike a unique vacuum, this cannot even be called an explanation for

the parameter values we observe. If only one value is allowed by fundamental physics, there is nothing else to discuss. But if an ensemble of values is allowed, some of which are outside the observer window, statistics does not help. If the majority of values is within the observer window, do we find ourselves there because it is more likely, or because no observers can exist outside that window? On the other hand, if the distribution has a huge peak outside the observer window, would that be a reason for concern?

This should not be confused with a different application of statistical arguments, which try to determine our place *within* an observer window. If there is a well-defined notion of probability, we may hope that our universe is the most likely one, within the set of inhabitable ones. In particular, if there is a multitude of observer contours, one may hope that there is no other that is far more densely covered than our own. This would cast doubt on the correctness of the fundamental theory producing such a distribution.

3.8 Discrete or continuous?

In the previous discussion I was assuming that the second gedanken calculation will either yield a unique solution, or else produce a discrete set of solutions. Obviously the demystification works equally well if there are solutions with continuous parameters that cover part of the observer contour. One could even ask if we need a fundamental theory at all? If indeed we need a large ensemble of theories to explain why there exist theory within the contour, perhaps the Standard Model itself, with its 28 parameters, is just what we need. Perhaps we were wrong in regarding the existence of this relative small set of parameters as a problem. Perhaps the "theory of everything" we are looking for is simply the Standard Model, combined with a (hypothetical) theory of Quantum Gravity that is completely insensitive to the Standard Model and its parameters.

The trouble is not only that this is esthetically unappealing, but more importantly it does not allow for the possibility of moving to other points in the parameter space. The Standard Model with its observed value of the fine-structure constant, $\alpha = 1/137.04$, has nothing to do with a similar theory with a different value of α. It is of course imaginable that the parameter values are simply fixed to random values at the initial moment of our universe, but this would be highly unsatisfactory, to say the least.

Therefore the parameters must become, in some way "dynamical". Once the parameters are dynamical, it makes sense to compare theories with different values for parameters like α. In particular one would expect their ground state energies to depend on the parameter values, if we can make sense of them. Ground state energy in quantum field theory is a highly divergent quantity, which furthermore is not physically relevant for Standard Model physics. It starts being important if one includes gravity, because only gravity couples to it. In theories of gravity, the Standard Model contribution to the vacuum energy is part of a long-standing problem, the cosmological constant problem, which looks even more serious if one attempts to quantize gravity. I will say more about this below. If α and other parameters are just constants, one may postpone discussing this issue until it is understood in a consistent theory of quantum gravity, and simply set the ground state energy to zero in the meantime. But if we allow α to vary, we cannot expect to be able

to set it to zero for all values of α. If we assume that ultimately a theory of quantum gravity turns vacuum energies into meaningful quantities, they would seem to produce a kind of potential, and what are called "solutions" above would actually correspond to minima of this potential. These may just be local minima. They need not be absolutely stable with respect to quantum tunneling, as long as they live long enough. This obvious fact has been understood for many years already, but the full implication has only been appreciated in recent years.

Such a potential picture would almost inevitably imply the possibility of fluctuations around the minima. The description of these fluctuations in quantum field theory necessitates the introduction of new fields, which can take constant values in one of the minima without breaking Lorentz invariance. Such fields are called scalar fields. Nothing I have said so far requires the minima to be fully discrete. There might exist elongated valleys along which the parameters could vary. But it is not likely that we find ourselves in one of those elongated valleys, for at least three reasons. First of all for observational reasons: such a valley would imply the existence of massless scalar fields (the mass corresponds to the second derivative of the bottom of the valley), which would mediate a "fifth force" with has not been observed. Secondly, a scalar field is not expected to be massless unless there is a symmetry that requires it; in other words, generically one would not expect a valley connecting physically distinct values of α to be absolutely flat. This would only be possible if there were miraculous cancellations between various quantum contributions to the vacuum energy.[10] Finally, our kind of life would not develop if crucial parameters could vary significantly over space or time.

The previous paragraphs were written with string theory hindsight, and perhaps there are different ways in which an ensemble of special parameter values in the Gauge Theory Plane could be selected by a fundamental theory, but it seems unlikely that any theory would produce a continuum. In chapter (5) I will discuss how precisely such a picture emerges from string theory. It is called the "String Theory Landscape".

3.9   The danger of circularity

In this section I want to compare this kind of arguments for the standard mode parameters with those for the cosmological constant. First I will briefly summarize some basic facts about the latter. For more details see *e.g.* [15].

The Cosmological Constant

The vacuum energy discussed above may be thought of as an additional axis orthogonal to the Gauge Theory Plane. One can extend the contours into this additional parameter dimension, and work out where life is possible. It turns out that, at least in our own neighbourhood, the contour is an incredibly tiny strip around zero. The reason vacuum energy affects the existence of life at all is that gravity couples to it. It enters into

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[10]Such cancellation do occur in supersymmetric theories, but supersymmetry is at best an approximate symmetry in nature.

the Einstein equations for gravity via the cosmological constant Λ, that appears in the following way

$$R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R + \Lambda g_{\mu\nu} = 8\pi G_N T_{\mu\nu} \ , \qquad (3)$$

where G_N is Newton's constant and $T_{\mu\nu}$ the energy momentum tensor of matter. The left-hand side contains the Ricci tensor $R_{\mu\nu}$ and the curvature scalar R, both related to the curvature tensor $R^{\mu\nu\rho\sigma}$. The physical principle behind the precise form of the left-hand side is general coordinate invariance. If one arbitrarily imposes an additional requirement, namely that the left-hand side should be linear in R, one gets the standard form of the Einstein equations, with $\Lambda = 0$. But there is no physical reason for such a constraint, and on general grounds one should therefore expect the left-hand side to contain terms of higher order in R (which are irrelevant in normal circumstances) as well as a term of zeroth order, Λ.

Precisely such a term is generated by the the energy density ρ_Λ of the empty universe, namely $\Lambda = 8\pi G_N \rho_\Lambda$. This is a quantity of no relevance in the absence of gravity, which is why it is legitimate to adjust the zero-point of the energy scale in non-gravitational physics. But gravity sees everything.

The coefficient Λ, if present, has important implications for the fate of the universe. If it is negative, the universe collapses, whereas if it is positive the universe undergoes accelerated expansion. The energy density ρ_Λ has dimension [length]$^{-4}$, which in the standard units of particle physics (with $\hbar = c = 1$) is equivalent to [mass]^4. In a theory of quantum gravity, where the only fundamental mass scale is the Planck mass, its natural value would seem to be of order M_{Planck}^4. More precisely, ρ_Λ receives quantum corrections due to virtual particle creation and annihilation which diverge with the energy of the virtual particles. If we assume that quantum gravity provides a natural cut-off for these divergences, one would indeed expect a result of order M_{Planck}^4. But even if we plead ignorance about the cutoff mechanism and the scale at which they occur, we cannot put that scale lower than the energy scale we have explored experimentally, because then we would have discovered the mechanism already. Known physics, cut off just above the Standard Model scale still contributes about $10^{-56}M_{\text{Planck}}^4$ (see [15], [16] and [17], and references therein for recent discussions of this issue).

Furthermore, as noted above, ρ_Λ receives contributions from any re-adjustment of the zero-point of the energy scale. The most notorious example is the Higgs mechanism. In the standard classical picture, this involves a shift in the value of a scalar field from a local maximum to a local minimum of a potential density. The difference of the values of the potential density at the maximum and the minimum is a contribution to ρ_Λ. This contribution is also of order $10^{-60}M_{\text{Planck}}^4$. The problem is that the observed value is about $+10^{-123}M_{\text{Planck}}^4$. The first way in which this was observed, around 1998, was from the fact that we appear to be accelerating away from distant supernovae.

CC versus SM

It goes without saying that if ρ_Λ were 60 or even 120 order of magnitude larger we would have noticed this long before 1998. In fact, if it were just a few orders of magnitude larger there would have been nobody to notice it. The universe would have collapsed before we even existed (if Λ were negative), or would have been ripped apart in a catastrophic acceleration, if Λ were positive.

These bounds where made more precise by Barrow and Tipler [6] and by Weinberg [18]. The former authors assumed that main sequence stars are essential for our existence, and required that the universe would not collapse before they were formed. This gives a negative lower bound. Weinberg assumed that galaxies (as star factories) are crucial, and from the fact that they cannot form if the universe expands too rapidly obtained a positive upper bound. Both of these bounds have an order of magnitude of about 10^{-120} (the precise factor depends on various assumptions), and leave an allowed region that is considerably smaller on the negative axis then on the positive one.

Although both bounds can be criticized for possibly be too anthropocentric(are stars and galaxies just essential for life in our universe, or in any imaginable universe?), it is quite clear that an observer window exists which is many order of magnitude smaller than the natural value of the cosmological constant. This window is the analog of the first gedanken calculation in section 3. In this case the word "gedanken" can safely be omitted. It is also clear that the amount of tuning required is far larger than what is needed for the Standard Model. For this reason this argument convinces some people more of the need for a large ensemble than the analogous argument for the Standard Model parameters.

But there are two other issues to worry about that look more convincing for the Standard Model case. The first is the risk of circularity. It is important to know that physics continues to make sense if we consider different values for a parameter than those observed in our Universe. In the case of the cosmological constant the problem is that it becomes relevant only if we couple it to gravity, which is precisely the theory we are still struggling to understand. Suppose some alternative theory of gravity does not couple to Λ, or that Λ=0. If that theory were correct, we had no right to consider $\Lambda \neq 0$ in the first place. The observer window becomes meaningless, and only the value $\Lambda = 0$ would be acceptable. This is not the case for observer windows in the Gauge Theory Plane. This is because we can discuss gauge theories at the quantum level without understanding gravity. Let us imagine that some fundamental theory of gravity singled out one particular value of the electron mass or the fine-structure constant, by being consistent or finite only for those particular values. We could still do meaningful chemistry computations with different values different values of those parameters. All computations that produce the observer contours in the Gauge Theory Plane remain valid. Hence the puzzle produced if a unique fundamental set of values landed precisely within the observer contour remains genuine.

Furthermore it would not be equally surprising if a fundamental theory managed to land exactly within the ridiculously small observer contour along the cosmological constant axis, since this contour contains the mathematically special number zero. Of course, this

would require an alternative explanation of the apparently observed non-zero value for Λ, but this possibility will continue to attract those that have strong objections to an explanation in terms of a large ensemble. For these reasons I am more strongly convinced of the need for a large ensemble covering the Gauge Theory Plane than of the need for such an ensemble for the cosmological constant, even if the degree of apparent fine-tuning in the latter case is much larger.

4 The Anthropic Principle

The term "Anthropic Principle" was coined by Brandon Carter in 1973, and for a long time it received limited attention, and mainly from cosmologists and relativists whose aim was to understand certain "large number problems", such as the age of the universe in Planck units.

The basic idea is that we cannot possibly observe any other universe than a universe which allows us to exist. Therefore our observations are necessarily biased by our own existence. Depending on the interpretation one gives this to these phrases this can be turned either into a meaningless tautology or a disturbing form of anthropocentrism. Indeed, initially the anthropic principle was presented as a reaction to the Copernican Principle.

Anthropic principles

There seem to be as many anthropic principles (called WAP, SAP, FAP, etc) as there are letters in the alphabet, but two main classes can be distinguished: one where the existence of humans, intelligent life or observers is considered essential for the existence of physics and/or the cosmos, and one where human beings, from a cosmic point of view, are a minor chemical perturbation, of no special significance, except to ourselves. The first is profoundly anthropocentric, the other just the opposite. Remarkably, many of the early discussions of the "anthropic principle" discuss both of these extreme possibilities as viable alternatives. I will not attempt here to describe the various forms of the anthropic principle in detail because it is often hard to understand what people really mean. Language is an extremely limited tool for expressing ideas. Consider for example the following definition of the "Strong Anthropic Principle" by Barrow and Tipler [6]: "The Universe must have those properties which allow life to develop within it at some stage in its history." What is the meaning of "must"? And what is "*The* Universe"? Our Universe, or any possible Universe? Why would anyone even want to formulate such a "principle"? Reading the context of such statements usually does not help much either. Occasionally, however, one finds a formulation that leaves no room for misinterpretation, such as Barrow and Tipler's "Final Anthropic Principle": "Intelligent information-processing must come into existence in the Universe, and once it comes into existence, it will never die out". Perhaps this should be understood as the extreme limit of anthropocentric thinking: if the universe exists just for us, why would we die out?

When I first started discussing the point of view explained in section (3), around 1987 or 1988, I had a vague awareness about something called the "Anthropic Principle", but because I did not know exactly what people meant by that, I usually avoided that term. I am still not sure that what I am talking about here is what other people call "the anthropic principle". But in any case I hope I made it clear that for me it does *not* mean that human beings have some sort of essential rôle in the cosmos.

Disgust, Denial, Derision

Some of the versions of the anthropic principle may sound preposterous, but so are some of the reactions it has received.

During the many discussions I have had on this subject, I learned that it is against some unwritten rule of particle physics to think like this. Someone even called it "religion", a word some physicists use as the superlative of "disgusting". This strong resistance exists until today. Just last year (2006) a respected colleague vowed to quit his job if the anthropic principle would ever play a rôle, and he was not the first to make such a statement. In Physics Today Nobel prize winner B. Richter refers to people thinking in this way as "creationists" [19]. I could fill an entire section with similar remarks.

What is even more remarkable than the emotional language used in these criticisms is the fact that – in the area of Particle Physics – nearly all of them date from the last few years. Before that time, particle physicist and string theorists tried to ignore the issue altogether. An interesting example is [7] by R. Cahn from 1996, containing a very nice discussion about the contours of our region in the Gauge Theory Plane, pointing out that many aspects are crucial for our existence, all without any mention of the anthropic principle (by then a well-known subject, at least in cosmology), not even to reject it.

It is still not uncommon, even now, to hear an entire talk about the cosmological constant (usually described as one of the deepest mysteries of physics), in which several solutions are suggested that even by the speaker's own admission don't make any sense, without even any mention of the possibility of an anthropic explanation, and when it is mentioned at all it is usually with some sarcasm, as in *"The situation is so desperate that anthropic arguments have been advanced"* [20].

As far as I know, the first particle physics paper openly discussing anthropic arguments for Standard Model parameters is Agrawal et. al. [8] from 1997, almost twenty-five years after Carter's talk, more than fifteen years after Linde [22] advocated it in the context of inflation and more than ten years after Vilenkin [23] mentioned it (still somewhat reluctantly) in relation to and quantum cosmology. In string theory, it took even longer. Although string theory clearly pointed towards an "anthropic ensemble" of solutions since 1985, mentioning this in public was "not done". Even nowadays, one can attend an entire conference on "string phenomenology" without any mention of anthropic arguments, except occasionally in a derogatory way. Remarkably, the aforementioned paper by Agrawal et. al. does not even mention string theory! I know of just two texts on particle physics phenomenology before the year 2000 that contain both the terms "anthropic" and "string theory". The first, a review of string phenomenology by Quevedo from 1996 [26] discusses

it in a footnote as a possibility we may have to face if the quest for uniqueness fails. My own text from 1998 [3] advocates a huge ensemble as the most desirable outcome, but was written in Dutch and not translated until 2006 [27] . If we expand the scope to super-gravity, there is also a paper by Gibbons and Townsend from 1993 [21], which mentions anthropic selection or chance as options in the introduction. If we expand it further to include cosmology, there is a remarkable paper by Andrei Linde from 1986 [24], in which he states that *"...an enormously large number of possible types of compactification which exist e.g. in the theories of superstrings should be considered not as a difficulty but as a virtue of these theories, since it increases the probability of the existence of mini-universes in which life of our type may appear"*. Also a remark on string theory in Vilenkin's paper from 1994 [25] is noteworthy. While string theorists continued to overlook or deny the most striking feature of their own theory, the cosmologists Andrei Linde was clearly the first to get the point.

The attitude of denial is nicely illustrated by the title of [28] from January 2000, the first paper on the preprint archives to confront the issue directly: *"The beginning of the end of the anthropic principle"*. But contrary to what the title may suggest, the authors talk about the possibility of many solutions, and conclude the following: *Then life would actually arise in those minima that were approximately "just so". Thus the "just so" issue is resolved by having a large number of possible vacua in which universes can end up.* That statement is precisely what some people would call the anthropic principle. Another ironic twist of history is that three months later a seminal paper by Bousso and Polchinski [29] appeared, that marked the "beginning of the beginning" of the anthropic principle in string theory.

So what is new?

Much of what there is to say about the "anthropic principle" can already be found in the classic book on the subject by Barrow and Tipler [6]. If nothing had changed since then, my reaction might be similar to those described above. But a few things have changed in an essential way. They include:

- Focus on non-uniqueness as the main principle, instead of considering various philo-sophical options more or less on equal footing. The fact that our observations are necessarily biased by our own existence is of interest only if there could be some-thing else: the essential issue is non-uniqueness of the laws of physics. If the laws of physics are really unique, there is not much left to discuss, as far as I am concerned.

- The Standard Model. Many of the "anthropic coincidences" described in [6] are based on nuclear physics or chemistry, without a clear notion of what can be varied. The Standard Model, viewed as a point in the Gauge Theory Plane changes all that.

- The Cosmological Constant, which by Weinberg's argument is bounded from above, and from observations appears to have a non-zero value.

- String Theory. This offers a concrete example of a fundamental theory that fixes the possible parameter values, and seems to do so in exactly the right way. One could try to discuss such a theory purely hypothetically, as I did in the last section, but I doubt that it would be as convincing if we did not have this explicit example.

An (anti)-anthropic check list

Perhaps it is useful to divide the argument in steps, so that everyone can identify their exact point of disagreement. This is a list of statements in decreasing order of acceptability in the particle physic community:

- The Standard Model may not be the *unique* mathematical solution of any fundamental theory.

- Not all alternative solutions allow observers.

- The total number of solutions should be sufficiently large to make the existence of a solution with observers plausible.

- We live in the most probable universe which allows observers.

Some people already see the first statement as something close to an act of treason against the goals of physics. We are supposed to assume that every number we encounter can be computed from first principles. One does not have to go back very far in history to encounter statements indicating this. For example in Randall's book "Warped passages" from 2005 [30] the author states with obvious disapproval: *Some String Theorists no longer try to find a unique theory.* The phrase suggests that most string theorists *are* still trying, but I am not sure if this is true.

Accepting point one but rejecting point two is an interesting way out of anthropic reasoning, but not a popular one. If all alternatives allow observers anyway, there is not much reason to reject uniqueness. People who accept the first two points have already accepted a form of the anthropic principle. If there exist (at least mathematically) universes different from ours in which no life is possible, it is obvious why we don't live there.

Point four is the most controversial one. It is not clear how probabilities are defined and if we can compute them, and even if we can, our Standard Model is just one sample out of an ensemble, a nightmare scenario for statistics. This is another favorite line of attack against anthropic arguments. However, it is useful to distinguish two frontiers in the current debate: one that concerns point four, and one that concerns point three (or even just one and two). Some people are still hoping for a unique outcome, while others take non-uniqueness for granted, and are marching ahead trying to master probabilities. Even if they fail, it does not bring us back to the unique outcome many people hoped for two decades ago. In this article I am just advocating point three. I do not really care whether that is properly called "the anthropic principle". That is what others called it in discussions (usually as an accusation).

Another way of stating this is to distinguish two kinds of anthropic arguments: anthropic window arguments and anthropic probability arguments. The first just states that parameter values have to lie within certain ranges to allow observers, and the second kind of argument tries to derive our precise position within such a window. The first is non-tautological only in the context of a fundamental theory with an ensemble of parameter values in- and outside the window. In that context it really explains why we find ourselves within that window, whereas otherwise it explains nothing. Once a fundamental theory with such an ensemble is established beyond any doubt, the anthropic principle as defined in point three is only of secondary relevance. It is obviously true that the anthropic principle explains why the size of the orbit of the earth is neither extremely large or extremely small, but this is usually not worth more than a footnote in a treatise on planetary orbits. We have not reached the same level of understanding of a fundamental theory of all interactions as of the theory of planetary orbits. Anthropic arguments tell us something about the properties such a theory should have.

The cosmological constant provides a nice illustration of this point. If the fundamental theory just has one solution, or only solutions within the anthropic window, the anthropic argument explains nothing. If it has many solutions spread in and outside the window, one can indeed claim that the anthropic argument explains why we measure a value inside the window. This fact becomes less miraculous as the density of the solutions increases. If the solutions have a known distribution one can move to point 4, and try to derive *where* we should expect to be within that window.

Note that I am implicitly assuming discrete solutions here, for reasons explained earlier. Apparently some people (in particular the authors of [28]) have reserved the term "anthropic principle" for the continuous case only. Their logic is that if the distribution is discrete, we will eventually be able to determine experimentally which one describes our universe. Indeed, the tiny circle in fig. 1 is monotonically decreasing with time, as long as we continue to do experiments, and eventually will contain only a single point. But does not take anything away from the anthropic argument I am advocating here.

Throwing in the towel?

An often-heard objection to anthropic thinking is that it is like "giving up" or "throwing in the towel". But what are we giving up? These statements are based on the assumption that the ideal outcome would be that everything is derived from an underlying theory. As I tried to argue above, that would be a disastrous result. This is not a battle I would like to pursue.

The objection would be valid if someone just declared that we live in a point in the Gauge Theory plane where life is apparently possible, and that there is nothing left to understand. Perhaps this is what some supporters of anthropic principles have been advocating in the past. But this is not what I a advocating here. The crucial difference is the requirement of finding a fundamental theory that produces the required distribution of solutions in the Gauge Theory Plane. This goal is actually *more* ambitious than many ideas just aiming at partial understanding, in the form of relations between gauge theory

parameters.

Superintellects?

When Fred Hoyle saw his prediction of a resonance in Carbon confirmed he was so impressed that he stated "A common sense interpretation of the facts suggests that a superintellect has monkeyed with physics, as well as with chemistry and biology". After surrendering the solar system and loosing the battle concerning the origin of species, people seeking scientific evidence for their favorite superintellect hold on to anthropic coincidences as their next straw. While it is logically possible that in the course of science we discover that the only possible explanation of some phenomenon is the involvement of some sort of superintellect, it is not what most scientists would aim for, because almost by definition that would be the end of the road towards a deeper understanding.

But the question I am addressing here does not hold much promise for evidence for superintellects. A fundamental theory with a huge number of solutions solves the anthropic coincidence problem without any need to monkey with the laws of physics. After many frustrating debates with physicists, I was delighted to see that biologist Richard Dawkins understood that point immediately [31] (among physicist there are notable exceptions, including Weinberg, Linde and Susskind).

But even if the other side in the debate is right, there is not much left for a superintellect to do. Even though a unique solution creates a deep mystery, and even though the hope for such a solution smells like medieval anthropocentric thinking, a unique solution would be mathematically fully determined, just as the value of π. Even a superintellect cannot change mathematics.

The Multiverse

If the fundamental theory has a huge number of solutions we avoid the anthropic fine-tuning problem in the mathematical sense. It would not be surprising anymore that the two gedanken computations of the previous section overlap. Our universe would be based on one of those solutions. The others would just be possibilities that are not realized in our universe.

But could the others be realized in other universes? Whatever that statement might mean, something like it is an almost inevitable consequence of the foregoing. If our universe were unique (as opposed to its laws of physics being unique), we would be faced with another puzzle: how was our particular set of laws of physics selected from the huge set of possibilities? Here there would still appear to be a potential for involvement of superintellects.

But there is an obvious way out: that our universe is *not* unique, but it is created multiple times by some process that samples the set of possibilities. I have always taken something like that for granted, without focusing on a specific mechanism. The whole idea of non-uniqueness of the laws of physics just does not make any sense without it.

Fortunately other people *did* focus on specific mechanism, which led them to the same conclusion from a different direction. The concept of a "multiverse" goes back a long

time, and originated from a variety of ideas in cosmology. In the early eighties, the idea of cosmic inflation was proposed, and naturally led to the possibility of separate patches of space-time blowing up into distinct universes, a notion that was especially emphasized by Andrei Linde and Alexander Vilenkin. This leads to a concept known as "eternal inflation". Linde and Vilenkin concluded that such a multiverse would offer a scientifically acceptable realization of the "anthropic principle".

The multiverse idea is close to, but not exactly the same as the main topic of this paper. One could have a multiverse consisting of universes with identical fundamental laws of physics (gauge theory choices), and one could even imagine a single universe where the laws of physics were chosen randomly from a set of mathematically allowed possibilities. But clearly the last option would not suit my goal, as it replaces a mathematical coincidence (the matching of the two gedanken computations) by a statistical miracle. Obviously, both ingredients are essential: the multiverse and eternal inflation on the one hand and a plethora of solutions of a fundamental theory on the other hand. Cosmology and inflation will not be discussed here in detail since I wish to focus on two other ingredients whose importance has been somewhat underrated: the Standard Model and string theory. For more information and references I refer the reader to [32].

The multiverse raises some obvious questions. Do these other universes "exist"? Can we observe them, even in principle? If not, have we left the boundaries of science? For me, a sufficient answer would be that they exist as solutions to some fundamental theory that also contains our own universe as a solution. Our task is to gather evidence for the correctness of that fundamental theory from the only universe we are likely to have access to: our own. This evidence may be experimental, but it can also be based on consistency of the theory itself. The best one can hope for is that this fundamental theory makes a prediction about our own universe that is so convincing that we also have confidence in the extrapolation to other universes. But it is logically possible that our own universe does not contain enough information to crack this problem. We may be left with a huge number of unrelated candidates for a fundamental theory, each (of course!) with a huge number of solutions, without being able to distinguish between these fundamental theories. Fortunately, at this moment there is only one candidate.

5   String Theory

Even a basic introduction into string theory would be too long for this article, and there are many available elsewhere. I will limit myself to a few points that are relevant in this context. Let me begin by stating the most important unsolved problem.

We are used to specifying a physical theory by writing down, for example a Hamiltonian. In most problems of interest in physics this Hamiltonian is too complicated to deal with. Then we may resort to perturbation theory. We write the Hamiltonian H as $H = H_0 + \epsilon H_1$, where H_0 is a manageable part that gives the dominant contribution, and ϵ is small. Then we expand in ϵ.

In string theory we are in the bizarre situation that we know the perturbative expan-

sion, but we do not know what H is. That this can happen is easy to understand, since there are functions that do not have a Taylor expansion, for example $\exp\left(\frac{1}{\epsilon}\right)$. Such a contribution can never be detected perturbatively.

String theory got its name because H_0 describes the propagation through space-time of string-like objects, and H_1 describes splitting of such objects into other strings, or the joining of two strings into one string.

Quantum mechanics imposes severe restrictions on the space-times in which strings can propagate consistently. The simplest theories can only exist in ten dimensions. There are in fact five such theories, with different H_0 and H_1, apparently defining five distinct fundamental Hamiltonians H. However, there are reasons to believe that in fact all five are different perturbative expansions of the *same* theory. This is expected because we know that all five ten-dimensional are closely related. These relations typically involve replacing one or more expansion parameters ϵ by its inverse $\frac{1}{\epsilon}$. Such relations are known as *dualities* and caused a great deal of excitement around 1995, because they pointed towards a possible uniqueness: instead of five theories there seemed to be just one, with different perturbative expansions. Furthermore, to most people's surprise, there turned out to be a sixth perturbative expansion of H that is not described by string propagation at all, and that corresponds to a theory in 11 dimensions.

The proper definition of string theory would be to specify H. At present, nobody knows how to do that, or even if it is possible. This is not a new problem and also not one that string theorists have overlooked or tried to hide. Indeed, the problem is mentioned prominently on page 27 of the classic string theory book from 1987 by Green, Schwarz and Witten [33], where it appears as a big question mark in a figure. The fact that twenty years later this question mark is still there is worrisome. At this point one may see this either as a challenge or a fatal flaw. It may mean that some of the ingredients we are using are just wrong: this is undoubtedly true for the concept of a Hamiltonian which I used for simplicity above, but may also be true for the idea of an action, quantum mechanics, the continuity of space-time or the very concept of space-time or. Indeed, the fact that in different limits it gives expansions with distinct space-time dimensions may give a hint: it is not even clear in how many space-time dimensions H should be formulated. In fact, many people think that the definition of H should not involve space-time at all, and the space and time are "emergent" concepts, in a sense that remains to be made precise. Hence the name "string theory" is misleading in two respects: it is not yet a theory, and it is only described by propagating strings in some special limits. Sometimes one uses the name "M-theory" for this entire connected ensemble, but I will simple continue to call it string theory despite these caveats.

Every statement we make about this would-be theory is based on results in one of the perturbative regions, with additional insights from some non-perturbative effects that can be taken into account, and symmetries such as duality and supersymmetry. Attempts to make these insights mathematically rigorous tend to be hampered by the fact that at some point full control of all non-perturbative effects is required. However, what we have learned by studying string theory using the limited tools at our disposal is amazing. The spin-offs alone (various results in mathematics and quantum field theory, and especially the

so-called AdS/CFT correspondence) have made the effort worthwhile so far. Remarkable results in quantum gravity, in particular an understanding – in certain cases – of the microscopic origin of black hole entropy suggest that something is right about string theory.

Even without explicitly knowing H, we can deduce which answers the theory will give to fundamental questions, such as the one that is the main topic of this article, the uniqueness of the laws of physics formulated in terms of gauge theories. The answers it gives to questions like this one will tell us whether it is worthwhile to try and unveil the mysteries hidden in H. Two decades ago that was clearly true, and the pursuit of this goal has led to a bounty of beautiful results that all by themselves made the effort worthwhile. Let us see what it says about the central theme of this article, and decide if the answer is sufficiently encouraging to continue.

5.1   Quantum Gravity and Particle Physics

String theory may – and must – provide the solution to many outstanding problems, but there is one problem that it addresses directly: Quantum Gravity. Basically, the goal is to put Quantum Gravity on equal footing with the other three interactions we know: to turn it into an unlimited precision theory. When one tries to quantize gravity naively, one is confronted with exactly the same problem we encountered in the quantization of the Standard Model: quantum contributions that depend on unknown short distance physics. The difference is that in the case of quantum gravity these unknown contributions do not "decouple": they are proportional to powers of E/M_{Planck}, and since M_{Planck} itself is expressed in terms of Newton's constant, one cannot send it to infinity if one discusses gravity. In the case of the Standard Model, such contributions, should they exist, are proportional to E/M_X, where M_X is an unknown scale which is not linked to the parameters of the Standard Model itself.[11] Hence we can, at least formally, consider the limit $M_X \to \infty$. At present, we do not know of any such corrections to the Standard Model, but if we couple it to gravity, corrections proportional to powers of E/M_{Planck} may be expected to the Standard Model parameters as well. To make matters worse, there are infinitely many independent corrections of this kind.

One could consider various options. Perhaps some principle of nature prohibits unlimited precision, in the spirit of the uncertainty principle. Or perhaps there simple exists an infinity of parameters, and the goal of unlimited precision can only be reached if we measure all of them (for practical purposes a finite number would suffice to reach some definite degree of precision). In the latter case, perhaps some unknown new theory at a scale beyond the Planck scale fixes these parameters. It is hard to dismiss these alternatives rigorously, but it is clear that they are less attractive than the obvious one: that all of these problems are caused by naive quantization of gravity, and that they should be solved by a correct theory of quantum gravity. Since we already achieved the goal

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[11]The only exception are the Landau poles described in section (2.4), which however are beyond the Planck scale, if the Higgs boson is not too heavy.

of unlimited precision for three of the four forces we know, it does not seem *a priori* unreasonable to expect the fourth to be manageable as well.

To achieve the goal of unlimited precision, we need to control these corrections. However, controlling these corrections is not merely a problem limited to gravitational interactions. All other particles and interactions inevitable couple to gravity, and contribute quantum corrections of the same order of magnitude as purely gravitational contributions. This insight predates the new string era, which started in 1984. Very illustrative is the following quote of Howard Georgi, which appeared on Jacques Distler's blog "musings" [34], and dates from 1982 or 1983:

"There's no decoupling limit in which it is sensible to consider quantum gravity effects, while neglecting other interactions. Unless you know particle physics all the way up to the Planck scale, you can never hope to say anything predictive about quantum gravity".

One might hope that this can be turned around, and that when we understand quantum gravity, we will know particle physics all the way up to the Planck scale. Perhaps a theory of quantum gravity will then uniquely determine the Standard Model?

Let us now see what string theory has to say about these matters. First of all, what we learn from the perturbative expansions described in the previous subsection suggests that the quantum corrections are indeed under control. Infinite (and hence indeterminable) integrals in quantum gravity perturbation theory turn into finite integrals yielding definite numbers. Furthermore the reason why this happens is precisely the one stated above: in string theory we know all particle physics all the way up to the Planck scale. The infinities of quantum field theory and naive quantum gravity are identified as being due to an infinite over-counting of a finite contribution. In the best understood case, theories of closed strings, this is related to a property known as "modular invariance". At lowest order, the relevant integral (an expression for the simplest quantity that displays the problem) takes the form (a few irrelevant details have been ignored here)

$$\int \frac{d^2\tau}{(\text{Im }\tau)^2} (\text{Im }\tau)^{(2-D)/2} \text{ Tr } e^{2i\pi\tau H_0} , \tag{4}$$

where D is the number of space-time dimensions and H_0 the perturbative Hamiltonian. The trace is over all states in the spectrum of H_0. The integral in quantum field theory would be over the entire complex upper half plane, and is clearly divergent near $\tau = 0$. But in string theory some contributions to this integral can be shown to be identical copies of each other, and we they would be over-counted if we were to integrate over the entire upper half plane. These identical copies are related by the following transformation

$$\tau \to \frac{a\tau + b}{c\tau + d}, \quad a, b, c, d \in \mathbf{Z}, \quad ad - bc = 1. \tag{5}$$

It is an amusing exercise to work out how this divides the complex upper half plane into an infinite number of copies. For this to be relevant the integrand must be invariant under this transformation, which implies strong constraints on the spectrum of H_0. These constraints are known as "modular invariance". To avoid the over-counting we can then limit ourselves to one region, and in particular we may choose one that excludes $\tau = 0$,

thereby explicitly avoiding the field theory divergence. The spectrum of string theory consists of an infinite "tower" of excited states, corresponding to quantized energy levels of the various modes of the string. Any change in the spectrum of such a tower destroys the crucial property of modular invariance.

5.2   Non-Uniqueness in String Theory

It is understandable that this rigidity of the spectrum fueled the hope that string theory might lead us to a unique gauge theory, and perhaps a completely unambiguous derivation of the Standard Model from first principles. This hope is very well described by the following paragraph from the book "The Problems of Physics" by A.J. Legget, which dates from 1987 [35].[12] The author is not a string theorist (he received the Nobel Prize in 2003 for his work on superfluidity) but echoes very accurately the atmosphere in part of the string community around that time:

The hope is that the constraints imposed on such theories solely by the need for mathematical consistency are so strong that they essentially determine a single possible theory uniquely, and that by working out the consequences of the theory in detail one might eventually be able to show that there must be particles with precisely the masses, interactions, and so on, of the known elementary particles: in other words, that the world we live in is the only possible one.

If this had been true, this would have led us to straight to the anthropic dilemma explained in section (3). So how does string theory avoid this?

The answer to that question emerged during two periods of revolutionary change in our understanding, one occurring around 1986, and the the other during the first years of this century. I will refer to these periods as the first and second string vacuum revolution. Although string theorists love revolutions, these two are usually not on their list.

It is important to distinguish two concepts of uniqueness: uniqueness of the theory itself, or uniqueness of its "ground states" or "vacua". I will use these notions in a loose sense here, because one of the issues under dispute is even how they are defined (which is especially problematic in a universe with a positive cosmological constant, as ours seems to have). By "vacuum" I will simply mean anything that is suitable to describe our universe, and anything that merely differs from it by being located in a different point in the Gauge Theory Plane. I am not trying to argue that such vacua exist, but merely that if they do exist there are likely to exist in huge quantities. The picture that seems to emerge is that of a perhaps unique theory, but with a huge number of vacua. Although this picture has started emerging more than twenty years ago, most people refused to accept it as the final outcome, and instead were (and in surprisingly many cases still are) hoping that one of the many candidate vacua would be singled out by some still to be

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[12]This book also contains a remarkably prescient description of what might be called an "anthropic landscape", even with references to an important rôle for higher-dimensional theories, a notion that also appeared in equally prescient work by Andrei Sakharov from 1984 [36] about a possible anthropic solution to the cosmological constant problem. However, precisely because of the cited text about string theory, this remained an overlooked link in the idea for more than a decade.

discovered mathematical principle.

The first string vacuum revolution

String theory was originally discovered in 1969 in an attempt to understand certain features of the strong interactions. Around 1975 it was realized that it always contains graviton-like particles and hence was an interesting candidate for a theory of gravity. It has been under serious consideration as a theory of *all* interactions since 1984, when a new kind of string theory (called the "heterotic string") was discovered. This became the arena for the first string vacuum revolution.

Heterotic strings are fundamentally ten-dimensional theories, and six of the ten dimensions must be "compactified" in order to make them macroscopically unobservable. A rather trivial way to achieve that is to roll them up almost literally on a six-dimensional torus. This leads to a set of four-dimensional string theories with continuous parameters that specify the shape and size of the torus. Note that already at this point the uniqueness of the outcome is in doubt. Clearly this construction would work for any number of space-time dimensions less than ten, and furthermore it leaves us with a set of undetermined continuous parameters. It is hard to dismiss torus compactifications on theoretical grounds, but at least they are ruled out for phenomenological reasons.

These theories *do* contain matter and gauge interactions in four dimensions, in addition to Einstein gravity, but they suffer from a fatal flaw that makes them unsuitable for describing the Standard Model interactions that we observe: they are unable to produce different couplings for left- and right handed particles, a feature known as "chirality", and which is experimentally observed in the Weak interactions. This may seem like a minor point in view of many other unsolved problems, but it is in fact a crucial feature of the Standard Model. Indeed, progress in string theory was severely hampered until 1984, precisely because of the problem of obtaining chiral interactions. This problem could be solved by using instead of a torus a special kind of six-dimensional manifold called a "Calabi-Yau manifold". In 1984 this subject had barely been studied by mathematicians, and hence the preprint version of the first paper [37] using them could state with confidence that "very few are known". This remark was removed in the published version of the paper, because by then it was clear that there were many possibilities.

Others tried to construct Heterotic Strings directly in four dimensions, by modifying the ten-dimensional construction (this is now believed to be equivalent to space-time compactification). One of the first papers following this strategy was by Narain in 1985 [38]. He found in fact a continuous infinity of solutions, a result that was initially received with scepticism by some people, but was soon realized to be absolutely correct. Most people comforted themselves with the fact that Narain's theories were unsuitable for describing the observed Standard Model interactions for a rather basic reason: they could be identified as generalized torus compactifications, suffering the same chirality problem mentioned above.

In subsequent years this explosion of possibilities continued. In particular in 1986 Andy Strominger [39] studied a generalization called "Calabi-Yau manifolds with torsion", and

he found so many possibilities that he remarked in desperation that "all predictive power seems to have been lost". The last phrase of his paper reflects the attitude at that time: "All this points to the overwhelming need to find a dynamical principle for determining the ground state, which now appears more imperative than ever". The need for this elusive dynamical principle was mentioned in many papers in subsequent years.

Other papers in 1986 generalized the direct construction of String Theories in four dimensions further, to overcome the lack of chiral interactions of Narain's theories. These approaches also suggested huge numbers of solutions, which could not be dismissed so easily anymore. The authors of these papers commented on their results in interestingly different ways, but they essentially all took a phenomenological attitude: there are many possibilities, but it is sufficient if just one of them matches the experimental data. The authors of [40] conclude with "We believe [...] a complete classification is a tractable problem and that the relevance of string theory to nature can be tested". In [41] one finds the statement "The number of consistent four-dimensional string theories is so huge that classifying them all would be both impractical and not very illuminating". At CERN I was involved, with Wolfgang Lerche and Dieter Lüst, in another paper [4] from that year, in which we made an attempt to quantify the meaning of "huge". We quoted the number 10^{1500} as a precisely defined, but not saturated upper bound on the kind of theories we were constructing. We also stated "Even if all that string theory could achieve would be a completely finite theory of all interactions including gravity [...] it would be a considerable success".

By that time everyone understood that at the level of approximation of the time, there were going to be huge numbers of solutions, which could not be dismissed straightforwardly. The first vacuum revolution continued for many more years after 1986, and was not limited to Heterotic strings. This just made the number of possibilities even larger.

However, there were (and still are) plenty of unsolved problems, and at least two of those are relevant to the problem of defining the ground state. The first is that most results relied on a symmetry between fermions and bosons that is not observed in nature: supersymmetry. Supersymmetry leads to a cancellation of quantum corrections between fermions and bosons, and makes certain computations possible (well-defined and finite) that are otherwise undoable. It is a great computational tool, but it is still not clear whether it is a symmetry humans need in order to deal with quantum field theory, or whether nature itself makes use of it, at least approximately. A popular idea, testable at the LHC, is that it will emerge as an approximate symmetry, valid for energy scales larger than about a TeV. Whether that is true or not, in any case the supersymmetric solutions are at best approximately relevant for our universe.

The second problem is the so-called moduli problem. Just as torus compactifications, Calabi-Yau compactifications turn out to have continuous parameters called "moduli". The same is true for all the other four-dimensional string constructions, although the geometric interpretation of their moduli may be more complicated.

Essentially, what the first string vacuum revolution brought us was a huge (but still incomplete) list of topologically distinct supersymmetric moduli spaces: four-dimensional string theories, that are not connected to each other by continuous changes of their mod-

uli. The topological distinctions arise from various discrete choices one can make when construction a four-dimensional string theory: the choice of the Calabi-Yau manifold, and a variety of fields that wrap non-contractible cycles on such a manifold. These choices give rise to distinct gauge theories in four dimensions: distinct gauge groups, coupling to distinct sets of particles. These particles usually come out organized in "families" occurring with a topologically determined multiplicity. Each of these topologically distinct choices comes with a certain number of moduli.

The number of such topologically distinct moduli spaces is huge indeed. We do not know how many, but just to get an idea, let us consider Calabi-Yau manifolds. The topology of these spaces is characterized in part by two integers called Hodge numbers. An extensive list of Calabi-Yau spaces is available [42], containing 30.000 distinct pairs of Hodge numbers. Since spaces with identical Hodge numbers may still be topologically different, the actual number is much larger. If we consider more general topological possibilities, it becomes much larger still.

On top of these topological choices one gets the continuous parameters, the moduli. A typical four-dimensional string vacuum may have hundreds of them. The physical parameters of the resulting four-dimensional theory depend on these moduli. This includes in particular the strength of the gauge couplings and the Yukawa coupling parameters, and hence ultimately the masses of all quarks and leptons, in those cases where quarks and leptons can be identified.

Dynamical Parameters

In the supersymmetric approximation all values the moduli can take are equally good. We might be able to get the Standard Model, but essentially have no theoretical control over any of its parameters. They are functions of the moduli.

This may seem to disagree with the expectation expressed in section (5.1) that Quantum Gravity would be restrictive enough to fix the entire spectrum. But there is no disagreement. The way out is that the spectrum consists of infinite "towers" of particles, which themselves are rigid. All that can be changed is a *finite* number of parameters that determine the *infinite* number of coupling constants of the particles within the towers. In other words, the set of variables is of measure zero in the space of all possible couplings of all particles in the string spectrum. It appears that the infinite number of excitations of the string spectrum is an essential feature to satisfy on the one hand the rigidity of Quantum Gravity, and allow on the other hand enough variability to end up with an interesting gauge theory. At the moment, however, this is merely an observation about string theory, and not a general theorem about Quantum Gravity.

If the moduli were merely parameters, we would now be facing the problem mentioned in section (3.8): how can we move around in parameter space, so that we can end up in different points in the parameter space when new universes get started?

Fortunately string theory provides a natural solution to this problem: all parameters are dynamical. In string theory all parameters are functions of scalar fields, that obey field equations. Classically, a scalar field is just a functions of space and time without

a preferred direction, and whose dynamics is governed by a field equation. There is such a scalar field for each modulus. Since scalar fields do not have preferred space-time directions, they can take constant values in space-time patches without violating Lorentz invariance locally. The classical values of these fields determine the values of the gauge theory parameters in each patch. These classical values correspond to vacuum expectation values of the scalar fields in a quantum theory.

This feature has often been heralded as one of the greatest triumphs of string theory: "string theory has no free parameters" was a popular slogan. However, what used to be parameters had now become vacuum expectation values of scalar fields. Nowadays, one sometimes hears the objection that the anthropic principle is turning physics into an environmental science: some parameters are claimed to be a feature of our environment, and not derived from first principles. But how different is "environmental variable" from "vacuum expectation value"? Turning the Standard Model parameters into functions of scalar fields opens the door for the possibility that they may be different in different parts of the universe/multiverse, and hence makes it immediately a lot more plausible that their values depend on the observer (or the presence of observers).

The second string vacuum revolution

If the parameters of the Standard Model are dynamical some obvious questions are: how do they get fixed, and why would they remain constant over large ranges of space and time? Indeed, in the supersymmetric limit one can change their values without any cost in energy. In fact, this energy is always exactly zero (in the absence of gravity). But this turns out to be an artifact of supersymmetry. This causes an exact cancellation between bosonic and fermionic contributions to the vacuum energy which leads to a rather counter-intuitive result: physically distinct systems can have identical vacuum energies. As soon as supersymmetry is broken this degeneracy is lifted, and the result is as one would expect: any change in the values of one of the parameters of the gauge theory leads to a change of the vacuum energy.

If we take into account the values of these energy densities, we get something like a potential for the values of the scalar fields, the moduli. This is a function of tens or hundreds of such moduli. If we plot the energy density as a function of the values of the scalar fields, one may hope to get a curve with some minima. To obtain a more or less stable universe, one would have to end up in one of the minima. Such a universe may not be exactly stable: it may decay by quantum tunneling. But to fit the experimental data of our universe it just has to live at least 13.7×10^9 years.

However, it is by no means guaranteed that a minimum exists. What may happen (and indeed does often happen in simple models) is there are directions in moduli space where the potential just gradually decreases, reaching its true minimum at infinity. In this situation the moduli run away to physically unacceptable values. This is known as the "moduli stabilization problem".

We encounter an even more serious problem if we couple the system to gravity. Then not only the existence of minimum is relevant, but also the value of the potential at the

minimum. This is the famous cosmological constant, discussed earlier.

In a given string theory vacuum the cosmological constant is a finite and calculable number. But since the natural scale of string theory is the Planck scale, the result of such a calculation is some dimensionless constant x times M_{Planck}^4. Somehow this dimensionless number x has to get a value of about 10^{-120}.

It is impossible to say how many non-supersymmetric vacua string theory could have been expected to have, based on the information available before the end of last century, but I think most people would have put this number between 1 and 10^{20} or so, had they been forced to mention a number. If that had been correct, it seems extremely improbable that a value $x = 10^{-120}$ could come out.

Indeed, most people took the attitude that presumably x was exactly zero even in non-supersymmetric string theories, due to some mechanism that was still to be discovered. If such a mechanism had been found, the observation of a non-vanishing x would have falsified string theory. But even with a distribution of 10^{20} non-zero values x, string theory would have been falsified at an enormous confidence level, simply because the distribution is not dense enough.

But a discovery by Bousso and Polchinski [29] in the year 2000 made string theory survive this potential falsification. They identified a new topological feature that has the potential to solve this problem at the expensive of an explosive growth in the number of string vacua.

The Bousso-Polchinski mechanism

We have seen that scalar fields can take constant values without breaking Lorentz or translation invariance. Electric and magnetic fields cannot do that, because they must point in a certain direction. But there is one other kind of field that can also play this rôle. Electromagnetic fields are given by field strength tensors $F_{\mu\nu}$, which are anti-symmetric in μ and ν. Such tensors have natural generalizations to tensors with an arbitrary number of anti-symmetrized indices. The one of interest here[13] is the one with the maximum number of indices in four space-time dimensions, $F_{\mu\nu\rho\sigma}$. The reason we do not usually hear much about such fields is that they are non-dynamical in four dimensions; they do not have any propagating modes, and hence there are no "photons" associated with them. In higher dimensions such fields are conceptually on equal footing with electromagnetic fields, and they do occur abundantly in string theory. They appear as field strengths of three index anti-symmetric tensor fields $A_{\mu\nu\rho}$, obvious generalizations of the electromagnetic vector potential A_μ, and the possible existence of fields of this kind is limited only by the number of space-time dimensions.

Such four-index fields *can* get constant values without breaking Lorentz invariance, namely $F_{\mu\nu\rho\sigma} = c\epsilon_{\mu\nu\rho\sigma}$, where $\epsilon_{\mu\nu\rho\sigma}$ is the Lorentz-invariant completely anti-symmetric four-index tensor; it is unique up to normalization, which is fixed in the standard way

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[13]The possible rôle of rank-4 field strength tensors in understanding the cosmological constant was already discussed by several authors around 1980. See [29] for further references, and also the last page of [45] for a very early discussion including another main ingredient, the anthropic principle.

as $\epsilon_{\mu\nu\rho\sigma}\epsilon^{\mu\nu\rho\sigma} = -24$. The presence of such a classical field strength in our universe is unobservable unless we couple the theory to gravity. If we do, it turns out to gives a contribution similar to the cosmological constant Λ, in such a way that the latter is replaced by $\Lambda_{\text{phys}} = \Lambda - \frac{1}{48}F_{\mu\nu\rho\sigma}F^{\mu\nu\rho\sigma} = \Lambda + \frac{1}{2}c^2$.

One might think that this solves the problem, since we can now choose c in order to tune Λ_{phys} to any desirable value (provided Λ is negative, which it always is in a supersymmetric theory coupled to gravity). However, it turns out that in string theory c is not an arbitrary real number: it is quantized. So the formula for the cosmological constant now looks something like this

$$\Lambda_{\text{phys}} = \Lambda + \frac{1}{2}n^2 f^2 \ , \tag{6}$$

where f is some number derived from the string theory under consideration. If instead of $F_{\mu\nu\rho\sigma}$ we were to consider an electromagnetic field, the quantization of c is akin the charge quantization, and f would be something like the strength of the electromagnetic coupling e: some number of order 1. This looks like bad news. For generic negative values of Λ we would only be able to tune Λ_{phys} to an extremely small value if f is ridiculously small.

However, it turns out that string theory typically contains more than one field $F_{\mu\nu\rho\sigma}$ Usually it contains tens or hundreds. Taking N such fields into account, the result now becomes

$$\Lambda_{\text{phys}} = \Lambda + \frac{1}{2}\sum_{i=1}^{N} n_i^2 f_i^2. \tag{7}$$

One would expect the values for the real numbers f_i to be different. Again an analogy with electromagnetic fields is useful. Although for the Standard Model we need just one such vector field, string theory may contain more than one. Generically, these will all have different fine-structure constants, or in other words different values for the electromagnetic couplings e_i.

If indeed the values of f_i are distinct, and in fact incommensurate, then Eqn. (7) defines a dense set of values. Bousso and Polchinski called it a "discretuum", a very descriptive though linguistically awkward name. It is an easy exercise to show that with N equal to a few hundred, values for f_i of the order of electromagnetic couplings and small integers n_i, one can indeed obtain the required small value of Λ_{phys}, given some negative Λ. It is clear that in addition to the correct value for Λ_{phys} one will find a huge number of wrong values. This is the price one pays if the cosmological constant is to be neutralized in this way.

I should emphasize one important point. All the ingredients used in the foregoing discussion are already present in string theory; nothing was added by hand. In particular the fields $F_{\mu\nu\rho\sigma}$ are present, and the quantization of the field strengths follows using standard arguments. The fact that there are so many of these fields is closely related to what used to be viewed as an embarrassment: the large number of moduli. Furthermore the mechanism could not even have worked by having a single, extremely small f. This would allow the same fine-tuning of Λ, but one would end up in an empty universe [43].

Although the Bousso-Polchinski mechanism can be derived in string theory, this does not mean that no assumptions or approximations were made. In reality, everything is quite a bit more complicated than explained above. In addition to the field strengths $F_{\mu\nu\rho\sigma}$ there are analogous fields that live in the six (or seven) compactified dimensions. They play a rôle in the quantization of the field strengths, in a way analogous to the old argument due to Dirac that relates the existence of magnetic monopoles to charge quantization. Those fields are analogous to electromagnetic flux lines that are wrapped around cycles in the internal space, like rubber bands around a doughnut. The arguments of [29] are done in an idealized situation, and ignore the back-reaction of the non-zero values of these "flux fields" on the compactification manifold. They treated the manifold as rigid, so that around any of its cycles an unlimited number of "fluxes" could be wound, without affecting the shape (*i.e* the values of the moduli). But in the supersymmetric limit, the manifold is certainly not rigid. Quite the contrary, its shape can be changed without cost in energy by changing the moduli. The mechanism can therefore not be made to work without solving at the same time the problem of stabilizing those moduli. Furthermore, one cannot simply add arbitrary fluxes to a given string theory compactification. If we start with an exact solution to the equations of motion, we cannot simply add fields and and expect that we still have a solution after doing that. One has to reconsider the full set of equations of motion for every set of choices n_i. It turns out that those flux lines contribute to the stabilization of the internal space, in other words to the moduli stabilization problem explained earlier. In practice, Bousso-Polchinski fluxes only stabilize part of the moduli. In addition we have to get rid of the symmetry that made the entire discussion technically possible, but that is not observed in nature: supersymmetry. Other mechanisms are needed (and available [44]) to achieve these goals.

The description of this full stabilization mechanism combined with supersymmetry breaking takes us to (or even beyond) the edge of the current knowledge of string theory, and there is a lot of debate about the validity of the approximation used. The skeptics often express their doubts with the phrase "no example of this is known in a controlled approximation". This statement would probably hold up in court, at present, but I think it is used mainly as a last straw by people who are still hoping for uniqueness.

The cosmological constant problem finally solved?

The cosmological constant problem has been viewed during several decades as one of the most difficult and profound problems in fundamental physics. Can we now declare it "solved"? Well, not yet. As I have emphasized earlier, string theory is not yet properly a "theory". And even if that problem is solved, we still do not know if it is a theory of our universe. It is not enough to know that a Bousso-Polchinski discretuum of sufficient density can exist, but it must exist in combination with a realization of the Standard Model. The difficulty with string theory is that these two issues cannot be decoupled. However, within the context of string theory the cosmological constant problem has lost its status as the most profound problem we have to worry about. It is truly remarkable that string theory guided us to this potential solution of the problem, despite the fact

that this is not the kind of solution most people hoped for.

5.3 The String Theory Landscape

The combination of the results of the two string vacuum revolutions was called the "String Theory Landscape" by Susskind [2] [46]. It yields some distribution of points in the huge space formed by the Gauge Theory Plane combined with a cosmological constant axis. One of those points should correspond to our universe. To find out if that is true, we need a map of the landscape.

Naively, one might think that these two revolutions could be combined in the following way. The 1986 revolution produced a huge number of topologically distinct supersymmetric moduli spaces, each with tens or hundreds of moduli. Each defines some piece of the Gauge Theory Plane: a choice of particles and gauge symmetries, and a set of continuous parameters. Let us assume that we can work out the vacuum energy as a function of the moduli, after having solved the moduli stabilization problem and the supersymmetry breaking problem. This then defines a kind of potential on the Gauge Theory Plane. Let us furthermore assume that this potential has some discrete local minima. These local minima fix the values of the moduli, and thereby the values of all couplings and masses. Now we add the Bousso-Polchinski fields. The local minimum also fixes the numbers Λ and f_i in formula (7). We can add a cosmological constant axis orthogonally to the Gauge Theory Plane, and all the Bousso Polchinski mechanism would do is to generate a infinite number of points along that axis, all projecting down to the same point in the Gauge Theory Plane. If this were correct, all we have to do is match the couplings and masses of one local minimum to the experimental data, and then use the Bousso-Polchinski integers n_i to make the physical cosmological constant as small as 10^{-123} times its natural, Planckian size. Ideally we might also hope to determine the integers n_i that achieve this, but in order to do this we would have to compute Λ and all the f_i with 123 digit precision, which is clearly impossible in practice.

But this is far too naive, as it completely ignores the back-reaction of the fluxes, as explained above. Indeed, the potential postulated above may not even *have* any local minima in the absence of fluxes. Furthermore, the local minima themselves will start moving in the Gauge Theory Plane when we change the integers n_i, and the range of allowed values for each n_i becomes finite. So adding fluxes is not like wrapping rubber bands around a rigid doughnut, but more like wrapping them around a balloon with many handles, which change shape when we increase the wrapping number, until they finally snap.

Consequently the total number of choices for the set of integers n_i is finite. Their range and their total number depends on the string compactification under consideration. For one particular one, the total number of possibilities was estimated to be around 10^{500} [47], an often quoted number that started leading a life of its own and is often quoted as an estimate for the total number of string vacua.[14] To get the total number of string vacua

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[14]Most likely the actual number is not even finite, but there are arguments [49] suggesting that it is finite after imposing bounds on the parameter space. In particular, the total number that would agree

one would have to sum such numbers over all possible compactifications, *i.e.* the results of the first string vacuum revolution. It is impossible to estimate at this moment how many of those there are. It is know that the number of topologically distinct Calabi-Yau spaces is at least 30.000, but this is a very conservative lower limit, and furthermore a straightforward compactification on a Calabi-Yau manifold is only a very small fraction of the possibilities discovered since 1984.

So there will not be a clean separation between the results of the two string vacuum revolutions. The picture where the first gives a scattering of points in the Gauge Theory Plane and the second a distribution of points on lines orthogonal to that plane is too naive. But it it might happen that all the points originating from the same moduli space, but with different values of n_i cluster around a certain central value when we projected them on the Gauge Theory Plane. In [48] it was argued that this would happen for statistical reasons: there are many more ways to get a value close to the central value than to get a point far away, for which all contributions would have to add up coherently. Under the assumptions of this paper, only dimensionful quantities (namely the cosmological constant and the scale of the weak interactions) would have a huge spread, whereas all others would form dense clusters of points, something like the interference points in the spot of a laser pointer.

If this is indeed true in the string theory landscape, we might still be able to discover features of the underlying topological moduli space in the parameter values, although only with limited precision. We might be able to find out which of the laser spots we live in. In order to get unlimited precision we would have to determine precisely which of the separate, distinct points within the laser spot corresponds to our world, and this is probably an impossible task.

Is the Standard Model contained in this huge and extremely complicated ensemble? This will be discussed in a little more detail in the next chapter. At present we know enough to say that the very rough topological features of the Standard Model (*i.e.* the gauge groups and the quarks and lepton representations, including the number of families) can indeed be reproduced. Despite the size of the landscape, this is non-trivial: infinitely many gauge theories are not realized. The exploration of more detailed features, masses and couplings, is still in its infancy. Furthermore a very small piece of the Landscape is accessible with currently available methods: we are only scratching the surface.

Neither the gauge group, nor the structure of a particle family, nor the number of families follow uniquely from string theory. The same will be true for all the remaining details, in particular the couplings. There will not be a simple formula for α expressing it in terms of e or π, as Feynman may have hoped. Instead we should expect a dense set of possible values, correlated in complicated ways with dense set of values of the other parameters. In some sense the second string vacuum revolution smears out the results of the first, making it harder to discover the Planckian topological origin of the Standard Model. But in the friendly landscape of [48] enough of the underlying topological structure may be preserved to allow us to solve that puzzle nevertheless.

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with all current experimental data would then be finite. In the following, I will continue to use the number 10^{500} as a guess for the number of vacua, but this should not be taken too literally.

6 Our place in the Landscape

6.1 Possible Landscapes

Without committing ourselves to one particular fundamental theory, let us compare some scenarios for the way the Standard Model might emerge from such a theory. No matter what the fundamental theory is, it will have a "landscape": the points in the Gauge Theory Plane it allows. One may distinguish several possibilities.

Scenario A: The Standard Model comes out uniquely. We can compute the gauge group, the type and number of families, all masses and couplings, and they all agree with experiment. There is a unique prediction for the value of the cosmological constant, or some effect that mimics it. Let us furthermore assume that there are good indications that this value is as small as it should be; a precise calculation might be too much to ask for.

Scenario B: The fundamental theory has a large, but still manageable number of vacua, let us say about 10^{30} in some finite region R of the Gauge Theory Plane near the Standard Model. Within the Gauge Theory Plane we can draw a region E that constitutes the current experimental information, with a size determined by the experimental error bars. It is represented by the small circle in the figure in section 3. If we assume that the 10^{30} points are smeared out more or less equally in a large region around the Standard Model (thus defining a measure), then 10^{30} is too small a number to expect one of them to land in the experimental region E just by chance. One estimate, due to Michael Douglas, implies that about 10^{80} vacua would be needed (the number 80 is the number of digits of information in the masses and couplings of the Standard Model particles, expressed in natural units, plus a guess about the likelihood for the discrete data, *i.e.* the gauge group and representations). Therefore, if we *do* find a solution within E (and with only 10^{30} to check we might just be able to find it), there would be 50 digits worth of predictions left over. This is less that the 80 digits we would have in scenario A, but still more than enough to convince ourselves that we had found the correct theory. This would at the same time prove, beyond any doubt, the mathematical existence of the other 10^{30} vacua.

Furthermore, such a result would prove the version of the anthropic principle advocated here. The relative size of the anthropic window (assuming the same measure) might be anywhere from 10^{-5} to 10^{-25} (depending mainly on the physical mechanism that determines the weak scale). If it is, for example, 10^{-10}, we may expect to find 10^{20} out of the 10^{30} vacua to lie within the anthropic region, but still the vast majority lies outside. Nobody would dispute the statement that some of the parameters of the Standard Model are anthropically determined. Furthermore in scenario B (as well as in A) there would an unlimited amount of falsifiable information available, since after determining the vacuum corresponding to our environment, everything would be fixed: we would be able to predict the properties of all particles and all interaction all the way up to the Planck scale, with unlimited precision.

After achieving this tremendous success, some people would start discussing the question why we find ourselves in one of the 10^{20} vacua that allow life, rather than any other.

Perhaps we would be able to argue that among the vacua that allow life, ours is among the most probable ones, but this would not be a major issue anymore. It would be a bit like worrying if our planet is the most common one in the set of all planets that allow life. That is not a major issue anymore because no-one doubts the existence of a landscape of possible planets: we can see more than a single sample with our own eyes.

In my opinion, scenario B is much more desirable than scenario A. Both would give us a huge amount of confidence in the correctness of the underlying theory, but scenario B has the advantage that it also explains why we ended up in such an apparently fine tuned universe, something that scenario A converts into an eternal mystery.

However, there is no theoretical upper limit on the number of vacua the fundamental theory may have. In this situation, hoping for not more than 10^{30} vacua is wishful thinking, just as hoping for just one. Furthermore, scenario B is unlikely to work if we include the cosmological constant into the discussion. After having determined which of the 10^{30} vacua we live in, this becomes a computable number, and it has to have a value of about 10^{-120}, although it is naturally of order 1. This computation requires an unachievable amount of precision, but no-one would expect it to work just by chance, and even if it did we would once again be left with an eternal mystery.

The only way out would be to hope for a fundamental theory that by some mechanism dramatically reduces the natural range of the cosmological constant. That might be acceptable: as I argued in section (3.9), it does not produce the same kind of troublesome conflict between unrelated 'gedanken' computations as a derivation of the Standard Model would. However, no such theory is in sight.

In string theory, it seems that we end up with scenario C: The actual number of vacua is too large to allow enumeration, let us say 10^{500} for definiteness. If they are spread around evenly over the relevant region, this would nicely explain all anthropic coincidences, including the cosmological constant, but there is no way to determine exactly which one is realized in our universe. Indeed, in case of a structureless distribution, if we include the 80 digits of the Standard Model data and the 120 of the cosmological constant, then there are still 10^{300} vacua left that fit all the current data. Unfortunately those 120 digits are not even of any practical value. In order to use them we would have to compute each of the two terms on the right hand side of Eqn. (7) with a precision of 120 digits.

In this scenario there are not only more vacua than current experimental data, but in addition they may be spread over the parameter space in such a way that, statistically speaking, there is no way the Standard Model could be absent. In fact, it could be present so abundantly that no predictions can be made. In this case, it seems that we are back where we started: we end up with the Standard Model with all its parameters taking practically any possible value. But even if this were the final result, it would still be tremendous progress. First of all we would have succeeded in coupling the Standard Model to a consistent theory of quantum gravity. Secondly, all parameters of the Standard Model are now dynamical, allowing them to change in the very early universe, or during the birth of new ones, providing a mechanism to move around in the landscape. And last, but certainly not least, we would have gained a profound understanding of why we live in such a special universe, apparently finely tuned to allow our existence. In this case we

would not gain any insight in the Standard Model itself, nor would we be able to use it to learn something about the theory of quantum gravity. But this was neither guaranteed nor required.

But the amount of experimental data is monotonically increasing, so for any finite number of (relevant) vacua, scenario C will eventually turn into scenario B. Even if the landscape densely covers the parameter space of the Standard Model, this may not be the case for extensions of the Standard Model we may discover experimentally. Undoubtedly the 10^{300} vacua would make entire different predictions about such extensions. One popular extension of the Standard Model, low energy supersymmetry, adds 105 parameters. Measuring each of them with three digit precision would bring us to the highly desirable scenario B. Obviously this is far beyond our present horizon.

The extreme limit of this might be called scenario D. This is a featureless landscape obtained by covering the entire gauge theory plane densely (or even continuously) with points according to some smooth distribution. The string theory landscape is probably not like this at all. The little bit we know about the string theory landscape does not suggest that, and what we know about the Standard Model does not suggest that it lives in such a landscape.

6.2   String Spectra

Before discussing how the Standard Model fits (or might not fit) in the string theory landscape, let us consider what we know about the kind of spectra string theory produces.

A string spectrum consists of a few massless particles plus an infinite "tower" of particles with Planckian masses. The latter cannot be observed, because they are out of range of any imaginable accelerator. The Standard Model particles are of course not exactly massless. In Planckian units, there masses are of order 10^{-19}, so if they come out massless in a string spectrum, that is the correct answer to an excellent approximation, but not what we ultimately want.

Such spectra can be computed rather easily in a huge number of cases. It is usually an algebraic exercise that can be done by hand in a few cases, but is normally done by means of a computer. The result is described in terms of some Lie-group (called the "gauge group") and a set of representations of that group. The Lie group gives us a set of vector bosons, like the photon or the gluon (the vector bosons that bind quarks together in a proton or neutron), and the representations tell us how the massless particles couple to these vector bosons. They are essentially the charges of the particles.

In nearly all published results the spectra are obtained in supersymmetric string theory. This means that every particle is part of a boson/fermion pair with identical masses. In particular, all the Standard Model particles have massless supersymmetric partners, because it turns out that the already know particles cannot be supersymmetrically paired. The reason for computing supersymmetric spectra is that non-supersymmetric spectra are much more difficult to control in exact string theory. They tend to have instabilities which are automatically absent or easily avoided in the presence of supersymmetry. In the absence of supersymmetry, these instabilities ("tachyons" and "tadpoles") can all be

avoided at the lowest order in perturbation theory in exact string theory, but at the price if a huge loss in statistics (the conditions for the absence of instabilities are very restrictive) and even then one still has to worry about stability at higher orders in perturbation theory. For this reason, the most common approach is to compute supersymmetric spectra in exact string theory, and make further assumptions about low energy physics from there on. Ultimately, all these extra assumptions will have to be derived from string theory.

These exact supersymmetric string spectra are not really points in the string landscape yet. They are points in flat planes in the landscape, the moduli spaces. After supersymmetry is broken and the moduli are stabilized, a true landscape of mountains and valleys is expected to rise up on these planes, and we hope to live in one of those valleys. Ideally, we would prefer to compute the bottom of one of these valleys exactly in string theory, but just as anywhere else in physics, the only available way is to get as close as possible with exact methods, and then approach the point of interest using a variety of approximations.

In this process of landscape formation, the masses of all particles are expected to shift. In order for the particles in a supersymmetric spectrum to get a realistic mass two things have to happen: supersymmetry has to be broken, and the weak interaction symmetry (the $SU(2)$ of the Standard Model) has to be broken. In both cases this is believed to happen because the true vacuum of the theory, the valley of the landscape where we end up, violates these symmetries by a tiny amount: they are symmetries of the Hamiltonian but not of the ground state. In the case of the weak interactions this goes by the name "Higgs mechanism", and leads to the prediction that a neutral scalar particle should exist, the famous Higgs boson.

When supersymmetry is broken, the fermionic and bosonic partners of the Standard Model particles acquire a mass. There is indeed an excellent reason why the as yet unobserved superpartners acquire a mass at this stage, and not the Standard Model particles themselves. The reason is that the Standard Model particles can only get a mass once the weak interaction symmetries are broken as well. Because we have not observed any of the supersymmetric particles yet, the scale associated with supersymmetry breaking (the mass splitting among the supersymmetric particles) is apparently larger than the scale of weak interaction symmetry breaking (related the masses of the W and Z bosons and the heaviest quarks). Usually one takes something of order 1 TeV for the former, whereas the latter is of order 100 GeV. There are no convincing fundamental physics arguments for the values of these scales, and in a landscape a large range of values is likely to exist, with an unknown distribution.

Once one of these supersymmetric string spectra comes out of the computer, the first criterion we can impose is that the set of vector bosons must include the ones of the Standard Model, in other words that the gauge group contains $SU(3) \times SU(2) \times U(1)$. It may contain more than that; there may exist vector bosons we have not seen yet, either because in our valley they have acquired a mass out of reach of current accelerators, or because they do not couple to any known matter. The next thing to check is that there are other particles (supersymmetric multiplets of fermions and bosons) that couple to the $SU(3) \times SU(2) \times U(1)$ vector bosons precisely as three families of quarks and leptons,

i.e. three times Eqn. (1). Also in this case there may exist additional particles, called "exotics", which may be a blessing or a curse, depending on their coupling to the Standard Model. Just as superfluous vector bosons, exotics may acquire a mass when we move out of the supersymmetry plane into our own valley.

Supersymmetric spectra containing (at least) the Standard Model with three families of quarks and leptons exist in abundance in many regions of the string landscape. There are examples with just the Standard Model gauge group and nothing else, and there are examples without exotics, but as far as I know there is not yet an exact string theory example which has the exact supersymmetric Standard Model spectrum without anything else. This seems just a matter of statistics: if we work out enough examples, eventually we will find this. But it may well be that this is not what we should be looking for anyway: there appears to exist dark matter that does not belong in the Standard Model. Many people hope that this dark matter is made up of some supersymmetric partners of the Standard Model particles, but it could well be something entirely different.

The next step is to compute the coupling constants between the various particles. In particular the Yukawa couplings between fermions and the Higgs boson are of great interest. Knowing them, we can work out the mass ratios of quarks and leptons, and compare with observations. However, this is considerably harder than working out groups and representations. It is not just harder because the computation of coupling constants is technically more complicated; the more important problem is that these quantities (unlike groups and representations) depend on the moduli, in other words they vary over the supersymmetry plane. This implies that they will also take distinct values in the various valleys of the landscape the rises up on top of that plane. This is good, because it is precisely this variation that will be needed to end up in one of the small anthropic regions. Coupling constants have been computed in a few cases, but little is known about the distribution of their values over the landscape.

6.3   String Phenomenology

The field that deals with the way the Standard Model is realized in string theory is called "String Phenomenology". It is an essential activity: all matter, including the Standard Model has to fit into string theory, or else this theory has nothing to do with our universe. One may think that the explosion of string vacua should have had a major impact on this field. I have been active in this are around 1987 (which led me to the conclusions presented here) and again in the last few years, and to me the similarities are more striking than the differences. There has certainly been progress: we can obtain string solutions that are more similar to the Standard Model than twenty years ago, and we have more methods to construct them. There has been major progress in moduli stabilization and supersymmetry breaking. There is more interest in "landscape statistics". But very little seems to have changed in the way many people view the problem we are facing. Although many of my string phenomology colleagues claim that it was clear to them a long time ago that there are many solutions, I cannot help noticing that they still talk about their

most recent "model"[15] as if it would actually have a chance to be *the* Standard Model. And even nowadays one still hears the occasional expression of hope for the unknown and elusive dynamical principle that will select the vacuum. The most common way of dealing with the large vacuum degeneracy is to say "I do not care about the other 10^{500} vacua, I only care about the one that describes our universe". That may sound reasonable, and fact it may sound like the very definition of phenomenology, but it is actually an escape from reality.

First of all, if indeed there are 10^{500} vacua, it is highly unlikely that anyone will find "*the* Standard Model" in string theory. One should expect to find a huge number that satisfy all current experimental constraints. In addition, although we now have many techniques at our disposal to construct string theories in four dimensions, it is quite clear that we are just scratching the surface. Statistically speaking, our chances of finding even one of the expected huge number of Standard Model realizations is essentially zero. Furthermore, even if we do find one, we can only make predictions about novel phenomena if we know *all* the other solutions and their predictions for the same phenomena. This is a crucial change in comparison to the state of the art about ten years ago: with 10^{20} solutions (the largest number anyone may have expected), if one is found that agrees with all current data, the probability that there is a second one is extremely small. With 10^{500}, the same probability is astronomical. So we should forget about the idea of finding *the* Standard Model and then making predictions based on it.

The second reason why the aforementioned "phenomenological" point of view is unreasonable is that the phenomenological wish list has always included the so called "why" problems: why $SU(3) \times SU(2) \times U(1)$, why three families, etc. If the Standard Model is part of a huge ensemble, the only way to answer such questions is to understand the distribution of that ensemble. We *have* to care about more than just our own universe. An inevitable consequence of a huge ensemble is that anthropic arguments will play a part in answering some of the "why" questions. But in the field of string phenomenology this is still nearly equally unmentionable as twenty years ago. There is even more attention to anthropic arguments in traditional phenomenology (*i.e.* not based on string theory) than there is in the field that cries out for it, string phenomenology. On the other hand, I often hear talks containing statements like "property X is observed in our universe, but occurs rarely in string theory. Here we (proudly) present an example with property X". Examples of property X are the absence of exotics (see below) or the presence of exactly three families. In this situation I ask myself the question: "if property X is so rare in string theory, why do we observe it in our universe?", but few other people seem to worry about that.

Of course most of the 10^{500} vacua are utterly irrelevant to us. It may be possible to identify regions in the landscape where Standard-Model-like spectra occur more abun-

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[15]The way particle physicists use the world "model" may be confusing to outsiders. In this context it simply means some string theory spectrum, which is not really modelling anything else. The use of this word dates back to the days the Standard Model was constructed as an approximate, idealized description (model) of the forces of nature. Attempts to build something with similar properties were called "model building". In string theory one does not really build anything; one simply finds what is there.

dantly, and other regions that are essentially barren. It may well be that most of the huge number of vacua are in such barren regions. However, we cannot expect that to reduce the number that really matters to something extremely small. If that were true, the cosmological constant problem is still not solved, nor are the anthropic fine-tunings of the Standard Model explained. If the Standard Model were an essentially isolated point, the anthropic reason of existence of the landscape would lose its meaning. We must expect to find ourselves in a dense region of the landscape. There is no reason why the densest region of the landscape should coincide with the anthropic region – in fact, if it would that would itself be a mystery – but *within* the anthropic region most universes with observers obviously occur in the densest regions.

So what *can* realistically be achieved in string phenomenology? A fairly modest goal is to use string theory as a guide to new ideas in phenomenology beyond the Standard Model, which keep their validity outside that context; there are already plenty of examples of that, but this is not my main interest here. A goal that seems within reach is to arrive at a statistical proof that what we are looking for, a string vacuum that agrees with all current data, must exist, even if we cannot find a single explicit example. It may be possible to identify features of the Standard Model with those of certain classes of string vacua, and extract generic predictions for that class. But the most important point I am trying to make here is that we should not focus too strongly on the observed Standard Model, but explore the region around it. Even if we have convinced ourselves that the Standard Model is present in the landscape, there are serious challenges left, and I will discuss some of them in the next section.

6.4 Challenges and worries

The Standard Model may look complicated, but it does not look like someone was throwing darts at random into in the Gauge Theory Plane until an observer window was hit. In section (3.3) those aspects of Standard Model structure having to do with our own existence were sketched. Here I will focus on the rest. The *anthropic* features of the Standard model need not be typical in the full ensemble of possibilities; some parameters may be outliers, taking some rather extreme values in our universe. However, the *non-anthropic* structure has to be understood in terms of a landscape of a fundamental theory.

The denser the string theory landscape, the less information we will get from the necessary condition that it must contain the Standard Model. In this limit the more pressing question becomes: it is really plausible that the Standard Model structure we see comes from the string theory landscape? Ultimately, this would involve computing relative probabilities of various features, which may be beyond our present capabilities. However, if some clearly non-anthropic feature is dominant in the accessible part of the landscape, and is not seen in the Standard Model, this is a reason for concern. An accumulation of such concerns will end up removing our confidence in the hypothesis that the string theory landscape is the right one. Here "dominant" means that the number of vacua with a given property vastly outnumbers the number of vacua without that property. In ensembles as large as the string theory landscape, such relative multiplicities can easily

be huge. Of course the multiplicity of vacua is not the final answer. One has to combine this with the probability that we end up in a particular vacuum when a new universe is created. Such probabilities are notoriously hard to define, not to mention compute. One could still try to argue that such an initial probability might compensate for the dominance of a wrong feature, but it seems implausible that cosmic probabilities have large variations over the gauge theory parameter space, and even less plausible that such a variation would precisely compensate an undesirable surplus or absence of vacua.

We are assuming here that the Standard Model is directly embedded in this fundamental theory, without layers of unknown physics in between. Otherwise it is simply to early to ask the question. A bit of intermediate structure may be acceptable, provided it is a sufficiently transparent layer. However, it is certainly necessary that the quarks and leptons are fundamental degrees of freedom, and not some sort of composites. All of the following features are serious challenges for any landscape, and most of them will come out wrong in a featureless landscape.

The θ-parameter

Perhaps the most obvious challenge is the so-called θ-parameter of QCD, one if the 28 parameters of the Standard Model. This is an angular parameter that is experimentally consistent with zero, for no known reason. If non-zero, it would generate an electric dipole moment for the neutron, which is not observed[16] The current limit on θ is about 10^{-9}. Unlike the cosmological constant, there is no anthropic constraint on this number. Even if a non-zero value were measured in one of the ongoing efforts to measure the neutron electric dipole moment, it would still be unacceptable that we live in a universe with $\theta \approx 10^{-9}$ purely by non-anthropic coincidence. It is noteworthy that there does exist a mechanism in which $\theta = 0$ automatically. This mechanism requires the existence of a new particle, the "axion" which may have anthropic implications [50]. So then the challenge is to demonstrate that this mechanism is realized naturally in a candidate landscape, or that we are forced into it by a chain of indirect anthropic requirements.

Gauge groups and representations

The Standard Model gauge group, $SU(3) \times SU(2) \times U(1)$ with its family structure looks like one of the simplest gauge theories with a sufficiently interesting spectrum (although one can argue about the need for the $SU(2)$ factor [12]). Since in the string theory landscape large gauge groups are statistically suppressed, and smaller ones may not allow life, the Standard Model looks like a reasonable choice: perhaps among all the anthropic ones it is the most abundant one.

One may distinguish string theories in which all strings are closed, and string theories in which both open strings (strings with two end-points) and closed strings occur (this distinction is valid if we are in the neighbourhood of one of the perturbative regions). In both

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[16]The parameter θ also violates CP-invariance (which is essentially the same as invariance under time-reversal), but since this symmetry is violated in the weak interactions, it is not a symmetry of nature, and hence not a valid argument for the vanishing of θ.

cases, gravity is described by closed strings. In the former case, all other interactions are described by closed strings as well, but in the latter case all non-gravitational interactions are described by open strings. There seems to be nothing fundamentally wrong with either approach. Indeed, the existence of both is required by a complicated network of "dualities", amazing relations between apparently totally different string theories. It is easy to get the basic standard model structure to come out right in either case. Historically this was first achieved in 1985 [37] in closed string theories, in particular the heterotic string. One can say without exaggeration that the Standard Model comes out rather naturally in these theories. By simply imposing the condition that space-time should have four flat dimensions one gets a theory with a certain number of families, although not directly with the group $SU(3) \times SU(2) \times U(1)$ but rather with one of the unified gauge groups (see below) that were first proposed in the mid-seventies. Around the turn of the century [51] it was shown that the same feat could be achieved with open strings as well. The fact that this took so much longer is due only to the fact that open strings are technically a bit more difficult to deal with. Both Standard Model realizations, closed as well as open, are among the simplest constructions one can make in either case. It is straightforward to write down examples of gauge theories and matter that would be essentially impossible to obtain from string theory, and that at the very least would require some extremely contrived and unnatural tricks. It is perhaps even slightly disturbing that one can get the Standard Model so easily in *two* totally different ways. To some people, this may smell like a consequence of the aforementioned duality. If one could make that precise, it would be additional circumstantial evidence that the structure of the Standard Model suggests a string theory origin.

The structure of a single family is also one of the simplest non-trivial choices. It is strongly restricted by consistency conditions called "anomaly cancellation", leading to cubic sum rules for charges.

The number of families

But why do we observe *three* families of quarks and leptons? For our existence, one family might seem all that is needed: we are made of up-quarks, down-quarks and electrons. The top quark may play an essential rôle in triggering weak symmetry breaking, which might justify the existence of two families. Why there are three families is somewhat of a mystery. Already for a long time there exists a qualitative argument: with three families one can construct couplings that violate CP symmetry in the quark sector [52]. CP violation is one of the famous three necessary conditions formulated by the Russian physicist Andrei Sakharov for generating a surplus of baryons over anti-baryons in the universe. Such a surplus is, at least locally, clearly necessary for our existence. But using CP-violation in the quark sector does not appear to work quantitatively, and even if it did, it would work just as well with four or more families. One can also generate a surplus of baryons using CP violation in the lepton sector, by first generating a surplus of leptons ("leptogenesis") [53] [54]. However, this uses the so-called Majorana masses of neutrinos, a feature that has no analog in the quark sector. This in its turn introduces new possibilities for CP

violation which would allow leptogenesis with just two families.

The number of families affects the physics of our universe in other ways. One of them is neutrino processes, for example Big Bang nucleosynthesis (see below). All families enter in the running of coupling constants through the coefficients b_0 in Eqn. (2). If we simply add a family or remove one while keeping everything else fixed at the Planck scale (while making some choices for the masses of the families to be added or removed), the strong and electromagnetic coupling constant will move far enough from their observed values to endanger our anthropic environment. To some extent this can still be compensated by changing the initial values at the Planck scale. But if there are more than 16 quark flavors (*i.e.* eight families) in a non-supersymmetric version of the Standard Model even that does not work anymore: in that case the QCD coupling constant decreases towards lower energies, and hence the strong force becomes a weak force. If one considers other couplings and higher order quantum corrections even stronger limits may emerge. In [55] it is shown that with four families the Standard Model couplings cannot be extrapolated consistently to the Planck scale anymore. If we turn that around, it would imply that a four family analog of the Standard Model (keeping everything else as much as possible fixed) cannot be obtained from a fundamental theory at the Planck scale. To turn any of these statements into true anthropic bounds requires a careful and systematic study of possible initial conditions at the Planck scale, to see exactly under which modified conditions we can still reach our current anthropic island in parameter space.
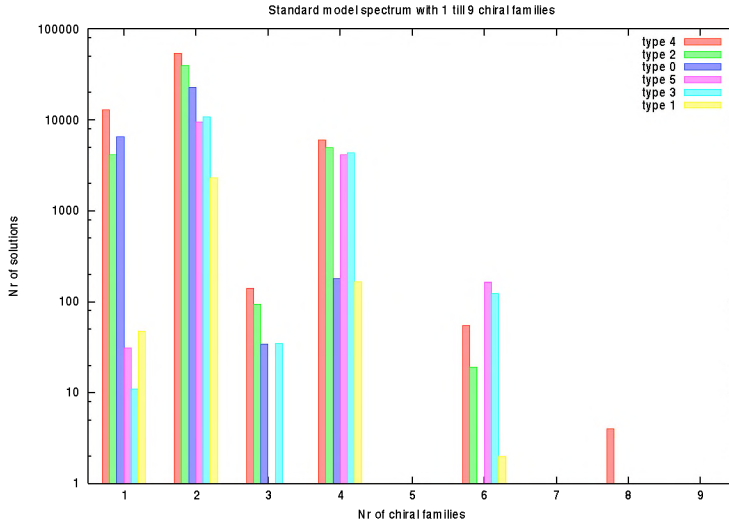
In string theory, the fact that there is a repetition of families is easily reproduced. In the simplest heterotic models, one obtains a number of families equal to the Euler number of some six-dimensional compactification manifold. This allows many possibilities, up to hundreds of families. The distribution of the number of families has been studied in certain cases for open strings. (*e.g.* [56], [57]). Some care is needed in drawing conclusions from these results: they are obtained for samples of supersymmetric models, and not for true landscape distributions. Furthermore they only cover a very limited region of the landscape. It turns out that vacua with larger number of families are exponentially

suppressed. This is clearly visible in the figure above, which is from the first of these papers (the colors represent different ways of realizing the Standard Model; the vertical scale represent the number of models found with a given number of families within a certain sample; the absence of five families is simply a result of having a small enough sample size to keep the number of two-family models reasonably small). Note that odd numbers are suppressed with respect to even ones, and as a result four families are one or two orders of magnitude more common than three.

We may conclude that it is a minor puzzle why there are three families in our universe: cases with two families are more common by a factor of a hundred to a thousand, while there is no convincing anthropic argument against having just two families.

Quark and lepton masses

Although they are poorly understood, quark and charged lepton masses certainly do not look structureless, and are not fully determined by anthropic considerations either. The distribution of their values must tell us something about the nature of the underlying theory. Donoghue [10] has suggested that they are more or less randomly distributed on a logarithmic scale, a feature that is indeed produced in a class of string models. If the masses were more or less random on a linear scale, one would expect most of them to be distributed around some natural scale for quark or lepton masses, which is clearly not the case. On the other hand, the masses may be hierarchical, expressible in powers of some small parameter. These hierarchies may be anthropically tuned, as they help in pushing down the up and down quark masses and the electron masses to small values. It is quite common in string models that some of the quark or lepton masses vanish to first order in some perturbative expansion parameter, and appear in higher order or by non-perturbative effects. Too little is known about masses and their distributions in the string theory landscape to see quark and lepton masses as a serious worry at the moment.

Neutrinos

Every Standard Model family contains two quarks, with charges differing by one unit (the charges are $\frac{2}{3}$ and $-\frac{1}{3}$), and two leptons, also with charges differing by one unit. If we order the families by mass, the quark masses within a family differ by less than two orders of magnitude. But for the leptons this is radically different. Leptons can have charge -1 or 0. The charged leptons (electron, muon and tau) have masses ranging from .511 MeV to about 1888 MeV. Neutrinos are so light that for a long time they were thought to be massless. The experimental upper bound on the mass of the electron neutrino is about 2 eV, at least five orders of magnitude below the lightest charged lepton. Since about a decade we know that the differences of the squares of the neutrinos masses are non-zero. This is a consequence of the observation that a quantum-mechanical linear combination of distinct mass eigenstates changes its composition periodically with time ("neutrino oscillations"), a process that vanishes if the masses are the same. These differences are tiny: the two that have been measured are of order 10^{-4} and 10^{-2} eV^2 respectively. If we make the assumption that the actual masses are of the same order as the differences, we

get masses of order .01eV to .1eV, about 6 to 10 orders of magnitude smaller than typical charged lepton masses (depending on which charged lepton one considers "typical").

This extreme lightness of neutrinos might appear to be a deep mystery, but a potential way out is suggested by the fact that neutrinos, unlike all other quarks and leptons, have zero charge. This means that they have the same charge as their anti-particle. This in its turn implies that there might exist a possibility of quantum-mechanical mixing between neutrinos and anti-neutrinos. For a single family, this mixing results in a two-by-two mass matrix with two parameters, of the form

$$\begin{pmatrix} 0 & m \\ m & M \end{pmatrix} \tag{8}$$

Here m (called the Dirac mass parameter) is generated in the same way as all other quark and lepton masses (*i.e.* by means of the Higgs mechanism). Therefore it may be expected to be of the same order of magnitude as those masses (which still leaves a huge range from .511 MeV for the electron to 174 GeV for the top quark). The parameter M (called the Majorana mass parameter) is special for neutrinos and does not occur for charged leptons and quarks. For $M = 0$ a neutrino behaves like any other lepton, and has mass m. For $M \gg m$ there are two neutrino mass eigenstates per family, one with a large mass M, and one with a tiny mass m^2/M (this is know as the "see-saw mechanism").

There is a priori no reason to expect any particular value for M. In other words, its natural value, without any theoretical prejudice, would be a number of order 1 times the Planck mass, about 10^{19} GeV. Even for the largest acceptable values for m, about 200 GeV, this would give neutrino masses that are too small to yield the observed differences. Although some people are advocating using the unification scale (see below) of about 10^{16} GeV for M, this also seems uncomfortably large. This would just barely give reasonable neutrino masses if we assume m to be of order the top quark mass, whereas it seems to be more plausible that m is like a typical charged lepton mass, *i.e.* at least two orders of magnitude below the top quark mass. This implies that M must have a value at some intermediate scale between the weak scale and the unification scale.

Although the neutrino interacts so weakly that it cannot have any relevance for our everyday life, it does affect our existence in many ways. It plays an essential rôle in Big Bang Nucleosynthesis (the creation of the light elements). Indeed, this led to a rather tight upper limit on the total number of neutrino species (which is meanwhile superseded by the result of the LEP experiments, $2.994 \pm .012$ neutrino species). Neutrinos are important in the functioning of the sun, in supernova explosions (which liberate the heavy elements made in stars), in the decay of the neutron (which would be stable if the electron neutrino had a typical leptonic mass), and they contribute to the mass density of the universe, just to name a few examples. The latter fact leads to an anthropic bound on neutrino masses: if the sum of their masses exceeds about 40 eV they would "over-close" the universe. This means that the mass density of the universe is so large that it collapses. The anthropic window might be even smaller if neutrinos are involved in generating the matter-antimatter asymmetry in our universe through a process called "leptogenesis". This works only if all neutrinos are lighter than 0.1 eV [53][54]. The anthropic nature

of this bound requires more discussion: they are derived under the assumption that only the neutrino mass is varied, while everything else is kept fixed and one would have to systematically investigate all alternative mechanisms before claiming that without light neutrinos we cannot exist.

Neutrinos provide thus an interesting battle ground between fundamental and anthropic explanations. The string theory landscape will certainly provide a wide range of possible values for both m and M. There is a tendency to say that their values will be "generically non-zero". This would be true in the Gauge Theory Plane: if we pick a random point we will find non-zero values. But is that also true in the string theory landscape? One might hope that string theory is not generic in all respects. If string theory only produces generic points in the Gauge Theory Plane, there is little hope left to extract a prediction from it that could not have been made just as well without string theory.

In explicit examples one often finds that m or M vanish. Perhaps this is because we only have access to rather simple examples, which might be atypical. There is no anthropic *lower* limit on neutrino masses; if there were it would have been possible a long time ago to predict that they would be massive. This means that the anthropic requirements are fulfilled if all neutrino masses vanish. If vanishing neutrino mass turns out to be a dominant feature in the string theory landscape, this would be sign that something is seriously wrong. Unfortunately the attitude of most string phenomenologist is to simply reject models without a mechanism to generate neutrino masses; they do not agree with experiment, so they are of no interest. Likewise, in explicit models it is not easy to get non-zero values for M. If M vanishes, but m does not, neutrinos have typical leptonic masses. If $M = 0$ turns out to be dominant in the string theory landscape, this would be a smaller problem, as long as no direct evidence for Majorana masses exists. One could then argue that the small masses we observe are Dirac masses, which are just exceptionally small for anthropic reasons.

In [62] an anthropic probability argument was presented in favor of neutrino masses of the observed order of magnitude. This argument is based on the effect of neutrinos on galaxy formation. However, a prerequisite for applying such an argument to the string theory landscape is a better understanding of mass distributions.

Another interesting issue is the mass hierarchy we observe for the charged quarks and leptons. In the Standard Model this hierarchy helps in pushing the anthropically important electron, up-quark and down-quarks masses to small values. For neutrinos, this is not needed. It would therefore be very interesting to know if there is a hierarchy in the values of the neutrino Dirac masses m. And from a theoretical point of view it is obviously extremely important to find out if mass hierarchies for standard model particles of different charges are correlated.

There are many other issues that could be discussed here, but I just want to re-emphasize the main point: we should not just aim at finding "the Standard Model" in string theory, but explore all its variations, and ask if we live in the most plausible one.

Apparent unification

There are two remarkable facts about the Standard Model whose precise rôle in the story is not yet clear.

One family of Standard Model particles, Eqn. (1), fits precisely in a single representation of a larger gauge group, namely $SO(10)$, provided that we extend it with just one particle: a right-handed neutrino. This amounts to adding $(\mathbf{1}, \mathbf{1}, 0)$ to Eqn. (1), a particle that does not couple to any of three Standard Model interactions, not even the weak one. Nothing we know excludes the existence of such a particle, and the observation of neutrino masses even makes its existence plausible.

Another remarkable fact is the apparent convergence of the three Standard Model couplings, if we extrapolate these couplings to higher energies. Such a extrapolation can only be done if we know the exact particle spectrum, all the way up to the energy scale of interest. This includes particles with masses much larger than are currently experimentally accessible. All these particles contribute to the coefficients b_0 in Eqn. (2). If one assumes that, in addition to the know particles, the only ones that contribute are their supersymmetric partners (a new boson for every known fermion and vice-versa) plus the Higgs and its partner, then it is found that the three lines go roughly through one point at an energy of about 10^{16} GeV, with a precision of about one percent![17]

This will be subject to experimental verification at the LHC. If the LHC experiments do not find these supersymmetric partners as well as the Higgs, we must conclude that the seeming convergence of these three lines was indeed nothing but a coincidence, based on an invalid extrapolation. On the other hand, if these particles are found and if there are no surprises, then the three lines may go through one point at even higher precision, making it harder to dismiss as a mere coincidence. It is then natural to assume that the coupling constants do not just go through one point, but that at energies larger than their meeting point there is a single theory with a single coupling constant. This is called "Grand Unification". This new theory could have a gauge group $SO(10)$ (although there are other possibilities), which would nicely explain why one family of quarks and leptons looks like it fits in $SO(10)$.

The idea of Grand Unification goes back more than thirty years [58] and has had several ups and downs. It is partly responsible for the hope for uniqueness of the laws of physics. I will comment on that in Appendix B.

If the convergence of coupling constants survives the LHC, we have two new kinds of Standard Model structures to wonder about, namely not only Grand Unification but also supersymmetry. Most string theorists are very excited about that possibility, but is that excitement justified? If we did not know about string theory the answer would obviously be positive. We could say that we had discovered new symmetries that point to the existence of a beautiful new theory. But string theorists already know what that

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[17]This result depends on the masses of the supersymmetric partners. This is because the coefficients b_0 are actually functions of energy. To first approximation they are step functions that receive contributions from all particles with a mass smaller than a given energy. The convergence works if we assume that the supersymmetric partners start contribution to b_0 for energies larger than about a TeV.

theory is supposed to be, and it seems to me that they have to look at these discoveries (if indeed they are made) in a totally different way.

String theory does not really need Grand Unification. It already unifies all interactions with gravity, and there is no particular reason why the three gauge interactions should unify first, at an energy scale of about 10^{16} GeV, three orders of magnitude below the Planck scale.

Certainly there exist string constructions that produce supersymmetric Grand Unification. This occurs most naturally in the case of fully closed string models (in particular the Heterotic String). There are some difficulties with understanding the relative low scale of 10^{16} GeV, since a Planckian theory like string theory naturally favors a higher scale (slightly below the Planck scale, in fact). But even in Heterotic string theories, and certainly in theories based on open strings, it is at least as easy to get the Standard Model without any intermediate step of Grand Unification.

But then why, if the string theory landscape is correct, would we observe Grand Unification in our universe? Would it not be easier to end up in an observer region if one could tune all three couplings separately and independently, rather then being constrained by a relation between them?

One possibility would be that Grand Unified solutions numerically dominate the string theory landscape, if we impose the condition that we should get the Standard Model at low energies. They were certainly the first kind of solution to be found around 1984, but the first type of needle found in a haystack is not necessarily the most common one. A proper discussion would require an actual counting of the valleys in the landscape. If it can be shown that heterotic models dominate the subset of the landscape that contains the Standard Model, it would still seem most plausible that the distribution peaks at a value of the unification scale that is a bit closer to the Planck scale than 10^{16} GeV. But perhaps here the anthropic principle can help. Perhaps this is what pushes the unification scale to slightly smaller values in the tail of the distribution, since otherwise the strong interactions would end up being too strong.

If Grand Unified solutions do not dominate the Standard Model part of the string theory landscape, we may have to look for anthropic reasons why we see evidence for it in our universe (if indeed such evidence emerges). Most anthropic arguments in the Gauge Theory Plane have to do with chemistry and nuclear physics. They depend on the properties if quarks and leptons at very low energies, less than a GeV. If we consider the three Standard Model interactions, the strength of two of them (the electromagnetic and strong interactions) at those low energies is important, but the strength of the weak interactions seems less crucial. The fact that the three coupling constants are identical at a very short distance scale (corresponding to 10^{16} GeV) has no direct relevance for our existence. But Grand Unification may affect the cosmology of the very early universe. Depending on how its realized, it may imply a phase transition, when we move from the symmetric phase with a unified symmetry to the broken phase corresponding to the Standard Model. This phase transition may have some relevance for the early evolution of our universe. In addition, Grand Unification adds a mechanism for violating the conservation of baryon number. Indeed, in these theories protons decay with a lifetime of

about 10^{36} years. Stability of the proton has obvious anthropic implications, but that is not at stake here. Life can easily exist even when the proton lives much shorter than 10^{36} years. However, the baryon number violation itself may play a crucial rôle in generating the surplus of baryons over anti-baryons we observe ("baryogenesis"), and which does have anthropic implications. One of the three aforementioned Sakharov conditions for generating this surplus is – not surprisingly – violation of baryon number. However, baryogenesis related to grand unified theories has fallen out of favor as a plausible mechanism, for reasons to complicated to explain here. So even though Grand Unified Theories have potentially anthropic ingredients, I don't think anybody has put these ingredients together in a convincing way so far.

The discovery of low-energy supersymmetry, which would go hand-in-hand with the discovery of grand unification, poses even more questions. Low-energy supersymmetry means that the not yet observed bosonic and fermionic partners of the Standard Model particles have a small mass (in Planckian units), say about 1000 GeV. Some people claim that string theory predicts supersymmetry.[18] In presently known string theories supersymmetry plays an important rôle in controlling the gravitational quantum corrections at high energies (although in my opinion the purely string theoretic concept of modular invariance, mentioned in section (5.1), is even more important). The fact that we seem to need supersymmetry may simply be due to our limited abilities in dealing with *any* problem in quantum field theory, but let us assume that it is true. There is a convincing anthropic argument that supersymmetry cannot remain unbroken, *i.e.* that it cannot be an exact symmetry of nature: electrons would be degenerate with their bosonic partners, and all electrons in excited states of atoms could decay to their bosonic partners in the ground state; being bosons, they are not subject to the Pauli principle. Then there would be no atoms of any interest. So supersymmetry cannot be an exact symmetry of nature. This is of course also what we observe, but if there did not exist any anthropic argument against exact supersymmetry, its absence in our universe could be a serious worry for any theory – such as string theory – that seems to have exact supersymmetry built in. If supersymmetry is broken, this means that it can only be observed as a symmetry at high energies: the degeneracy between bosons and fermions is lifted, their masses differ by a quantity of order M, which may be ignored for energies much larger than M. If string theory has anything to do with our universe (anthropically and observationally), it must remain consistent when supersymmetry is broken at some scale M. But nothing in string theory, at least as far as anyone knows, predicts the value of M. So even if string theory predict supersymmetry, it still does not follow that it predicts *low-energy* supersymmetry. In other words, the mass differences between the super-partners could be anything.

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[18]But not everyone agrees with that, see *e.g.* [63]. However, a fair comparison of non-supersymmetric and supersymmetric string vacua requires better theoretical control over the former, and a way of dealing with relative measure factors.

The weak scale

A related, and very important point is the weak scale, and in particular its smallness with respect to the Planck scale: 246 GeV versus 10^{19} GeV. There is no doubt that this small ratio has essential implications for our existence [8], given the rest of the Standard Model.

The smallness of the weak scale has long been regarded as a deep mystery that required a solution in a fundamental theory, like the smallness of the cosmological constant. Both quantities appear to be "fine-tuned", which means that the natural size of all quantum corrections is much larger than the quantity itself. For the weak scale quantum corrections contribute to the square of the mass so that the observed value, $(246 \text{ GeV})^2$, is a factor of about 10^{-33} smaller than the "naive" quantum corrections from quantum gravity, of order $(M_{\text{Planck}})^2$. For the cosmological constant the natural size of the quantum corrections is $(M_{\text{Planck}})^4$, and the observed value is 120 orders of magnitude smaller. Decades of theoretical and experimental efforts have been focused at understanding the fundamental physics that determines the smallness of the weak scale, and many people expect the upcoming LHC experiments to give us crucial information. There is no convincing theoretical and *non*-anthropic argument that determines this scale, although it is possible that we will eventually stumble one a mechanism similar to the one the "explains" the QCD scale: a logarithmic modification of the measure. On the other hand, during all those years people have wondered whether we should worry about the weak scale hierarchy problem, as long as we had no clue about a much more serious one: the cosmological constant problem. If we accept the string theory landscape and in particular the Bousso Polchinski mechanism to neutralize the cosmological constant, this puts us in an interesting situation: there is no mechanism that "explains" the smallness of the cosmological constant, other than the fact that we would not exist if it were a few orders of magnitude larger. But if we accept that solution for the most horrendous hierarchy problem, then why would we need any special mechanisms for the lesser one? Perhaps the answer lies in the fact that the two problems are not completely analogous: the weak scale is associated with a symmetry breaking mechanism, and cannot be arbitrarily small, whereas the cosmological constant is just a number without any implications except in a very far future. Furthermore, the cosmological constant may just as well be zero, unlike the weak scale. The theoretical physics community is very much divided on this issue, with some people still insisting on the need for a fundamental mechanism, whereas others would agree with the point of view expressed by Weinberg [16] *"If the electroweak symmetry breaking scale is anthropically fixed, then we can give up the decades long search for a natural solution of the hierarchy problem."*.

The most popular physical idea associated with the weak scale is supersymmetry. In supersymmetric theories, quantum correction due to bosons exactly cancel quantum corrections due to fermions. As already mentioned above, the weak scale is determined by a parameter m^2 of dimension $(\text{mass})^2$, which acquires quantum corrections of order M_{Planck}^2. The physical weak scale m^2 is an infinite sum of such corrections, but its observed value should only be about $(100 \text{ GeV})^2$. This is where supersymmetry comes in. It nicely cancels all these large quantum corrections, so that it seems less bizarre that such a small

value comes out in the end. Supersymmetry does not, however, *explain* the actual value, and it has a mass parameter, named μ, which in principle can take any value. The real benefit lies in the fact that μ does not get Planckian corrections, unlike m. Nevertheless, understanding why μ is much smaller than M_{Planck} has worried people so much that this problem has been given a name, the "μ-problem".

We have encountered supersymmetry before as a possible fundamental symmetry of string theory, and also as a mechanism to provide extra particles that make the three Standard Model coupling constants converge. However, the most common argument in favor of supersymmetry is the one of the previous paragraph, the cancellation of corrections to m^2. In order to do its job it does not have to be an exact symmetry. If the mass differences between the superpartners is roughly equal to m, or a little larger, the quantum corrections to m^2 are themselves of order m^2 and there is no fine-tuning. So the absence of fine-tuning requires low energy supersymmetry, broken at a scale of about 1 TeV. Remarkably, this is precisely the mass the superpartners must have to make the coupling constants converge. Could this be just a coincidence? In the last few years, as experimental falsification of low energy supersymmetry might be approaching, there have been remarkable shifts of opinion. The lack of confidence that some people have in the fine-tuning arguments is perhaps best illustrated by an idea called "split supersymmetry" [59], in which supersymmetry is mutilated in such a way that it only contributes to the converge of the couplings, but does *not* solve the fine-tuning problem.

One could say that supersymmetry is a non-solution to a non-problem: the large weak scale hierarchy is already understood anthropically, and supersymmetry by itself does not even explain it.

If there is low energy supersymmetry is realized in nature, the upcoming LHC experiments should find the superpartners. This is the favorite scenario of many theorists. Some will say that they have correctly predicted supersymmetry, and they would be right. But if it is found, we will have to ask why it is present in our universe. Surely the fine-tuning argument is inadequate. Anyone with a basic knowledge of quantum mechanics should have realized that this argument is not even correct: we can only measure the sum of amplitudes, and it makes no sense to give significance to individual terms in the sum. If supersymmetry is discovered, it was predicted for the wrong reason, and we will have to find a better one. A large landscape may ultimately provide a rationale for the fine-tuning argument. Perhaps supersymmetric vacua dominate the ensemble of Standard-Model-like string vacua with small values for the weak scale, because it is "easier" to get a small value in supersymmetric vacua? It is not as simple as that. One has to deal at the same time with the fine-tuning of the cosmological constant. Low energy supersymmetry also helps with that, so that one might think that it is even more beneficial to have it. But some simple statistical models suggest that in fact supersymmetry breaking at a *high* scale may dominate the statistics, simply because they are more numerous, allowing them to overcome their disadvantage. It is not yet clear whether that is the correct conclusion for the full string theory landscape. For a longer discussion of this issue and further references see [60]. As these authors point out, if the correct conclusion is that a high supersymmetry breaking scale is statistically favored, *"the discovery of TeV scale supersymmetry would in*

some sense be evidence against string theory". It is noteworthy that many string theorists see the discovery of TeV scale supersymmetry as evidence *in favor* of string theory.

With the start of the LHC just months away (at least, I hope so), this is more or less the last moment to make a prediction. Will low energy supersymmetry be found or not? I am convinced that without the strange coincidence of the gauge coupling convergence, many people (including myself) would bet against it. It just seems to have been hiding itself too well, and it creates the need for new fine-tunings that are not even anthropic (and hence more serious than the one supersymmetry is supposed to solve).

But even if evidence for low energy supersymmetry emerges at the LHC, in the context of a landscape it will not be the explanation for the smallness of the weak scale. The explanation will in any case be anthropic. The landscape will undoubtedly allow a distribution of values for the weak scale, including values outside the anthropic window.

Features that are not observed

Most string phenomenologists seem to focus exclusively on one point: finding an (approximate) string vacuum that resembles the Standard Model as closely as possible. Of course this is important: if the Standard Model is not present in the string landscape, the string landscape is wrong. However, as I emphasized earlier, there is another important question: why does some feature appear abundantly in some sample, whereas it is not observed in out universe.

B-L

A good example is $B - L$. In many cases (namely if there are right-handed neutrinos), it turns out to be possible to add an extra gauge boson to the Standard Model that couples to the difference between Baryon and Lepton number (for technical reasons, "chiral anomalies", it cannot be any other linear combination of B and L). In the Standard Model one has the option to add such a gauge boson or omit it. In string theory, one cannot choose: one gets such an additional massless vector boson, or one does not. Most phenomenologists simply reject the cases with and extra massless $B - L$ vector boson without asking further questions. In a scan of part of the string theory landscape we found that in only 3% of all the cases this massless boson was absent [56]. In such a situation it seems natural to ask: why do we live in such a relatively rare universe?

In this particular case there is a plausible answer. An additional vector boson coupling to $B - L$ would change chemistry in a way that is hard to analyze. It would couple to neutrinos, which already by itself would have drastic consequences for neutrino physics. If neutrinos had such a coupling, this would forbid Majorana masses, and hence make it unlikely that they are light, which in its turn has serious consequences for neutrino processes. All of this makes it already clear that an additional massless $B - L$ photon would throw a spanner in the finely tuned works of our universe, but one could still ask whether this would inhibit *any* kind of life. Perhaps the most convincing argument is that $B - L$ photons would produce an additional repulsive force in nuclei, between protons as

well as neutrons. In the string theory examples with a $B - L$ photon the strength of its coupling is comparable to that of the normal photon. This would therefore very seriously affect the stability of heavier nuclei, including Carbon. It is unlikely that any interesting chemistry would result.

Whereas in this case it is rather easy to find plausible anthropic arguments against a Standard Model alternative, this is not always true. Of course one can always claim that perhaps we just live in an unlikely universe, but if one has to appeal to such an argument repeatedly it becomes harder and harder to believe that we really live in a point in the string theory landscape.

Exotics

By exotics we mean in general particles that occur in a given string spectrum, but not in the Standard Model. It is convenient to exclude particles that do not couple to any of the Standard Model vector bosons from the "exotics" category. Such particles ("Standard Model singlets") can only be observed through their gravitational interactions, and it is quite likely that they exist in our universe in the form of "dark matter". Furthermore, most string spectra that have been computed so far are supersymmetric, and hence all particles have a massless supersymmetric partner – a boson for each fermion and a fermion for each boson. Since they always occur in supersymmetric spectra, it is not convenient to label them as "exotics".

If one imposes the requirement that the Standard Model with its three families is contained in a spectrum, one finds essentially always, in addition to the quarks and leptons, large numbers of exotics. We may distinguish two kinds of exotics, chiral and non-chiral exotics. Roughly speaking, non-chiral exotics are particles that can acquire a mass without violating any gauge symmetries, whereas chiral ones can only become massive after a gauge symmetry has been broken. Since exotics, by definition, have not (yet) been seen, we must assume that when we move from the supersymmetric plane to our valley of the landscape, these particles become sufficiently massive. Clearly, chiral exotics are then a bigger worry than non-chiral ones.

The word "chiral" comes from the Greek word for "hand", and refers to the projection of the spin of a half-integer spin fermion on its direction of motion: a particle is left-handed if its spin points in the direction of motion, and right-handed if it points in the opposite direction. If a particle is massive we can overtake it by boosting ourselves to a velocity that exceeds the velocity of the particle. Then the relative velocity of the particle reverses, while the spin continues to point in the same direction: it flips from left-handed to right-handed, or vice versa. Since we merely changed the Lorentz frame, it follows that there cannot be an essential difference between the left- and right-handed particle: in particular, they must have the same couplings to all vector bosons. No such requirement exists for massless particles, because we cannot overtake them. Hence the left- and right-handed components may have distinct interactions; indeed, it is not even necessary that both exist.

Since we compute exact string spectra for massless particles, we can invert the ar-

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gument: particles which exist both in left- and right-handed varieties can, in principle, acquire a mass. These particles are called *non-chiral*, because they have no preferred handedness. On the other hand, particles whose putative left- and right-handed components couple in different ways to vector bosons cannot become massive, unless we remove the offending vector boson from the low energy spectrum by giving it a mass. These particles are called chiral. The process of giving a mass to a vector boson amounts to breaking the gauge symmetry corresponding to that vector boson.

It is usually assumed that non-chiral exotics appear in massless string spectra only by accident. They could become massive if we just move around in the supersymmetric plane by changing the moduli. However, when we compute string spectra we do not land on a generic point in the moduli space, but in special points that are computationally accessible. This accessibility is related to the existence of symmetries in the string description, and it is believed that these symmetries are responsible for the extra, non-chiral massless states. So the idea is that the explicit computations we are able to do are too special to correctly represent the generic situation. There is some evidence for this point of view, but there is also counter-evidence in certain examples: particles that remain massless even if one changes the moduli. The latter phenomena are sometimes referred to as "string miracles", because these are particles that one would not expect to be exactly massless outside the context of string theory. If non-chiral exotics are generically massive outside special points, then they are only a minor problem. The fact that we encounter them so frequently would merely be an artifact of the computational techniques at our disposal. But if this turns out to be false, we have to ask ourselves why the Standard Model spectrum as we observe it is so much cleaner (*i.e.* completely free of exotics that do not fit in one of he three families) as what we typically get out of string theory. Again the word "generic" appears here, and the same remarks apply as I made earlier regarding neutrino masses: if everything in string theory is generic, there is little left to recognize its specific features.

Chiral exotics are a more serious problem. They are usually avoided by simply rejecting spectra that contain them, since it is not very likely that such particles exist. Phenomenologically that is reasonable. All quarks and leptons we have observed so far are chiral with respect to the $SU(2)$ and $U(1)$ groups of the Standard Model. If there were any other matter that is chiral with respect to the Standard Model gauge group, it seems reasonable that we should have seen it already. If such matter can become massive, it needs the Higgs mechanism to do so, and then the mass would be of the same order as the quark and lepton masses. The top quark mass (174 GeV) is just about the maximum acceptable value for such matter. If nature is holding additional chiral matter up its sleeve, then the entire exercise of computing string spectra and comparing their chiral matter with observations becomes essentially pointless.

If the entire gauge group is just the Standard Model group $SU(3) \times SU(2) \times U(1)$ then there are not that many possibilities for additional chiral matter. One may add a fourth family, but that is essentially ruled out experimentally. But if the gauge group of nature contains additional components, there are numerous possibilities. This is true for open string realizations of the Standard Model, but it may be true for the closed

string realizations as well. If we insist on getting the observed three families of quarks and leptons, but do not restrict the spectrum in other ways, the results are dominated by spectra that contain extra chiral exotics. This extra matter couples to Standard Model vector bosons, in particular the photon, and often has half-integer charge. In addition it is charged under some additional gauge symmetry that is not part of the Standard Model.

Sometimes people admit spectra with chiral exotics out of desperation: without enough statistics this is often the only way to get something reasonably close to the Standard Model. People who do have a large enough sample can afford themselves the luxury to simply eliminate spectra with chiral exotics from their database. There will still be plenty of others left. But, once again, nobody asks the question why we do not see any evidence for these particles in nature if they occur so abundantly in string theory.

There are a few possible answers. It could be that this problem occurs mainly in open string realizations (which is what my own experience is based on). Perhaps open string realizations are simply sub-dominant in the full string theory landscape. Perhaps the statistics in the supersymmetric cases that have been studied is not representative for the actual landscape. In some specific cases one can invent dynamical mechanisms that break the gauge symmetries that prohibit a mass. Perhaps these particles then simply become massive after all, or occur only in massive bound states. The trouble is that, unlike the case of non-chiral exotics, this does not work "generically", but only in special cases. Chiral exotics that do not get a mass at all would certainly have anthropic implications: since they couple to Standard Model forces, they would get involved in chemical and nuclear bound states. But it would be hard to argue that this would make life impossible; it simply adds extra uncontrollable complications. At this moment I do not have a plausible argument why we do not see such exotics in nature. The best thing that could happen is that the LHC provides experimental evidence for such particles, having acquired just enough mass to have escaped detection so far.

6.5   Misguiding principles?

For the last thirty years, attempts to understand what might exist beyond the Standard Model have been guided by four basic principles:

- Consistency of the theory

- Agreement with experiment

- Naturalness

- Beauty, elegance, simplicity

The first two guiding principles are unquestionable, but the last two may well turn out to be misguiding principles.

The issue of "naturalness" arises in cases where dimensionless numbers are small. The principle of naturalness says that one would not simply expect such a number to come out of a fundamental theory as number of order one that just "happens" to be small, but

that there should exist some underlying mechanism that explains its smallness. The most serious naturalness problem is the smallness of the cosmological constant by 120 orders of magnitude. In the string theory landscape this is solved by having a distribution of values over a wide range, including the anthropic window. This brutally violates the idea of naturalness, and one should be prepared for more examples whenever naturalness meets the anthropic principle. The next frontier is the hierarchy between the weak scale of 100 GeV and the Planck scale of 10^{19} GeV, as discussed above.

The fourth guiding principle is also of dubious value. In the string theory landscape, there is not much reason the expect the point in the Gauge Theory Plane where we live to be especially beautiful or simple if there are 10^{500} other competitors in the beauty contest. Perhaps string theory itself can be called beautiful, or perhaps a truly elegant description will be found in the future, but that has nothing to do with beauty of its ground states. Nevertheless, within the string community it is standard practice to distinguish string vacua on the basic of esthetic criteria. Perhaps arguments *can* be found to justify that, but it would have to involve some knowledge about the distribution of vacua in the landscape. Perhaps certain "esthetic" features are simply more common.

It is also imaginable that in typical string vacua most matter acquires large masses, and that the Standard Model, with many light particles is very atypical. In that case, one would expect that the only those particles are light that are required for our existence. Having any additional light particles would simply lower the statistics without any benefits. This would indeed imply a certain degree of simplicity. Of course the Standard model seems to contain lots of inessential particles, but one can only remove an entire family at once.

6.6   Natural selection?

If there is a huge landscape, did we end up in our place just be chance after a humongous number of tries, or is there a better way? An interesting idea put forward by Lee Smolin [61] makes an analogy with the theory of evolution. An essential feature of the multiverse is some process of eternal creation of new universes. The currently most popular process is eternal inflation, in which bubbles of new universes are formed by quantum tunneling. Smolin considered the possibility that new universes are created in the interior of black holes. This idea has been criticized, but let us not focus on a concrete mechanism, but on the idea of natural selection itself. Regardless of the precise mechanism, one could contemplate the possibility that a child universe inherits some property from its parent. If that property furthermore enhances the number of child universes, then it is to be expected that in the total sample universes with that property will dominate. This would be the analog of natural selection in evolution. In evolution, there is also an analog of the landscape of possible universes. It is the entire set of all possible biochemical molecules and combinations thereof, including all possible forms of life. It is clear that in that biological landscape natural selection is absolutely essential in order to understand how we managed to reach the complexity of life on our planet in a few billion years.

All though I have some sympathy for this idea, Smolin proposed it as a remedy against

the anthropic principle. Perhaps that is correct for some forms of that principle, but certainly not for the point of view I have been advocating in this article: the likely existence of a huge landscape. Clearly natural selection can only work if such a landscape exists in the first place, just as biological evolution can only work if there is a biological landscape. None of the arguments I gave in chapter (3) are invalidated if there is a natural selection process that guides us to our valley exponentially faster than a random process.

Another objection is that creation of universes is necessarily a process on a cosmic scale, and it seems preposterous that such a process should care about the minor chemical accident we call life. In the concrete case of Smolin's proposal, there is one important ingredient for our kind of life that plays an important rôle, namely the formation of stars and the collapse of some of them into black holes. But these are mainly nuclear processes, and therefore I would expect universes to exist where black holes are formed, but the chemistry for life is just not available. In that case we would still have to appeal to the anthropic principle to explain why we live in this particular universe, within the set of those with abundant stars or black holes. In other words, cosmic natural selection, if it can be realized at all, might produce a large peak in the distribution somewhere in a large neighbourhood around the anthropic region, but it would be miracle if it were to coincide with it. It just might guide us a little faster to the right region, but it can never pinpoint the right spot.

An essential ingredient of such a process is that small changes in parameter values take place at the birth of a new universe. Exactly as in the case of evolution, large changes don't work; they would randomize the process. However, in the string theory landscape, an extremely small change in a Standard Model parameter implies a huge change in the cosmological constant. This may be compensated by a discrete change of the Bousso-Polchinski fluxes, but that would put us in an entirely different part of the landscape. In other words, points in the Gauge Theory Plane that are close to each other are generally very far from each other in the full string theory landscape. Therefore natural selection by small "mutations" of universes seems impossible in the string theory landscape.

There is another important, and somewhat disturbing, difference with biological evolution, namely the absence of an obvious time interval required to reach our universe. The adjective of "eternal inflation" says it all. What is worrisome about this is that "eternal" implies an infinity, and infinities can have nasty consequences. In this case they convert any option, no matter how improbable, to a real possibility. This even tends to undermine the argument that biological natural selection is needed to explain how the complexity of live could emerge in such a short time. With enough universes to try, in one of them it may have worked all in one step! This raises the question why had to pass through the complicated process of evolution if it could have happened so much more easily? The answer to questions like these requires a proper understanding of the thorny issue of probabilities, and may well be beyond the edge of our current knowledge.

7 Conclusions

One of my main points is nicely summarized by the following version of Hans Christian Andersen's fairy tale:

Many years ago, there lived some physicists who cared much about the uniqueness of their theories. One day they heard from about a beautiful theory, which was absolutely unique. They were told that only people stupid enough to be open-minded about the anthropic principle would not be able to see that.

Of course, everyone wildly praised the magnificent unique theory, afraid to admit that it had "anthropic principle" written all over it, until Susskind shouted:

"String Theory has an anthropic landscape"

Then some people claimed to have observed this already.

7.1 Will we ever know?

As I have emphasized repeatedly, the use of anthropic reasoning here is not the simple statement that some property of our universe could be different, but involves a fundamental theory that specifies precisely *how* it could be different. Although those differences are presumably realized in some other part of a multiverse, it is not likely that we can check that. Perhaps it might be possible to do that *in principle*; it has been suggested that information about ancestors of our own universe might be encoded in the microwave background radiation, in the same way as black hole radiation is now believed to contain the information that went in it; it has also been suggested that signals of other parts of the multiverse might be visible in the future (which of course is measured in billions of years) when our horizon has expanded sufficiently; there might even be a dramatic confirmation when our universe tunnels to another minimum in the landscape. But in all these cases *in principle* really means *in principle*.

This is not an acceptable state of affairs, if indeed it is the final word. But it is a completely acceptable outcome if we can establish the correctness of string theory (or some other fundamental theory) beyond any reasonable doubt. During the last two decades there was some reason to hope that we might be able to do that my means of some prediction of a Standard Model feature. That hope is fading now. I am not saying that this will never happen, but I have seen too much wishful thinking to make an optimistic statement about this. Essentially, we came to that conclusion already in 1986 [4]. We are dealing with a theory of gravity. Getting information about it through the back door of particle physics is a luxury that we once had good reasons to hope for, but that may not exist. Rejecting a theory of gravity that makes no particle physics prediction may be like rejecting the theory of continental drift because it does not predict the shape of Mount Everest.

One cannot count on any direct experimental check of a theory of quantum gravity, since any observable consequences it might have are extremely small, unless we are extremely lucky. Indeed, when 't Hooft en Veltman tackled Quantum Gravity in 1974 [64] they wrote about their motivation: *"One may ask why one would be interested in quan-*

tum gravity. The foremost reason is that gravitation undeniable exists. [....] Mainly, we consider the present work as a kind of finger exercise without really any further underlying motive". The problem of quantizing gravity is a problem of consistency, which can be checked by theoretical reasoning. Perhaps that is indeed the only way. As I have indicated in chapter (5), in string theory there is still a long way to go before success can be claimed on that point. The best imaginable outcome would be a proof that quantum gravity *requires* string theory; on the other hand, it is possible that we end up with a huge ensemble of unrelated theories of quantum gravity, each with its own landscape, and without any way of distinguishing between them.

Although the Standard Model may not provide us with a *verification* of string theory, it might easily have falsified string theory in many ways. It might have been impossible to get chiral fermions, the Standard Model gauge group, quarks and leptons, the right number of families or non-trivial couplings. All of these could have easily gone wrong, but did not. For comparison, if instead of the Standard Model we would have tried to get the Periodic System directly out of string theory (treating nuclei as fundamental particles), we would have failed convincingly (see appendix A). A failure to stabilize the moduli, or to obtain a small, but non-zero cosmological constant would also eventually have led to abandoning the whole idea. It is not hard to imagine future experimental results that would cast serious doubts on the entire landscape idea. If the upcoming LHC results point towards new strong interactions around the TeV scale, we have to revise the entire extrapolation of the Standard Model to the Planck scale; the same is true if the Higgs boson weighs more than 180 GeV. If there is no fundamental Higgs scalar at all, we might feel less confident about the rôle of fundamental scalars as moduli, and we certainly have to rethink the Standard Model itself, before even attempting to embed it in string theory. New particles with unusual charges or representations could make such an embedding essentially impossible.

There is yet another, extremely optimistic possibility for the LHC, namely that it might find direct evidence for string theory. This could happen if our naive estimates for the Planck scale are wrong. It could in fact be directly accessible for LHC experiments if there are extra dimensions that are large [65] [66] or warped [67]. In the first of these ideas the weakness of gravity (or in other words the smallness of the weak scale) is explained because at short distances of order .1 mm or smaller (but much larger than the Planck length) space-time has more than four dimensions, allowing the field lines of gravity to get rapidly diluted. If we are really lucky, we might observe excited states of string theory and even produce black holes. However, although the idea of large extra dimensions found its origin in string theory, it is not clear if it is a really a dominant feature of the string theory landscape, or at least that part of the landscape that can accommodate a small weak scale. Studies of different classes of string theories have led to opposite conclusions on this issue [68] [69].

There is an impressive experimental effort going on in the area of astro-physics, which could modify our current ideas about gravity and the cosmos. If any deviations from General Relativity are observed, this would have profound repercussions on our current understanding of string theory. A very important constraint is the possible variation

of constants of nature. If it is found that some of them are different in some part of the visible universe, this would imply that this part of the universe should have a very different vacuum energy, and hence should have collapsed or rapidly expanded [70]. If we observe such a variation experimentally, then either its contribution to the vacuum energy is compensated by another, unobservable variation, or our entire understanding of vacuum energy and its relation with the cosmological constant is wrong. In the former case, the landscape has a valley along which physics can change without a change in vacuum energy, which would imply the existence of a (nearly) massless scalar field with significant couplings to observable quantities. In the latter case, a basic principle behind of the string landscape idea would have been falsified. This may already be the case: in a recent paper [71] the observation of a small time variation of the proton/electron mass ratio (of order 10^{-5} over 12 billion years) is claimed. If correct, this would be very hard to reconcile with the idea that the cosmological constant is fine-tuned with 120 digit precision, and therefore extremely sensitive to even the smallest imaginable variations.

Most of these results would wreak havoc in other areas than just string theory, and one cannot rule out the possibility that some reformulation or renewed understanding of string theory might revive it. But in any case it underscores the fact that string theory is not immune to falsification, and that it is in fact a small miracle that it has not been falsified already. Indeed, it is truly remarkable that something like the Bousso-Polchinski mechanism exists to avoid a falsification by the cosmological constant. In string theory, this is not something one can engineer: it is either already there, or it is not. I find it difficult to believe that a wrong theory would produce such spectacularly correct results.

The foregoing is just a small selection of observations that would change our point of view completely and that might occur in the near future. On longer timescales, it is clearly ridiculous to pretend that what we currently know will be the state of the art forever. When Darwin formulated his theory of evolution he was unaware of Mendel's results on inheritance, and could not even have imagined DNA. It may well be that these are the kind of time-scales we have to think about, but I am convinced that eventually mankind will gather enough information to arrive at a definitive conclusion.

7.2 Final remarks

Finally, after 15 years the debate has started that should have started around the mid-eighties of last century, but was stifled by irrational opposition against the notion that our observation of the Standard Model could be biased by our own existence. To me at least one thing seems absolutely obvious: the idea that the Standard Model is (even approximately) unique will eventually find its place in history next to Kepler's attempt to compute the orbits in the solar system: understandable at its time, but terribly anthropocentric when properly thought about.

Far from "giving up", within the context of string theory and its landscape our new mission is exactly the opposite. We have to understand the landscape, find our place in it, and determine if it is really plausible that we find ourselves in that particular place and not elsewhere. Despite the huge size of the landscape, it is not guaranteed that the

Standard Model is in it. Because of the huge size, there is a risk that within the ensemble of universes that allow life, ours is incomprehensibly rare.

Every theory of quantum gravity has a landscape: the matter it can couple to. There are other ideas for quantizing gravity, but the reason I called string theory the "only candidate" earlier in the article is that it is the only approach that is sufficiently advanced to provide us some insight into its landscape. It would be interesting to know what alternatives other approaches have to offer. I think that a unique solution cannot be expected in any approach, and that a continuous infinity distributed over the Gauge Theory Plane would not be the right answer.

I have presented string theory here as concept that is still very much under construction, but which makes a very clear suggestion about what a "final theory" might look like. To me, what is emerging looks very appealing. It fulfills and even exceeds the hopes I expressed in 1998. It is has been amazing to see this theory leading us in the right direction, sometimes even against the initial expectations of most of the people working on it. We should continue to follow its lead, and do everything in our power to strengthen its theoretical underpinnings. The emergence of a huge "landscape" makes this more worthwhile then ever before.

String was expected to answer some of the big questions in science. Indeed it does. What bigger questions can a scientist dream of than those concerning our own existence? We got an answer to one such question, but unfortunately not everyone is ready for it. One can easily justify the past two and a half decades of research in string theory by the theoretical and mathematical spin-offs alone, but I think we are on our way to something much bigger than that.

But will this road really lead us to the "final theory"? Was this the last time we were mesmerized by misguided ideas about our special place in the multiverse? Was this the last time the emperor turned out to have no clothes? Could string theory just turn out to be the first item in a huge ensemble of theories with similar properties? I have no answers to these questions. History does tend to repeat itself, but unfortunately not in a predictable way.

Acknowledgements

A The Standard Model versus the Periodic System

I have presented the Standard Model as a high precision theory with clearly defined parameters, a property not shared by the old theories of nuclear and hadronic physics. Based on this fact, and on the apparent anthropic coincidences in the Gauge Theory Plane, I have argued that it is plausible that a Landscape of possibilities should survive in any more fundamental theory. Such an argument would clearly be meaningless when applied to the space of nuclear or hadronic theories, which we cannot even define. There is, however, one predecessor of the Standard Model that might qualify as an alternative, namely the Periodic System of Elements.

One often hears the statement that physics would never have progressed if people had surrendered to the anthropic principle too soon. So let us do a historic thought experiment. Let us imagine that string theory and gauge theory had reached its current state many decades earlier, as a mathematical theory. Is it conceivable that someone would have tried to apply it to understand the Periodic System, the Standard Model of their time, and arrive at incorrect anthropic conclusions?

By "Periodic System" I mean here concretely a theory with nuclei treated as point-like elementary particles, with an electron, and with only electromagnetic interactions. This is a gauge theory, so it is in fact a point in the Gauge Theory Plane.

In order to do this though experiment we have to assume that the physicists of that time were foolish enough to overlook some obvious problems, such as the fact that the nuclei are not stable, that we need to understand their abundances, and that this purely electromagnetic theory does not explain how the sun works. This alone would be enough to answer my rhetorical question. Perhaps there are problems today that I am foolishly overlooking as well, something that so obviously requires entirely new physics that it is premature to even consider a fundamental theory. I am taking that risk.

So what can be varied in the Periodic System, treated as a point in the Gauge Theory plane? Obviously this includes the electron mass, the strength of the electromagnetic coupling and the overall mass scale of all the nuclei. These are less strongly anthropically constrained than in the case of the Standard Model, because we decided not to consider abundances, and because there is no competition between the strong and electromagnetic interactions. But anyway, any anthropic considerations for these parameters are contained in those of the Standard Model, and hence would not be premature, if the latter are correct.

This leaves us with the charges and the nuclear mass ratios. The charges are sometimes mentioned as a candidate for anthropic consideration: if the electron charge is not equal to the proton charge with enormous precision, atoms would have a net charge and we would not exist. This is an excellent example of misuse of the anthropic principle for at least three reasons.

First of all, it is not natural to assume that these charges are variables. It is true that the nuclear charges can be treated as true variables in the description of the Periodic System. However, anyone would immediately have noticed two facts: that the charges are integers within experimental precision, and that atomic physics does not change smoothly

if the charges are allowed to vary over the real numbers: one cannot interpolate smoothly between a hydrogen and a helium atom, for example. Hence the most natural assumption would be that they belong to the set of integers, not to the set of real numbers. Indeed, it is unimaginable that someone would propose a dense landscape of Periodic Systems with arbitrary real charges for the nuclei, just to explain why all their observed charges are indistinguishable from integers.

The second fallacy is the "anthropocentric trap". Whether *we* would exist if the charges were non-integer is irrelevant. One would have to show that nothing of comparable intellect can exist. It is beyond our knowledge of atomic and molecular physics to conclude that nothing of equal complexity could exist if we change some of the charges.

The third argument can only be made with current insights, and is relevant because the integer charge anthropic argument is still heard from time to time. In the Standard Model we already know that the proton charge is the exact opposite of the electron charge with absolute precision, because the consistency of the theory demands that. I am referring to "chiral anomalies", which are cubic sums of charges that must vanish exactly. Although there are several ways of satisfying them, changing for example the electron charge while keeping everything else fixed is not an option. These anomaly constraints are only relevant if left- and right-handed fermions have different couplings, as is the case for the weak interactions. Hence this idea would not have occurred to a physicist studying only the periodic system. However, I was assuming that String Theory and gauge theory had already been developed theoretically, and anyone with knowledge of these theories would have understood that a fundamental theory explaining charge quantization was entirely feasible.

The other parameters to be considered are the nuclear masses. With enough experimental precision these would have been distinguishable from integers, because of binding energies and isotope averaging. But even if they would have been treated as free parameters, they have little anthropic relevance. Again, what matters is not whether we would exist, but if anything of comparable complexity would exist. The complexity of atomic and molecular physics is determined almost entirely by the charges. Indeed, even if we multiply the mass of the hydrogen by two, we get heavy water, a substance with physical and chemical properties very similar to normal water. So it seems plausible that chemistry of comparable complexity will exist over a wide range of the Periodic System plane, if we vary the masses. So no incorrect anthropic conclusions are possible here.

The imaginary string theorists in the Periodic System epoch would encounter serious problems if they tried to do what present-day string theorists are attempting: obtaining their low-energy theory from string theory. The first problem is that it is in fact not true that the Periodic System extrapolates to the Planck scale. The reason is that the Landau pole of electrodynamics in such a theory occurs at energies well below the Planck scale. The coefficient b_0 in eqn (2) receives contributions from all elementary particles, and these contributions are proportional to the square of the charge. The assumption we were making about the Periodic System is that all nuclei (*i.e.* all those observed) are elementary. But then they will all make separate contributions to the quantum corrections that make the gauge coupling increase. The contribution of Hydrogen en Helium alone is

already equal to that of all three quarks of charges $\frac{2}{3}$ and three of charge $\frac{1}{3}$; by the time we have included all nuclei up to Carbon the Landau pole has already moved below the Planck scale.

Even if one ignores that, it would be practically impossible to obtain a set of particles from string theory with charges ranging from 1 to, for example, 20, in sharp contrast to getting the gross features of the Standard Model, which come out easily.

B Grand Unification, little uniqueness

Grand Unification is the unification of the three known non-gravitational forces into one a gauge theory based on a single gauge group. It is sometimes mentioned as a sign of uniqueness of the Standard Model, or at least as the origin of a misguided expectation of uniqueness.

Grand Unification is based on two miraculous facts. The Standard Model group and representations fit nicely in a larger group, and the three Standard Model couplings, when extrapolated to higher energies, seem to meet at a common point at an energy just below the Planck scale. The last miracle requires another speculation: supersymmetry. The couplings only merge if we assume that every Standard Model boson/fermion has a hitherto unobserved fermionic/bosonic counterpart[19], contributing to the coefficients b_0 in (2). These "super-partners" must have a mass of at least a few hundred times the proton mass to have escaped detection up to now. Furthermore, the normalization of the $U(1)$ coupling constant must be modified by a (theoretically motivated) factor $\sqrt{3/5}$ in order for this to work.

The merging of coupling constants may just be a coincidence. The chance that it is, based on current experimental errors, is about one percent. In other words, if we assume that the value of the weak coupling varies over the multiverse and has no anthropic relevance, the inhabitants of about one in a hundred universes would be puzzled by this coincidence. Fortunately we will not be puzzled forever. If the necessary superpartners exist they must be found at the LHC. If the apparent merging of the couplings survives the LHC results and becomes even more accurate, it will be hard to argue that it is a coincidence.

The second grand unification miracle is that the group $SU(3) \times SU(2) \times U(1)$ fits exactly in the group $SU(5)$, in such a way that one family (see Eqn.(1)) fits precisely in the representation $(\bar{\mathbf{5}}) + (\mathbf{10})$ of $SU(5)$. One can go one step further and embed $SU(5)$ into $SO(10)$. Then one family (including a right-handed neutrino) fits into a single representation $(\mathbf{16})$ of $SO(10)$.

Even readers who are not familiar with the group-theoretical details will understand the $(\mathbf{16})$ looks a lot simpler than Eqn. (1). In fact, there *is* something mathematically unique about the $(\mathbf{16})$ of $SO(10)$: it is the smallest complex irreducible cubic-anomaly-free representation of any Lie group (please ignore the precise meaning of this mathematical jargon). Doesn't this point towards uniqueness of the Standard Model? Not really. First

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[19]Plus an additional Higgs scalar and the fermionic partners of the two Higgs scalars.

of all, families are repeated three times, so that we really have a representation $3 \times (\mathbf{16})$. This ruins both the "irreducible" and the smallest in the statement. Secondly, there are other particles in these models: Higgs particles needed to break symmetries. They must in strange representations like $(\mathbf{45})$, $(\mathbf{54})$ or $(\mathbf{126})$. There is nothing mathematically unique about these. In string theory, the symmetries may sometimes be broken in a different way, but certainly not more uniquely.

But there is a more serious problem with the concept of uniqueness here. The groups $SU(5)$ and $SO(10)$ also have other subgroups beside $SU(3) \times SU(2) \times U(1)$. In other words, after climbing out of our own valley and reaching the hilltop of $SU(5)$, we discover another road leading down into a different valley (which may or may not be inhabitable). When I was confronted with this around 1977, it started my doubts about the uniqueness of the Standard Model. I studied the parameters that determine the valleys, and tried to find values such that ours was the lowest one. This was possible, but I understood that I was just fooling myself: this just shifted the problem into the choice of parameter values. The thought also occurred to me that not only the choice of gauge group, but also the Standard Model parameters might be fixed at the start of the universe as a sort of boundary condition, and could not be computed from first principles. I am not sure if I attached any anthropic implications to that at the time, but I remember being deeply troubled by this.

In other words, people who associate Grand Unification with the idea of uniqueness are simply confusing the two concepts.

The story continues. Even if there is something conceptually nice about the fact one family of the Standard Model fits into the $(\mathbf{16})$ of $SO(10)$, it still is a choice out of many possibilities. Is there a natural way of realizing this in a fundamental theory of physics? Indeed, there is a fundamental theory that naturally realizes this: string theory, more precisely the heterotic string. This theory has a gauge group $E_8 \times E_8$ in ten dimensions. The group E_8 contains $SO(16)$ as a maximal subgroup (the other E_8 is not used), and $SO(16)$ has a natural decomposition into $SO(6) \times SO(10)$. There is also a natural reason to split it in precisely this way: The $SO(6)$ is the rotation group in the six extra dimensions.[20] In this way we do indeed get $SO(10)$, but not necessarily with three families. The number of families depends on the topology of the compactification manifold, and there is a huge number of possibilities.

However, even though the appearance of $SO(10)$ and the family structure is rather natural in Heterotic string theory, it is by no means unique, and this gets worse if one considers further breaking of $SO(10)$ to the Standard Model. In landscape terminology: if we continue climbing from the $SU(5)$ hill to $SO(10)$ and finally reach the summit, $E_8 \times E_8$ Heterotic string theory, we find that there is a humongous number of other trails leading to other valleys than the one we came from. And higher up, partly in the clouds, we see the outline of a still higher and rather mysterious summit, called M-theory, from which even more paths descend down into even more different valleys.

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[20]This can be made precise most easily in the supersymmetric case: E_8 decomposes to $SU(3) \times E_6$, and $SU(3)$ is the holonomy group of a Calabi-Yau manifold. The group $E(6)$ contains $SO(10)$.

C Dimensional transmutation and the measure

The Standard Model has two mass scales: The QCD scale and the weak scale. The existence of two small scales implies two problems: why is the QCD scale about twenty orders of magnitude smaller than the Planck scale, and why is the weak scale seventeen order of magnitude smaller than the Planck scale? Both of these small ratios have anthropic implications.

The second of these problems is often referred to as the gauge hierarchy problem, and there is a huge number of papers addressing it. The first problem, on the other hand, is often regarded as "solved", and this is used as a counter argument against anthropic reasoning.

The argument goes as follows [72]. Dirac already knew that the ratio of the proton mass and the Planck mass could be regarded as anthropic, but to his credit – the opponents of anthropic reasoning say – he did not use that argument to explain it. Instead he proposed that this ratio could be time dependent, and become smaller and smaller as time goes by. The large lifetime of the universe would then supply the large number needed to explain the small ratio. While this was a nice idea at the time, it was of course wrong, but according to opponents of anthropic arguments even a wrong idea is better than invoking the anthropic principle. In my opinion, it would have been equally reasonable for Dirac to have said: "maybe in a more fundamental theory we will understand that the proton mass has many possible values, and we see a proton with a small mass because otherwise we would not exist to observe it". The words "in a more fundamental theory" are crucial. As I have already emphasized many times, one cannot simply assume that some parameter can take many values. There must exist a theory that tells us what the allowed variations are. This is of course what the string theory landscape provides us with.

Meanwhile we understand the proton mass in terms of the QCD scale, and asymptotic freedom of QCD seems to give a convincing, non-anthropic answer:

$$\frac{M_{\text{proton}}}{M_{\text{Planck}}} = e^{-\frac{c}{\alpha_s(M_{\text{Planck}})}} \tag{9}$$

This relation is obtained by inverting Eqn. (2). The proton mass is of the same order as the QCD scale, which is the mass scale where (2) has a pole. We can use (2) to express the position of the pole in terms of the value of the coupling constant at any scale, and we choose the Planck scale. This relation is sometimes referred to as "dimensional transmutation", since it seems to generate a dimensionful parameter (the QCD scale) out of a dimensionless number, α_s. Here α_s is the strong coupling constant, the analog of the fine structure constant of QED. It is a function of energy, which is evaluated here at the Planck energy (which is how the mass dimension enters into the equation). The coefficient c is a positive numerical constant of order 1. The ratio on the left hand side is small if the value of $\alpha_s(M_{\text{Planck}})$ is small, which of course it is. But that, by itself, cannot yet be called an explanation of the smallness of the ratio unless we can compute $\alpha_s(M_{\text{Planck}})$. What the equation actually does is turn a major miracle into a minor miracle: instead of having to understand why in our universe we measure a number as small as 10^{-19}, we now only have to understand why it is of order 10^{-1}.

Of course this does take the urgency out of the problem, but unless someone actually computes $\alpha_s(M_{\text{Planck}})$ from first principles, it is not the end of the story. If we end up with a fundamental theory that has a landscape, what one would expect to find is a distribution of possible values of $\alpha_s(M_{\text{Planck}})$. This distribution will undoubtedly have values outside the anthropic window, in which case we will once again have to resort to anthropic arguments to understand why we find ourselves *within* that window. Eqn. (9) *does* tell us something very important, namely that before we worry about the smallness of a parameter, we should worry first about the measure on the parameter space. But in a landscape picture it only *changes* the measure, it does not provide us with the definitive measure on the space of couplings. This we will only know once we know the actual distribution of the values of $\alpha_s(M_{\text{Planck}})$ in the landscape.

There might exist a way to improve this argument. Perhaps the smallness of $\alpha_s(M_{\text{Planck}})$ could be related to the smallness of the fine structure constant α, which is also a function of energy. This is indeed true in Grand Unified Theories (see appendix B), which at this moment is still a theoretical speculation awaiting experimental evidence. In those theories both functions are actually equal at some GUT mass scale M_{GUT}, below which the strong coupling increases and the QED coupling decreases with decreasing energy. This would indeed look like a counter indication to anthropic reasoning, because it relates two quantities with anthropic implications[21] However, note that the two quantities are determined in terms of two other parameters, the GUT mass scale and the value of the unified coupling at that scale. Mathematically speaking this does not imply a relation at all. In reality, it does imply a limitation of the allowed values, if one imposes the condition that M_{GUT} should be smaller than M_{Planck}. This would be a reasonable assumption if M_{Planck} is the largest possible mass scale in physics.

If the GUT picture survives future experiments, it is undeniably true that there is something remarkable about the fact that M_{GUT} is close to M_{Planck}. Had it been equal to M_{Planck}, one could claim a direct relation between the QCD scale and fine-structure constant. But it is *not* equal, and hence M_{GUT} is just a free parameter with a value that is remarkably close to the Planck scale.

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[21]Gauge coupling unification relates *three* couplings to two input parameters, M_{GUT} and the value of the unified coupling at that scale. The third one, the strength of the weak coupling, has far less severe anthropic implications, and hence is not likely to enhance the amount of anthropic coincidence by much.

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