Quantum Gravity: The view from particle physics

Hermann Nicolai
Max Planck Institute for Gravitational Physics
(Albert Einstein Institute)
Am Mühlenberg 1, 14476 Golm, Germany
E-mail: nicolai@aei.mpg.de

Abstract. This lecture reviews aspects of and prospects for progress towards a theory of quantum gravity from a particle physics perspective, also paying attention to recent findings of the LHC experiments at CERN.

1. Introduction

First of all I would like to thank Jiří Bičák for inviting me to this prestigious conference in commemoration of Einstein's stay in Prague a hundred years ago. Although it was only a short stay, as Einstein left Prague again after little more than one year, it was here that he made major progress towards the final version of General Relativity, and surely the beauty of this city must have played an important inspirational part in this endeavor.

In view of the more general nature of this conference, I have decided not to give a technical talk on my current work, but rather to present some thoughts on the state of quantum gravity from the point of view of a particle physicist, but with an audience of general relativists in mind. Taking such a point of view is quite appropriate, as LHC is about to end it first phase of experiments, with the solid evidence for a scalar boson that has all the requisite properties of a Higgs boson as the main outcome so far. This boson was the final missing link in the Standard Model of Particle Physics (or SM, for short), and therefore its discovery represents the final step in a story that has been unfolding for almost 50 years. Equally important, as the CERN experiments continue to confirm the Standard Model to ever higher precision, with (so far) no indications of 'new physics', it is also a good time to ask whether these results can possibly offer any insights into quantum gravity. So my main message will be that we should not ignore the hints from particle physics in our search for quantum gravity!

I do not think I need to tell you *why* a theory of quantum gravity is needed, as some of the key arguments were already reviewed in other talks at this conference. There is now ample evidence that both General Relativity (GR) and Quantum Field Theory (QFT) are incomplete theories, and both are expected to break down at sufficiently small distances. The generic occurrence of space-time singularities in GR is an unavoidable feature of the theory, indicating that classical concepts of space and time must be abandoned at distances of the order of the Planck scale. Likewise, there are indications of a breakdown of conventional QFT in this regime. Accordingly, and in line with the title of this lecture, I would therefore like to concentrate on *the lessons from particle physics* pointing beyond QFT and conventional concepts of space and time.

In its current incarnation, QFT mainly relies on perturbation theory. The ultraviolet (UV) divergences that inevitably appear in higher order Feynman diagrams require a carefully crafted procedure for their removal, if one is to arrive at testable predictions. This renormalization prescription in essence amounts to an order by order tuning of a finite number of parameters by infinite factors. Although mathematically on very shaky grounds, this procedure has produced results in stunning agreement with experimental findings, with a precision unmatched by any other scheme in the physical sciences. The most famous example is, of course, the QED prediction of the anomalous magnetic moment of the electron, but the agreement between very recent precision measurements at LHC and the theoretical predictions of the Standard Model is now equally impressive. Yet, in spite of this extraordinary success there is good reason to believe that neither the SM in its present form nor any of its quantum field theoretic extensions (such as the supersymmetric versions of the SM) are likely to exist in a strict mathematical sense. The ineluctable conclusion therefore seems to be that the UV completion of the SM requires something beyond QFT as we know it.

The difficulties in both GR and conventional QFT have a common origin. In both frameworks space-time is assumed to be a *continuum*, that is, a differentiable manifold. As a consequence, there should exist no obstacle of principle in going to arbitrarily small distances if either of these theories were universally valid. Nevertheless, the very nature of quantum mechanics suggests that its principles should ultimately also apply to space-time itself, whence one would expect the emergence of a grainy structure at the Planck scale. Indeed, and in spite of their disagreements, almost all approaches to quantum gravity[1] are united in their expectation that something dramatic must happen to space-time at Planck scale distances, where the continuum should thus give way to some kind of *discretuum*.

A second, and related, source of difficulties is the assumption that elementary particles are to be treated as *point-like* excitations. And indeed, there is not a shred of a hint so far that would point to an extended structure of the fundamental constituents of matter (quarks, leptons and gauge bosons), so this assumption seems well supported by experimental facts. Nevertheless, it is at the root of the ultraviolet infinities in QFT. Moreover, it is very hard to do away with, because the point-likeness of particles and their interactions seems to be required by both relativistic invariance and locality/causality – building a (quantum) theory of relativistic extended objects is not an easy task! In classical GR, the very notion of a point-particle is problematic as well, because any exactly point-like mass would have to be a mini black hole surrounded by a tiny horizon, and thus the putative point particle at the center would move on a space-like rather than a time-like trajectory. Again, one is led to the conclusion that these concepts must be replaced by more suitable ones in order to resolve the inconsistencies of GR and QFT.

Current approaches to quantum gravity can be roughly put into one of the two following categories (for a general overviews see e.g. [2, 3, 4][2]).

- According to the first hypothesis quantum gravity in essence is nothing but the *non-perturbative* quantization of Einstein Gravity (in metric/connection/loop or discrete formalism). Thus GR, suitably treated and eventually complemented by the Standard Model of Particle Physics or one of its possible extensions, should correctly describe the physical degrees of freedom also at the very smallest distances. The first attempt of quantizing gravity relied on canonical quantization, with the spatial metric components and their conjugate momenta as the canonical variables, and the Wheeler-DeWitt equation governing the dynamics [3]. Superimposing Schrödinger-type wave mechanics on classical GR, this scheme was still rather close to classical concepts of space and time. By contrast, modern versions of this approach look quite different, even though their starting point is still the standard Einstein-Hilbert action in four dimensions: for instance, the discrete structure that emerges from the loop quantum gravity program relies on holonomies and fluxes as the basic variables,

[1] With the possible exception of the Asymptotic Safety program [1].

[2] As there is a vast literature on this subject, I here take the liberty of citing only a few representative introductory texts, where more references can be found.

leading to a discretuum made of spin networks or spin foams [5, 6].

- According to the opposite hypothesis (most prominently represented by string theory [7, 8, 9]) GR is merely an effective (low energy) theory arising at large distances from a more fundamental Planck scale theory whose basic degrees of freedom and whose dynamics are very different from either GR or conventional QFT, and as yet unknown. In this view, classical geometry and space-time itself, as well as all matter degrees of freedom are assumed to be 'emergent', in analogy with the emergence of classical macroscopic physics from the completely different quantum world of atoms and molecules. Likewise, concepts such as general covariance and even background independence might only emerge in the large distance limit and not necessarily be features of the underlying theory. Consequently, attempts to unravel the quantum structure of space and time by directly quantizing Einstein's theory would seem as futile as trying to derive microscopic physics by applying canonical quantization procedures to, say, the Navier-Stokes equation. The fundamental reality might then be something like the abstract space of all conformal field theories, only a small subset of which would admit a geometrical interpretation. The occasional 'condensation' of a classical space-time out of this pre-geometrical framework would then appear as a rare event.

Pursuing different and independent ideas is certainly a good strategy as long as we do not know the final answer, but it is a bit worrisome (at least to me) that the proponents of the different approaches not only base their approaches on very different assumptions, but continue to speak languages that are foreign to one another. Surely, when zeroing in on the 'correct' theory there should be *a convergence of ideas and concepts:* when Schrödinger proposed wave mechanics and Heisenberg formulated matrix mechanics, these were initially regarded as very different, but it did not take long before it became clear that they were just equivalent descriptions of the *same* theory. Unfortunately, at this time there is no such convergence in existing approaches to quantum gravity – a sign that we are probably still very far from the correct answer! So let us hope that our noble search will not end like the historic event in the Breughel painting shown in Fig. 1.

2. The divergence problem

From the point of view of perturbative QFT the basic difference between gravity and matter interactions is the non-renormalizability of perturbatively treated GR. For instance, at two loops the Einstein-Hilbert action must be supplemented by the following counterterm cubic in the

Figure 1. The steady progress of Quantum Gravity?

Weyl tensor [10, 11] [3]

$$\Gamma^{(2)}_{div} = \frac{1}{\varepsilon}\frac{209}{2880}\frac{1}{(16\pi^2)^2}\int dV C_{\mu\nu\rho\sigma}C^{\rho\sigma\lambda\tau}C_{\lambda\tau}{}^{\mu\nu}\,, \tag{1}$$

if the calculations are to produce *finite* predictions for graviton scattering at this order (the parameter ε here is the deviation from four dimensions in dimensional regularization, and must be taken to zero at the end of the calculation). At higher orders there will arise similar infinities that likewise must be cancelled by counterterms of higher and higher order in the Riemann tensor. Because one thus has to introduce an unlimited number of counterterms in order to make predictions at arbitrary loop orders and therefore has to fix an infinite number of coupling constants, the theory looses all predictive power.

From the non-renormalizability of perturbatively quantized gravity, one can draw quite different conclusions, in particular reflecting the two

[3] There is no need here to distinguish between the Riemann tensor and the Weyl tensor, as all terms containing the Ricci scalar or the Ricci tensor can be absorbed into (possibly divergent) redefinitions of the metric.

opposite points of view cited above. According to the string/supergravity 'philosophy', a consistent quantization of gravity necessarily requires a modification of Einstein's theory at short distances, in order to cancel the infinities. This entails the necessity of (possibly supersymmetric) matter and in particular fermions, thus furnishing a possible *raison d'être* for the existence of matter in the world. It was originally thought that the UV finiteness requirements might single out the unique maximally supersymmetric field theory – *maximal* $N=8$ *supergravity* – as the prime candidate for a unified theory of quantum gravity, but that theory was eventually abandoned in favor of superstring theory as it became clear that maximal supersymmetry by itself may not suffice to rule out all possible counterterms. Superstring theory gets rid of the divergences in a different way, by resolving the point-like interactions of QFT into extended vertices, relying not only on supersymmetry, but also on a specifically 'stringy' symmetry, *modular invariance*. Nevertheless, very recent developments [12] have rekindled the debate whether $N = 8$ supergravity could, after all, be a purely field theoretic extension of Einstein's theory that is UV finite to all orders.

On the other side, one can argue that the UV divergences of perturbative quantum gravity are merely an artifact of the perturbative treatment, and will disappear upon a proper *non-perturbative quantization* of Einstein's theory. In this view, perturbative quantization is tantamount to 'steamrollering Einstein's beautiful theory into flatness and linearity' (R. Penrose): by giving up the core features of GR, namely general covariance and background independence, one cannot expect to get any sensible complete answer. This is the point of view adopted by most of the 'non-string' approaches, see e.g. [5, 6]. The concrete technical implementation of this proposal invokes unusual properties which are very different from familiar QFT concepts; for instance, the finiteness properties of canonical loop quantum gravity hinge on the non-separability of the kinematical Hilbert space.[4] These features are at the origin of the difficulties that this approach encounters in recovering a proper semi-classical limit, and make it difficult to link up with established QFT results. Also for this reason there is so far no clue from non-perturbative quantization techniques as to what the detailed mechanism is that could dispose of the divergence (1).

There is a third (and more conservative) possibility that has lately received considerable attention, namely *asymptotic safety* [1, 14]. This is the proposal that the non-renormalizability of quantum gravity can be

[4] The non-separability of the kinematical Hilbert space is also a crucial ingredient in proposals to resolve space-time singularities in loop quantum cosmology [13].

resolved by a kind of *non-perturbative renormalizability*, in the sense that there might exist a non-trivial fixed point to which the theory flows in the UV. Such a behavior would be similar to QCD, which flows to an asymptotically free theory in the UV, but the UV fixed point action for Einstein's theory would not be free, but rather characterized by higher order contributions in the Riemann tensor. In this case there would be no such thing as a 'smallest distance', and space-time would remain a continuum below the Planck scale. If it works, asymptotic safety is probably the only way to tame the divergences of perturbatively quantized gravity *without* resorting to the cancellation mechanisms invoked by supergravity and superstring theory. The hypothetical non-perturbative renormalizability of gravity would also have to come to the rescue to resolve the inconsistencies of standard QFT.

However, independently of which point of view one prefers, it should be clear that *no approach to quantum gravity can claim complete success that does not explain in full and convincing detail the ultimate fate of the divergences of perturbative quantum gravity.*

3. The role of matter

A main point of disagreement between the different approaches concerns the role of matter degrees of freedom. At least up to now, in modern loop and spin foam quantum gravity or other discrete approaches 'matter does not matter', in the sense that matter degrees of freedom are usually treated as more of an accessory that can be added at will once the quantization of pure gravity has been achieved. By contrast, to a supergravity/string practitioner the matter content of the world must play a key role in the search for quantum gravity. String theory goes even further, positing that the graviton is but one excitation (although a very distinguished one) among an infinite tower of quantized vibrational modes that should also include all the constituents of matter, and that all these degrees of freedom are required for the consistency of the theory.

Perhaps it is fitting at this point to recall what Einstein himself remarked on the different character of the two sides of his field equations: the left hand side is pure geometry and beautifully unique, thus made of marble, whereas the right hand side has no share in this beauty:

$$\underbrace{R_{\mu\nu} - \frac{1}{2} g_{\mu\nu} R}_{\text{Marble}} = \underbrace{\kappa T_{\mu\nu}}_{\text{Timber?}} \tag{2}$$

Indeed, the question that occupied Einstein until the end of his life was this: can we understand the right hand side geometrically, thereby removing its arbitrariness? Put differently, is there a way of massaging the right hand side and moving it to the left hand side, in such a way that everything can be understood as coming from some sort of generalized geometry?

Over the last ninety years there has been some remarkable progress in this direction (see e.g. the reprint volume [15]), but we still do not know whether these ideas really pan out. Already in 1921, T. Kaluza noticed that electromagnetism (Maxwell's theory) can be understood as originating from a five-dimensional theory of pure gravity; later O. Klein extended this proposal to non-abelian gauge interactions. The idea of higher dimensions and of finding a geometrical explanation for the existence of matter continues to hold fascination to this day, most recently with the idea of *large* extra dimensions (whereas the original Kaluza-Klein proposal assumed the extra dimensions to be of Planck size in extension). Supersymmetry and supergravity may likewise be viewed as variants of the Kaluza-Klein program: they generalize ordinary geometry by including *fermionic* dimensions. This leads to the replacement of ordinary space-time by a superspace consisting of bosonic (even) and fermionic (odd) coordinates, thus incorporating *fermionic matter* into the geometry [16]. Accordingly, the possible discovery of supersymmetric particles at LHC could be interpreted as evidence of new dimensions of space and time.

There is not so much discussion of such ideas in the 'non-string' context, where neither unification nor extra dimensions feature prominently (the Ashtekar variables exist only in three and four space-time dimensions), and the focus is on Einstein gravity in four dimensions. Although loop and spin foam quantum gravity are thus very much tuned to four dimensions, there have nevertheless been attempts to extend the framework to higher dimensions, specifically by replacing the groups $SU(2)$ (for loop quantum gravity) or $SO(4)$ or $SO(3,1)$ (for spin foam models) by bigger groups, with higher dimensional analogues of the Ashtekar variables, but it is not clear whether one can arrive in this way at a unification properly incorporating the SM degrees of freedom.

4. The hierarchy problem

A problem that is not so much in the focus of the GR community, but much discussed in the particle physics community concerns the question of scales and hierarchies. The gravitational force is much weaker than the other forces (as one can see immediately by comparing the gravitational attraction between the electron and the nucleus in an atom with the electric Coulomb force, which differ by a factor 10^{-40}). The so-called hierarchy problem, then, is the question whether this huge difference in scales can be 'naturally' understood and explained. [5] In particle physics the problem is

[5] Of course, the biggest and most puzzling hierarchy problem concerns the smallness of the observed cosmological constant.

reflected in the mass hierarchies of elementary particles. Already by itself, the observed particle spectrum covers a large range of mass values: light neutrinos have masses of less than 1 eV, the lightest quarks have masses of a few MeV while the top quark, which is the heaviest quark discovered so far, has a mass of around 173 GeV, so even quark masses differ by factors on the order of 10^5, presenting a 'little hierarchy problem'. But all these mass values are still tiny in comparison with the Planck scale, which is at 10^{19} GeV! This, then, is the distance that theory has to bridge: at the lower end it is the electroweak scale that is now being explored at LHC, while at the higher end it is Planck scale quantum gravity.

A much advertized, but very QFT specific, indication of the problem is the occurrence of quadratic divergences in radiative corrections to the scalar (Higgs) boson mass, which require an enormous fine-tuning to keep the observed value so small in comparison with the Planck mass, the 'natural' value. The absence of quadratic divergences in supersymmetric theories, where divergences are at most logarithmic, is widely considered as a strong argument for low energy supersymmetry, and the prediction that each SM particle should be accompanied by a supersymmetric partner.

The smallness of the gravitational coupling in comparison with the other couplings in nature is the main obstacle towards the verification or falsification of any proposed model of quantum gravity. Unless there is a dramatic evolution of the strength of the gravitational coupling over experimentally reachable energy scales there is no hope of 'seeing' quantum gravity effects in the laboratory. So one needs to find ways and means to reason indirectly in order to identify low energy hints of Planck scale physics. One possibility might be to look for signatures of quantum gravity in the detailed structure of CMB fluctuations. The other possibility (which is more in line with this talk) is to try to read the signs and hints from the observed structure of the low energy world. In the final consequence, this would require a more or less unique prediction for low energy physics and the observed matter content of the world.[6] A more exotic possibility, advocated by proponents of large extra dimension scenarios, could be an (enormous) increase in the gravitational coupling strength in the TeV range that would make quantum gravity and quantum string effects directly accessible to experiment ('TeV scale quantum gravity').

At any rate, *it remains a key challenge for any proposed theory of quantum gravity to offer quantifiable criteria for its confirmation or falsification.* And the emphasis here is on 'quantitative', not on qualitative

[6] This option is not very popular with aficionados of the multiverse or the anthropic principle but, interestingly, the hope for a *unique* path from quantum gravity to the SM is also prominently visible in the very first papers on the heterotic superstring [17, 18].

features that might be shared by very different approaches and thus may not suffice to discriminate between them (for instance, I would suspect this to be the case for specific properties of the CMB fluctuations, which may not contain enough information for us to 'read off' quantum gravity). So the challenge is to come up with criteria that allow to *unambiguously discriminate a given proposal against alternative ones*!

5. From the Standard Model to the Planck scale

By now, the SM of particle physics is an extremely well tested theory. It is based on the (Yang-Mills) gauge principle with Yang-Mills group $G_{SM} = SU(3)_c \times SU(2)_w \times U(1)_Y$. Forces are mediated by spin-one gauge bosons. Matter is made up of spin-$\frac{1}{2}$ fermions: at this time, we know of 48 fundamental fermions which are grouped into three generations (families) of 16 quarks and leptons each (including right-chiral neutrinos). There is no evidence so far from LHC of any new fundamental fermions.

However, after decades of theoretical research, we still do not know what distinguishes the SM gauge group from other possible choices. Apart from anomaly cancellations (see below), the same ignorance prevails with regard to the observed matter content of the SM. Why does Nature repeat itself with three generations of quarks and leptons (another fact confirmed by CERN experiments, as well as cosmological observations)? What causes symmetry breaking and what is the origin of mass? And, returning to the question of hierarchies, what keeps the electroweak scale stable with regard to the Planck scale? (More on this below...) And, finally, why do we live in four space-time dimensions? To tackle these questions, numerous proposals have been put forward for physics beyond the Standard Model (or 'BSM physics', for short): Grand Unification (or GUTs, for short), technicolor, low energy supersymmetry, large extra dimensions, TeV scale gravity, excited gauge bosons, and so on.

The main recent progress is the discovery of a scalar boson by LHC and the strong evidence that this particle has all the requisite properties of the Higgs boson, especially with the most recent data indicating that it has indeed spin zero and even parity (a remaining uncertainty concerns the coupling to the SM fermions, which must be proportional to their masses). In addition the symmetry breaking mechanism giving mass to gauge bosons ('Brout-Englert-Higgs mechanism') has now been confirmed. However, much to the dismay of many of my colleagues, no signs of 'new physics' have shown up so far at LHC. It is therefore not excluded that there may be nothing more than the SM, augmented by right-chiral neutrinos, right up to the Planck scale, a scenario that is usually referred to as the 'Grand Desert'.

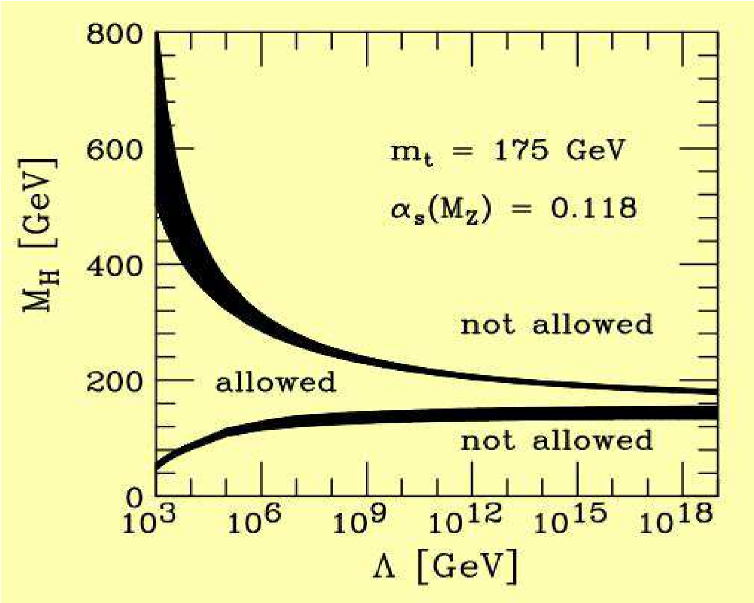

Figure 2. Can the SM survive up to the Planck scale? The upper envelope enforces avoidance of Landau pole for the scalar self-coupling, while the lower envelope ensures avoidance of vacuum instability [19] (with an assumed top quark mass of 175 GeV this plot is not quite up to date, but this does not affect our main conclusions).

Since the Higgs boson is partly responsible[7] for the generation of mass, and mass measures the strength of gravitational coupling, one can reasonably ask whether these data contain indications of Planck scale physics reaching down to the electroweak scale. It is here that the question of stability of the electroweak scale comes into play. Actually, the stability of the Standard Model is under menace from two sides. Following the RG evolution of the scalar self-coupling up to the Planck scale, one danger is the Landau pole where the scalar self-coupling diverges (as happens for IR free theories with scalar fields) and the theory breaks down. The other danger is the potential instability caused by the negative contribution to the effective potential from the top quark (the effective potential includes perturbatively computable quantum corrections to the classical potential).

[7] Only partly, as for instance the larger part of the proton mass is due to non-perturbative QCD effects!

In Homer's tale, Ulysses has to maneuver his ship between two formidable obstacles, *Skylla* and *Charybdis*: on the one side he must steer it away from the rock against which it will crash, and on the other side must avoid the sea monster that will swallow the ship. The plot of figure 2 illustrates the situation. The vertical direction is the Higgs mass, which grows proportionally with the Higgs self-coupling. Through the RG evolution the scalar self-coupling will eventually hit the Landau pole, at which point the theory crashes. Exactly where this happens depends very delicately on the Higgs mass. The upper curve (the rock) in figure 2 [19] relates the location of the Landau pole directly to the Higgs mass. The lower curve (the sea monster) is the constraint from the negative contribution to the effective Higgs potential. If this contribution is too negative, it will make the potential unbounded from below. For the SM, the negative contribution is mainly due to the top quark, and it is a danger precisely because the top quark mass is so large. As a result, if you want to salvage the SM up to the Planck scale, there remains only a very narrow strip for the SM parameters (masses and couplings). The recent results and data from LHC indicate that Nature might indeed avail itself of this possibility: with a Higgs mass of about 125 GeV, the Landau pole can safely hide behind the Planck scale, but this value is so low that the SM hovers on the brink of instability! See also [20] for an interesting interpretation of this value from the point of view of asymptotic safety.

To be sure, the potential instability of the effective potential is the worse of the two dangers. Namely, the occurrence of a Landau pole can always be interpreted as signalling the onset of 'new physics' where new degrees of freedom open up and thereby cure the problem. A well known example of this phenomenon is the old Fermi theory of weak interactions, where the non-renormalizable four-fermion vertex is dissolved at sufficiently high energies by new degrees of freedom (W and Z bosons) into a renormalizable and unitary theory. Another example would be the (still conjectural) appearance of supersymmetry in the TeV range, which would remove the Landau pole and also ensure full stability, as the effective potential in a globally supersymmetric theory is always bounded from below (this is no longer true for local supersymmetry).

If we find out whether or not there are genuine new degrees of freedom in the TeV range of energies, we may also get closer to answering the old question of the ultimate divisibility of matter, namely the question whether the known particles possess further substructures, sub-substructures, and so on, as we probe smaller and smaller distances. Translated into the UV, the question can be rephrased as the question whether there are any 'screens' ($\equiv$ scales of 'new physics') between the electroweak scale and the Planck scale. The more of such screens there were between the electroweak scale and the Planck scale, the less one would be be able to 'see' of Planck

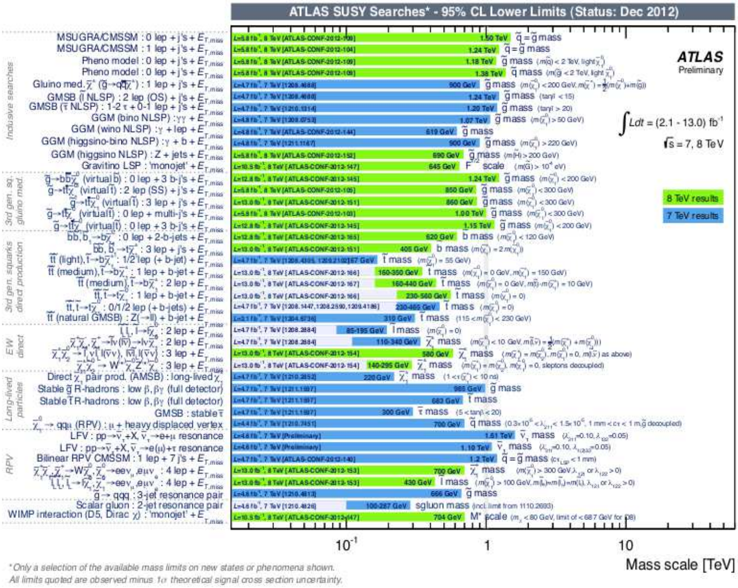

Figure 3. Low energy supersymmetry?

scale physics. On the other hand, the fewer there are, the harder becomes the challenge of explaining low energy physics from Planck scale physics.

LHC is now testing a large number of 'BSM' proposals, and actually eliminating many of them.[8] Figure 3 shows the latest exclusion plot from December 2012 on the search for various signatures of supersymmetry. Figure 4 shows a similar plot from December 2012 for 'exotica' such as large extra dimensions, mini black holes, excited W and Z bosons, quark substructure, and so on, with some exclusions already reaching up to 10 TeV. As you can see, even to refute only a representative subset of the proposals on the market and to keep up with the flood of theoretical ideas is a painstaking effort for the experimentalists, requiring teams of thousands of people and thousands of computers!

There are theoretical indications that LHC may not reveal much new beyond the SM Higgs boson, and thus *no* screens between the electroweak scale and the Planck scale. It is a remarkable fact that the SM Lagrangian is *classically conformally invariant except for a single term*, the explicit

[8] The two plots shown below have been downloaded from the CERN website `https://twiki.cern.ch/twiki/bin/view/AtlasPublic/CombinedSummaryPlots` where also a summary of many further results can be found.

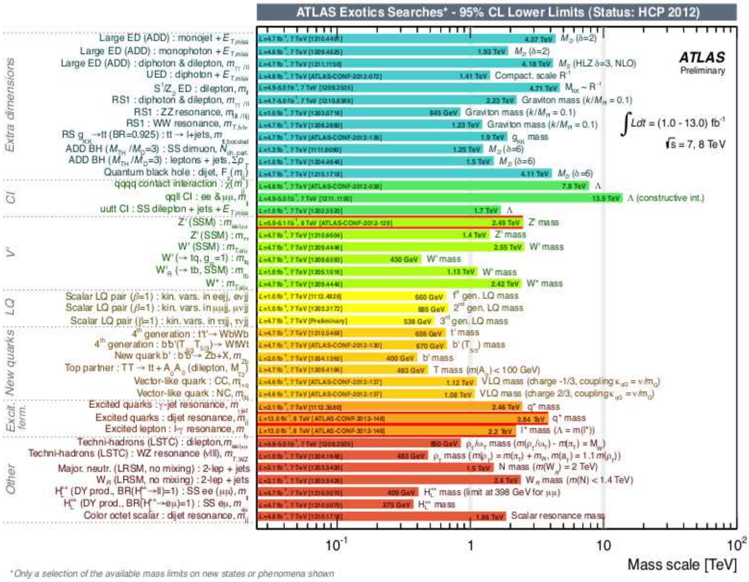

Figure 4. Low energy exotics?

mass term in the Higgs potential. But in a classically conformal theory mass terms can in principle be generated by the conformal anomaly, the quantum mechanical breaking of conformal invariance, which could also trigger spontaneous symmetry breaking [21]. Maybe no explicit mass terms are needed in the SM Lagrangian, and the mechanism stabilizing the electroweak scale is *conformal symmetry* rather than low energy supersymmetry [22, 23]? A further hint in this direction comes from the flows of the SM couplings under the renormalization group: it almost looks like these couplings could 'keep each other under control' so as to prevent both Landau poles and instabilities right up to the Planck scale! This is because bosons and fermions contribute with opposite signs to the corresponding β-functions. The scalar self-coupling would normally blow up under the flow, but is kept under control by the top quark contribution which delays the appearance of the Landau pole until after the Planck scale. The same mechanism is at work for the top quark (Yukawa) coupling which is asymptotically not free either: it, too, would blow up, but is kept under control by the strong coupling α_s, again shifting the Landau pole beyond the Planck scale. Finally α_s itself is kept under control in the UV by asymptotic freedom.

In summary, it could just be that *the mass patterns and the couplings in the Standard Model precisely conspire to make the theory survive to the Planck scale.* In this case there would be no 'new physics' beyond the electroweak scale and the theory would have to be embedded directly *as is* into a Planck scale theory of quantum gravity. In my opinion, this may actually be our best chance to gain direct access to the Planck scale, both theoretically and experimentally!

6. Anomalies

There is another remarkable property of the SM which may be interpreted as a hint of how Planck scale physics could affect low energy physics, and this is the complete cancellation of gauge anomalies (see [24] for an introduction and many references to the original work). Anomalies occur generically when a classical Lagrangian is invariant a symmetry, but that symmetry cannot be preserved by the regularization that quantization requires. When the regulator is removed there is a finite remnant, and this is referred to as the anomaly, an $\mathcal{O}(\hbar)$ violation of a classical conservation laws. The classic example of such a symmetry is chiral invariance that explains the (near-)masslessness of fermions, but cannot be regulated, leading to the famous axial anomaly in QED that accounts for the decay of the π^0 meson.

When anomalous currents are coupled to gauge fields, the anomaly can deal a fatal blow to the theory. Recall that the coupling of a gauge field A_μ to charged matter generally takes the Noether form $\propto A_\mu J^\mu$, where J^μ is the classically conserved matter current. In the presence of an anomaly the variation of this term would give

$$\int \delta A_\mu J^\mu = \int \partial_\mu \omega J^\mu = -\int \omega \partial_\mu J^\mu \propto \mathcal{O}(\hbar) \neq 0\,. \tag{3}$$

Gauge invariance would thus no longer hold, and this violation would destroy the renormalizability of the SM and thereby its predictivity. To verify that all gauge anomalies and gravitational anomalies cancel in the Standard Model requires the computation of various triangle diagrams with chiral fermions circulating in the loop, and involves traces of the form $\mathrm{Tr}\, T^a\{T^b, T^c\}$, where T^a belong to the Lie algebra of the Standard Model gauge group. More specifically, the calculation reduces to the evaluation of

$$\sum \pm \mathrm{Tr}\, YYY = \sum \pm \mathrm{Tr}\, ttY = \sum \pm \mathrm{Tr}\, Y = 0 \tag{4}$$

where Y is the electroweak hypercharge, and t denotes any generator of $SU(2)_w$ or $SU(3)_c$; the sum runs over all SM fermions, with '+' for positive and '−' for negative chirality fermions. If you work through the whole list of such diagrams you will find that they all 'miraculously' sum up to zero [24]. From (4) it is obvious that the cancellation would be trivial if the SM were a vector-like theory with no preferred handedness or chirality. Remarkably, Nature prefers to break parity invariance, and to do so subtly in a way that maintains the renormalizability, hence consistency. In fact, the anomaly cancellations fix the fermion content almost uniquely to what it is, separately for each generation. Therefore, despite its 'messy' appearance *the Standard Model is surprisingly unique, and also surprisingly economical for what it does!*

There are two crucial features that must be emphasized here. The first is that *a proper anomaly does not and must not depend on how the theory is regulated.* Secondly, anomalies are often regarded as a perturbative phenomenon, but this is not strictly true. The famous Adler-Bardeen theorem asserts that the anomaly is entirely due to the one-loop contribution, and that there are thus no further contributions beyond one loop. In other words, the one-loop result is *exact to all orders*, hence non-perturbative!

Anomalies should also be expected to play a role in quantum gravity, and in determining whether a specific proposal is ultimately consistent or not. For instance, the classical constraint algebra of General Relativity in the Hamiltonian formulation has the schematic form

$$\{D,D\} \sim D\,, \quad \{D,H\} \sim H\,, \quad \{H,H\} \sim D \tag{5}$$

where D and H, respectively denote the diffeomorphism constraints and the Hamiltonian constraint, and this algebra is expected to be modified by quantum corrections. This expectation is borne out by the simplest example, matter-coupled quantum gravity in two space-time dimensions. Here the most general form of the space-time diffeomorphism algebra including anomalies is known to take the form

$$[T_{\pm\pm}(x), T_{\pm\pm}(y)] = \delta'(x,y)\Big(T_{\pm\pm}(x) + T_{\pm\pm}(y)\Big) + \hbar c \delta'''(x,y)\,, \tag{6}$$

where $x, y \in \mathbb{R}$, $T_{\pm\pm} := H \pm D$ and c is the central charge. As is well known, virtually all of string theory hinges on the non-zero value of the central charge c! Unfortunately in higher dimensions, there exists neither an analogous uniqueness result, nor even a classification of what the anomalies may be. The main difficulty here is that higher-dimensional diffeomorphism algebras are 'soft', which means that Lie algebra structure 'constants' are not really constant, but field dependent.

7. Outlook

So where do we stand? At this time there is a growing array of proposals for quantum gravity, based on a variety of different and even mutually contradictory assumptions and hypotheses. The following is a selection of current approaches (to which you may add your own favorite):

- Supergravity, Superstrings and M-Theory
- AdS/CFT and Holography
- Path integrals: Euclidean, Lorentzian, matrix models, ...
- Canonical Quantization (metric formalism)
- Loop Quantum Gravity (with either connections or holonomies)
- Discrete Quantum Gravity: Regge calculus, (causal) dynamical triangulations
- Discrete Quantum Gravity: spin foams, group field theory
- Non-commutative geometry and space-time
- Asymptotic Safety and RG Fixed Points
- Emergent (quantum) gravity from thermodynamics
- Causal Sets
- Cellular Automata ('computing quantum space-time')

Among these string theory remains the leading contender, not least because it naturally incorporates (and even requires) matter degrees of freedom. Nevertheless, we still do not have a single hint from experiment and observation (for instance, in the form of supersymmetric partners to the known elementary particles) that it is indeed the right theory. Perhaps it is thus not so surprising that 'non-string approaches' have been gaining in popularity over the past few years.

Having grown out of particle physics and being modeled on its basic concepts, string theory has no problem of principle in connecting to low energy physics; being a perturbative approach, it also has no difficulties in reproducing the correct semi-classical limit and the Einstein field equations. But after more than two decades of effort, string theory is still struggling to reproduce the Standard Model *as is*, that is, without the heavy extra baggage that comes with (for instance) the supersymmetric extensions of the SM referred to as 'MSSM', 'CMSSM' or 'NMSSM', and so on. Moreover, it has considerable difficulties in incorporating a *positive* cosmological constant – in fact, like supergravity, superstring theory has an overwhelming preference for negative Λ! String theory, as originally formulated, is a background dependent and perturbative theory. However, there have been important advances and recent developments, especially in connection with the AdS/CFT correspondence and gauge/gravity or weak/strong dualities, that transcend perturbation theory and have provided important insights into the non-perturbative functioning of the theory (see e.g. [25] for a recent update). Nevertheless, in its present form string theory does not offer a convincing scenario for the resolution of (cosmological) space-time singularities, and so far cannot tell us what really 'happens' to space-time at the Planck scale.

I have already mentioned the impressive recent advances in perturbative QFT techniques [12], yielding evidence that $N = 8$ supergravity may be finite to all orders, contrary to expectations held for more than 30 years. If this theory could be shown to be a purely quantum field theoretic extension of Einstein's theory *without* UV singularities, this would partially undermine one of string theory's chief arguments why QFT must be abandoned. Of course, this would not relieve us of the task of working towards a *non-perturbative* understanding of physics at the very shortest distances, as the putative finiteness by itself would not tell us why and how the space-time continuum is dissolved at the Planck scale. And even if the theory turned out to be UV finite, many would doubt whether $N = 8$ supergravity has anything to do with 'real world physics'. Yet, there is a curious coincidence here: when supersymmetry is completely broken, eight spin-$\frac{1}{2}$ fermions are converted into Goldstinos in order render the eight gravitinos massive, leaving us with 48 spin-$\frac{1}{2}$ fermions, exactly the right number! Most likely a mirage, but who knows? [9]

In contrast to string theory the non-perturbative approaches put the main emphasis on GR concepts from the very beginning, to wit, (spatial) background independence and diffeomorphism invariance. Following this avenue has led to intriguing new ideas and proposals as to what a quantum space-time might actually 'look like'. Nevertheless, it is hard to see how such ideas could ever be put to a real test (other than internal consistency checks). A main criticism from the point of view taken here is that these approaches have not incorporated essential insights from particle physics up to now, such as the restrictions from anomaly cancellations. Furthermore, the ambiguities related to quantization and the incorporation of matter couplings have not been resolved in a satisfactory fashion in my opinion, and the recovery of the proper semi-classical limit remains an outstanding challenge.

To conclude let me restate my main worry. In one form or another the existing approaches to quantum gravity suffer from a very large number of ambiguities, so far preventing any kind of prediction with which the theory will stand or fall. Even at the risk of sounding polemical, I would put this ambiguity at 10^{500} (or even more) – in any case a number too large to cut down for any conceivable kind of experimental or observational advance.

- **Superstring theory** predicts the existence of myriads of 'consistent' vacua, all of which are supposed to be realized somewhere in the multiverse (or 'megaverse') – leading to the conclusion that essentially

[9] On this point, see also [26].

anything goes when it comes to answering the questions raised at the beginning of section 5 (most notably, it is claimed that the multiverse also 'solves' the cosmological constant problem).

- **Loop quantum gravity** and related approaches are compatible with many 'consistent' Hamiltonians (or spin foam models), and with an essentially arbitrary menu of matter fields. Even disregarding technical issues such as quantization ambiguities, it looks again like almost *anything goes*. Idem for models of lattice and discrete quantum gravity.
- **Asymptotic Safety** is an assumption that, according to its proponents, works almost *generically* – that is, independently of the specific 'initial' conditions for the RG flows, of the matter content and even the number of space-time dimensions (if that number is not extremely large), leaving us with numerous 'consistent' RG flows.

In my view the real question is this: if there are all these 'consistent' (according to your definition) ansätze, does Nature simply pick the 'right' answer at random from a huge variety of possibilities, or are there criteria to narrow down the number of choices? Being exposed to many talks from the different 'quantum gravity camps' I am invariably struck by the success stories I keep hearing, and the implicit or explicit claims that 'we are almost there'. I, for one, would much prefer to hear once in a while that something does *not* work, and to see some indications of *inconsistencies* that might enable us to discriminate between a rapidly growing number of diverging ideas on quantum gravity [27, 28]. If, however, the plethora of theory ambiguities were to stay with us I would conclude that our search for an ultimate explanation, and with it the search for quantum gravity, may come to an ignominious end (like in Breughel's painting). I cannot imagine that this is what Einstein had in mind during his stay in Prague, nor in the later years of his life when he was striving to figure out "the old one's tricks" (or, in the original German, "dem Alten auf die Schliche kommen").

So let me repeat my main message: the incompleteness of the Standard Model is one of the strongest arguments in favor of quantizing gravity and searching for new concepts replacing classical notions of space and time. The observed features of SM may contain important hints of its possible UV completion and Planck scale physics, and these hints should be given due consideration in the search for a consistent theory of quantum gravity.

Acknowledgments: I would like to thank Jianwei Mei for his help in turning my talk into a (hopefully) readable text and Krzysztof Meissner for many enjoyable and illuminating discussions on the state of the art.

References

[1] S. Weinberg, `arXiv:0903.0568[hep-th]`
[2] S.W. Hawking and W. Israel, (eds.): *General Relativity: An Einstein centenary survey*, Cambridge University Press, 1979
[3] C. Kiefer, *Quantum gravity*, Clarendon Press, 2004
[4] A. Ashtekar (ed.): *100 years of relativity. Space-time structure: Einstein and beyond*, World Scientific, 2005
[5] C. Rovelli, *Quantum Gravity*, Cambridge University Press, 2004.
[6] T. Thiemann, *Modern canonical quantum general relativity*, Cambridge University Press, 2007.
[7] M.B. Green, J.H. Schwarz and E. Witten, *Superstring Theory I & II*, Cambridge University Press, 1987
[8] J. Polchinski, *String Theory I & II*, Cambridge University Press, 1998
[9] R. Blumenhagen, D. Lüst and S. Theisen, *Basic Concepts of String Theory*, Springer Verlag, 2012
[10] M.H. Goroff and A. Sagnotti, Nucl. Phys. **B266** (1986) 709
[11] A. van de Ven, Nucl. Phys. **B378** (1992) 309
[12] Z. Bern, J.J. Carrasco, L.J. Dixon, H. Johansson and R. Roiban, Fortsch. Phys. **59** (2011) 561
[13] M. Bojowald, Phys. Rev. Lett. **86** (2001) 5227
[14] M. Reuter and F. Saueressig, New J. Phys. **14** (2012) 055022
[15] T. Applequist, A. Chodos and P.G.O. Freund, *Modern Kaluza-Klein Theories*, Addison-Wesley Publ. Comp., 1987
[16] J. Bagger and J. Wess, *Supersymmetry and Supergravity*, Princeton University Press, 1984.
[17] D.J. Gross, J.A. Harvey, E.J. Martinec and R. Rohm, Phys. Rev. Lett. **54** (1985) 502
[18] P. Candelas, G.T. Horowitz, A. Strominger and E. Witten, Nucl. Phys. **B258** (1985) 46
[19] T. Hambye and K. Riesselmann, Phys. Rev. **D55** (1997) 7255
[20] M. Shaposhnikov and C. Wetterich, Phys. Lett. **B683** (2010) 196
[21] S. Coleman and E. Weinberg, Phys. Rev. **D7** (1973) 1888
[22] W.A. Bardeen, preprint FERMILAB-CONF-95-391-T (1995)
[23] K.A. Meissner and H. Nicolai, Phys. Lett. **B648** (2007) 312
[24] R. Bertlmann, *Anomalies in Quantum Field Theory*, Clarendon Press, Oxford (1996)
[25] M. Blau and S. Theisen, Gen. Rel. Grav. **41** (2009) 743
[26] H. Nicolai and N.P. Warner, Nucl. Phys. **B259** (1985) 412
[27] H. Nicolai, K. Peeters and M. Zamaklar, Class. Quantum Grav. **22** (2005) R193
[28] S. Alexandrov and P. Roche, Phys. Rept. **506** (2011) 41