QUANTUM-GRAVITY PHENOMENOLOGY: STATUS AND PROSPECTS

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Over the last few years part of the quantum-gravity community has adopted a more optimistic attitude toward the possibility of finding experimental contexts providing insight on non-classical properties of spacetime. I review those quantum-gravity phenomenology proposals which were instrumental in bringing about this change of attitude, and I discuss the prospects for the short-term future of quantum-gravity phenomenology.

1. Quantum Gravity Phenomenology

The "quantum-gravity problem" has been studied for more than 70 years assuming that no guidance could be obtained from experiments. This in turn led to the assumption that the most promising path toward the solution of the problem would be the construction and analysis of very ambitious theories, some would call them "theories of everything", capable of solving at once all of the issues raised by the coexistence of gravitation (general relativity) and quantum mechanics. In other research areas the abundant availability of puzzling experimental data encourages theorists to propose phenomenological models which solve the puzzles but are conceptually unsatisfactory on many grounds. Often those apparently unsatisfactory models turn out to provide an important starting point for the identification of the correct (and conceptually satisfactory) theoretical description of the new phenomena. But in this quantum-gravity research area, since there was no experimental guidance, it was inevitable for theorists to be tempted into trying to identify the correct theoretical framework relying exclusively on some criteria of conceptual compellingness. Of course, tempting as it may seem, this strategy would not be acceptable for a scientific endeavor. Even the most compelling and conceptually satisfying theory could not be adopted without experimental confirmation. The mirage that one day within an ambitious quantum-gravity theory one might derive

from first principles a falsifiable prediction for the mundane realm of doable experiments gives some "scientific legitimacy" to these research programmes, but this possiblity never materialized, it may well be just a mirage.

1.1. *Objectives of quantum gravity phenomenology*

Over the last few years this author and a growing number of research groups have attempted to tackle the quantum-gravity problem with an approach which is more consistent with the traditional strategy of scientific work. Simple (in some cases even simple-minded) non-classical pictures of spacetime are being analyzed with strong emphasis on their observable predictions. Certain classes of experiments have been shown to have extremely high sensitivity to some non-classical features of spacetime. We now even have (see later) some first examples of experimental puzzles whose solution is being sought also within simple ideas involving non-classical pictures of spacetime. The hope is that by trial and error, both on the theory side and on the experiment side, we might eventually stumble upon the first few definite (experimental!) hints on the quantum-gravity problem. Here I intend to give an overview of this "quantum gravity phenomenology"[1].

"Quantum gravity phenomenology" is an intentionally [1] vague name, reflecting the fact that this new approach to quantum-gravity research requires a combination of theory and experiments and also reflecting the fact that it does not adopt any particular prejudice concerning the structure of spacetime at short distances (in particular, "string theory" [2,3], "loop quantum gravity" [4,5,6] and "noncommutative geometry" [7,8] are seen as equally deserving mathematical-physics programmes). It is rather the proposal that quantum-gravity research should proceed just in the old-fashioned way of scientific endeavors: through small incremental steps starting from what we know and combining mathematical-physics studies with experimental studies to reach deeper and deeper layers of understanding of the problem at hand (in this case the short-distance structure of spacetime and the laws that govern it).

The most popular quantum-gravity approaches, such as string theory and loop quantum gravity, could be described as "top-to-bottom approaches" since they start off with some key assumption about the structure of spacetime at scales that are some 17 orders of magnitude beyond the scales presently accessible experimentally, and then they should work their way back to the realm of doable experiments. With "quantum gravity phenomenology" I would like to refer to all studies that are intended to contribute to a "bottom-to-top approach" to the quantum-gravity problem.

Since the problem at hand is really difficult (arguably the most challenging problem ever faced by the physics community) it appears likely that the two complementary approaches might combine in a useful way: for the "bottom-to-top approach" it is important to get some guidance from the (however tentative) indications emerging from the "top-to-bottom approaches", while for "top-to-bottom approaches" it might be useful to be alerted by quantum-gravity phenomenologists

with respect to the type of new effects that could be most stringently tested experimentally (it is hard for "top-to-bottom approaches" to obtain a complete description of "real" physics, but perhaps it would be possible to dig out predictions on some specific spacetime features that appear to deserve special attention in light of the corresponding experimental sensitivities).

Until very recently the idea of a quantum-gravity phenomenology, and in particular of attempts of identification of experiments with promising sensitivity, was very far from the interests of mainstream quantum-gravity researchers. This is still true for a significant portion of the community, but finally, just over the last couple of years, there is also a significant portion of the community which is forming an interest in experiment-aimed research

1.2. *Prehistory of quantum gravity phenomenology*

To this author's knowledge the first experiment-related studies with some relevance for the quantum-gravity problems are the ones pertaining the effects of **classical** gravitational fields on matter-interferometry experiments. This really started in the mid 1970s with the renowned experiment performed by Colella, Overhauser and Werner [9]. That experiment has been followed by several modifications and refinements (often labeled "COW experiments" from the initials of the scientists involved in the first experiment) all probing the same basic physics, *i.e.* the validity of the Schrödinger equation

$$\left[-\left(\frac{\hbar^2}{2\,M_I} \right) \vec{\nabla}^2 + M_G\,\phi(\vec{r}) \right] \psi(t,\vec{r}) = i\,\hbar\,\frac{\partial\,\psi(t,\vec{r})}{\partial t} \tag{1}$$

for the description of the dynamics of matter (with wave function $\psi(t,\vec{r})$) in presence of the Earth's gravitational potential $\phi(\vec{r})$. [In (1) M_I and M_G denote the inertial and gravitational mass respectively.]

The COW experiments exploit the fact that the Earth's gravitational potential puts together the contributions of a very large number of particles (all the particles composing the Earth) and as a result, in spite of its per-particle weakness, the overall gravitational field is large enough to introduce observable effects.[a] This type of experiment of course does not probe any non-classical property of spacetime. It is the classical gravitational field that plays a role in the experiment. In this sense it should be seen as a marginal aspect of quantum-gravity phenomenology, just testing the correctness of (inherently robust) ideas on the behaviour of quantum mechanics in curved (but still classical) spacetime.[b] However, some important insight for quantum-gravity research has been gained through these experiments, particularly

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[a]Actually the effect turns out to be observably large because of a double "amplification": the first, and most significant, amplification is the mentioned coherent addition of gravitational fields generated by the particles that compose the Earth, the second amplification [10] involves the ratio between the wavelength of the particles used in the COW experiments and some larger length scales involved in the experimental setup.

[b]While "quantum-gravity phenomenology" is being adopted to describe experiments aimed at detecting quantum properties of spacetime, the wider subject of the interplay between quantum

with respect to the faith of the Equivalence Principle. This subject deserves a dedicated review by the experts. I here just bring to the reader's attention some useful reading material [12,13,14], a recent experiment which appears to indicate a violation of the Equivalence Principle [15] (but the reliability of this experimental result is still being debated), and some ideas for intruiging new experiments [10,16] of the COW type.

In the mid 1980s, together with the analysis of other experimental contexts [17,18] probing the interplay between classical general relativity and quantum mechanics of nongravitational degrees of freedom, the analysis of a second class of experimental contexts relevant for quantum gravity was started. This second class of experiments is based on the realization [19,20,21,22,23] that the sensitivity of CPT-symmetry tests using the neutral-kaon and neutral-B systems[c] is reaching a level such that even small quantum-gravity-induced CPT violation might in principle be revealed. Until now there is no evidence of any violation of the CPT symmetry. It should also be noticed that the theory work in this research line was not in the spirit here advocated for quantum-gravity phenomenology. In fact, the CPT tests were motivated with one or another specific idea about an ambitious (top-to-bottom) theory: a certain version of noncritical string theory in the case of Refs. [19,22] and a certain perspective on string field theory in the case of Ref. [21]. Rather than a study of deviations from CPT symmetry with the general objective of reflecting the variety of scenarios by which this could come about in quantum gravity, the motivation provided to the theory community and to experimentalists was linked directly to a specific theory-of-everything picture. Moreover, the phenomenology that got set up in this research line made direct reference only to particle physics, and it was unclear which type of ideas about the **structure** of spacetime were being investigated (one thing is to search for the CPT implications of, say, some schemes for spacetime discreteness or noncommutativity, which would appeal to all those involved in research in related schemes, another is to confront the quantum-gravity community with some particle-physics phenomenology whose connection to gravity/spacetime quantization is not at all transparent or direct). These aspects of those early CPT studies might have played a role in the fact that this research line did not manage to affect the attitude toward experimental tests of the quantum-gravity community.

1.3. *The dawn of quantum gravity phenomenology*

The early pioneering development of the research lines mentioned in the previous subsection (research lines which are still to be deemed crucial for quantum-gravity research) did not lead to a change of attitude of the quantum-gravity community. This change of attitude is however materializing in these last few years, as indicated, for example, by the sharp change of emphasis that one finds in

mechanics and general relativity (even when spacetime can be described in fully classical manner) is sometimes called [11] "Interface of Quantum and Gravitational Realms"

[c]In addition to the neutral-kaon and neutral-B systems there has been recent discussion [24,25] of the possibility to use neutrino physics in the study of quantum-gravity-induced CPT violation.

comparing authoritative quantum-gravity reviews published up to the mid 1990s (see, *e.g.*, Ref. [26]) and the corresponding reviews published over the last couple of years [5,27,28,29,30,31]. Over the last few years several new ideas for tests of quantum-gravity physics have appeared at increasingly fast pace, with a fast growing (although, of course, still relatively small) number of research groups joining the quantum-gravity-phenomenology endeavor. Of course the emergence of some first examples (see later) of experimental puzzles whose solution can plausibly be sought within quantum-gravity phenomenology marked an important turning point.

We now have several examples of experimentally accessible contexts in which conjectured quantum-gravity effects are being considered, including studies of in-vacuo dispersion using gamma-ray astrophysics [32,33], studies of laser-interferometric limits on quantum-gravity induced distance fluctuations [34,35,36,37], studies of the role of quantum-gravity effects in the determination of the energy-momentum-conservation threshold conditions for certain particle-physics processes [38,39,40,41], and studies of the role of quantum gravity in the determination of particle-decay amplitudes [42]. These experimental contexts (together with the CPT tests mentioned in the previous subsection) could be seen as the cornerstones of quantum-gravity phenomenology since they are as close as one can get to direct tests of space-time properties, such as space-time symmetries. I postpone to Sections 3 and 4 a discussion of these ideas.

In closing this subsection I should mention (even though I shall not come back to these studies in the rest of the paper) that there are also other experimental proposals that are part of the quantum-gravity-phenomenology programme but rely on the mediation of some dynamical theory of matter in quantum space-time, so that negative results of these experimental searches might not teach us much at the qualitative level about spacetime structure (the predicted magnitude of the effects depends on spacetime features just as much as it depends on some features of the postulated description of the dynamics of matter in that spacetime). Interested readers can find descriptions of these proposals in Refs. [1,43,44,45,46].

1.4. *Identification of experiments*

The first step for the identification of experiments relevant for quantum gravity is of course the identification of the characteristic scale of this new physics. This is a point on which we have relatively robust guidance from theories and theoretical arguments: the characteristic scale at which non-classical properties of spacetime physics become large (as large as the classical properties they compete with) should be[d] the Planck length $L_p \sim 10^{-35} m$ (or equivalently its inverse, the Planck scale

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[d]I do not review here the arguments that single out the Planck length. There is a large number of, apparently indepedent, arguments that all converge to this scale. Consistently with the overall attitude adopted in quantum-gravity phenomenology one should maintain some level of healthy doubt also about these arguments, but among all theory indications it is certainly fair to say that the indication of this characteristic scale is the most robust element of guidance for quantum-gravity phenomenology. Still one should take notice of recent studies [47] finding ways to effectively

$E_p \sim 10^{28} eV$).

The next step is the identification of the type of effects that quantum-gravity theories might predict. Unfortunately, in spite of more than 70 years of theory work on the quantum-gravity problem, and a certain proliferation of theoretical frameworks being considered, there is only a small number of physical effects that have been considered within quantum-gravity theories. Moreover, most of these effects concern strong-gravity contexts, such as black-hole physics and big-bang physics, which are exciting at the level of conceptual analysis and development of formalism, but of course are not very promising for the actual (experimental) discovery of manifestations of non-classical properties of spacetime. For example, the fact that we are not even able to observe/verify the expected classical properties of black holes clearly suggests that this is not a promising context for quantum-gravity phenomenology[e].

We clearly should give priority to quantum-gravity effects that modify our description of **flat** spacetime. The effects will perhaps be less significant than, say, in black hole physics (in some aspects of black hole physics quantum-gravity effects might be as large as classical physics effects), but we are likely to be better off considering flat spacetime, in which the quality of the data we can obtain is extremely high, even though this will cost us a large suppression of quantum-gravity effects, a suppression which is likely to take the form of some power of the ratio between the Planck length and the wavelength of the particles involved.

The presence of these suppression factors on the one hand reduces sharply our chances of finding quantum-gravity effects, but on the other hand simplifies the problem of identifying promising experimental contexts, since these experimental contexts must enjoy very special properties which would not go easily unnoticed. For laboratory experiments even an optimistic estimate of these suppression factors leads to a suppression of order 10^{-16}, which one obtains by assuming (probably already using some optimism) that at least some quantum-gravity effects are only linearly suppressed by the Planck length and taking as particle wavelength the shorter wavelengths we are able to produce ($\sim 10^{-19}m$). In astrophysics (which however limits one to "observations" rather than "experiments") particles of shorter wavelength are being studied, but even for the highest energy cosmic rays, with energy of $\sim 10^{20} eV$ and therefore wavelengths of $\sim 10^{-27}m$, a suppression of the type L_p/λ would take values of order 10^{-8}. It is mostly as a result of this type of considerations that traditional quantum-gravity reviews considered the possibility of experimental studies with unmitigated pessimism [26]. However, the presence of these large suppression factors surely cannot suffice for drawing any conclusions.

increase the size of this characteristic length scale. Those arguments are not in any way "natural" (not even in the eyes of the scientists who proposed them) but they do justify some reason of concern that perhaps we cannot even rule out surprises about the characteristic scale of quantum-gravity effects.

[e]But it does make sense to use related observational facts to constrain quantum-gravity theories: for example, some theories might be rejected if found to be inconsistent with what we, in some sense, "know" about the early universe or with future data on the abundance of black holes.

Even just looking within the subject of particle physics we know that certain types of small effects can be studied, as illustrated by the example of the remarkable limits obtained on proton instability. Outside of fundamental physics more success stories of this type are easily found: think for example of brownian motion. It is hard but clearly not impossible to find experimental contexts in which there is effectively an amplification of the small effect one intends to study. The prediction of proton decay within certain grandunified theories of particle physics is really a small effect, suppressed by the fourth power of the ratio between the mass of the proton and grandunification scale, which is only three orders of magnitude smaller than the Planck scale. In spite of this horrifying suppression, of order $[m_{proton}/E_{gut}]^4 \sim 10^{-64}$, with a simple idea we have managed to acquire full sensitivity to the new effect: the proton lifetime predicted by grandunified theories is of order $10^{39} s$ and "quite a few" generations of physicists should invest their own lifetimes staring at a single proton before its decay, but by managing to keep under observation a large number of protons (think for example of a situation in which 10^{33} protons are monitored) our sensitivity to proton decay is dramatically increased. In that context the number of protons is the (ordinary-physics) dimensionless quantity that works as "amplifier" of the new-physics effect. Similar considerations explain the success of brownian-motion studies already a century ago.

We should therefore focus our attention[1] on experiments which have something to do with spacetime structure (in flat-spacetime situations only structure can be revealed, in the sense discussed in the following sections) and that host an ordinary-physics dimensionless quantity large enough that (if we are "lucky") it could amplify the extremely small effects we are hoping to discover. So there is clearly a first level of analysis in which one identifies experiments with this rare quality, and a second level of analysis in which one tries to establish whether indeed the candidate "amplifier" could possibly amplify effects connected with spacetime structure.

In parallel with this type of analysis one can perform a complementary analysis which considers one-by-one the quantum-gravity effects which (for good or bad reasons) have surfaced in the quantum-gravity literature, and then for each of them attempts to identify the most significant experimental limit which can be obtained with available technologies. Only very few quantum-gravity effects that might affect flat-spacetime physics (what we presently perceive as physics occurring in the structureless arena of flat Minkowski spacetime) have surfaced in the literature. Moreover, for some of these effects one can quickly realize that the likelyhood of finding an "amplifier" is vanishingly small. For example, many studies consider discretizations of the concepts of length, area, volume, but all of these pictures predict geometric quanta whose magnitude is set by the Planck length independently of the size of the geometric observable being analzed (no amplification in going from the study of, say, small areas to the study of large areas). However, the idea of discretization, with associated short-distance nonlocality, does encourage the idea of departures from conventional CPT symmetry, since the CPT theorem relies on absolute locality (locality at all scales). Similarly spacetime noncommutativity, another

justifiably popular quantum-gravity idea, also encourages CPT studies because it is quite natural [48] (though not necessary) to find that P and/or T transformations acquire new properties in a given noncommutative spacetime. CPT-symmetry studies have the disadvantage that in some approaches to the quantum-gravity problem, *e.g.* loop quantum gravity, one is not yet able to couple ordinary particles to gravity, and the theories are therefore unprepared to describe C transformations.

Planck-scale discreteness or noncommutativity also provide encouragement for tests of Lorentz symmetry. The continuous symmetries of a spacetime reflect of course the structure of that spacetime. Ordinary Lorentz symmetry is governed by the single scale that sets the structure of classical Minkowski spacetime, the speed-of-light constant c. If one introduces additional structure in a flat spacetime its symmetries will be accordingly affected. This is particularly clear in certain classes of noncommutative spacetimes, whose symmetry transformations are characterized by the noncommutativity length scale [49] (possibly the Planck scale) in addition to c, and infinitesimal symmetry transformations are actually described in terms of the new language of Hopf algebras [50,51], rather than by the Poincaré Lie algebra. Because of their sensitivity to any type of structure introduced in the description of flat spacetime, tests of the continuous Lorentz symmetry will probably be found to be relevant for the majority of quantum-gravity approaches.

In addition to discreteness and noncommutativity, with associated possible deviations from conventional Lorentz and CPT symmetry, another picture of spacetime which would have significant implications for our description of flat spacetime is the one of "spacetime foam", which has frequently surfaced in the quantum-gravity literature, although always described at a sort of intuitive formal level, without proper operative description of any associated effects. Since experiments can only test well defined predictions, the fact that the description of spacetime foam that one finds in traditional quantum-gravity studies is not operative of course confronts quantum-gravity phenomenology with a first obvious task of providing such an operative definition. This operative definition must capture the indications that come from formalism, *i.e.* must give physical characterizations of the vague concept of "fuzzy geometry" which is the characterizing property advocated in studies of spacetime foam. In spacetime foam certain sharp predictions of classical-spacetime physics are rendered "unsharp" by quantum-gravity effects. As a way to capture (and endow with physical reality) the concept of fuzzy distance, this author introduced in Refs.[34,35,37] a first operatively defined characteristic of spacetime foam: the noise levels in the readout of a laser interferometer would receive an irreducible (fundamental) contribution from quantum-gravity effects. This noise can in principle be reduced to zero in classical physics, while the ordinary quantum properties of matter already introduce an extra noise contribution with respect to classical physics. Spacetime foam would introduce another source of noise, reflecting the fact that the distances involved in the experiment would be inherently unsharp in a foamy spacetime picture.

In general this idea of spacetime foam motivates us to seek effects that modify

classical-spacetime physics in a nonsystematic way. The studies of Lorentz symmetry and CPT symmetry mentioned above motivate the search of systematic departures from classical-spacetime physics; for example, in certain noncommutative geometries the relevant concept of Lorentz symmetry introduces a systematic dependence of the speed of photons on their wavelength. It appears meaningful to introduce a terminology that would instead attribute to "spacetime foam" all nonsystematic quantum-gravity effects. (Actually this could be a good physical definition of spacetime foam.)

All the, now numerous, proposals that presently compose quantum-gravity phenomenology can be recognized as belonging to one of these three categories: CPT-symmetry tests, Lorentz-symmetry tests, and searches of foam-induced effects. This reflects the fact that these are the only possibilities that have been discussed in the quantum-gravity literature as candidate effects that could be present in the spacetimes that we presently perceive as flat and classical (Minkowski). This may change in the future as more theory ideas are explored and more experimental studies are considered. But in general it will always be possible to distinguish between tests/studies of systematic quantum-gravity effects and nonsystematic quantum-gravity effects. Systematic quantum-gravity effects and nonsystematic quantum-gravity effects have been introduced in this Subsection at an intuitive level, but they will be more technically characterized in the next three Sections.

1.5. *Mathematical-physics aspects of quantum-gravity phenomenology*

One peculiarity of quantum-gravity phenomenology with respect to other phenomenological programmes (think for example of particle-physics phenomenology) is that even some of the relatively unambitious nonclassical pictures of spacetime and gravity that one should consider in quantum-gravity phenomenology may require a rather sophisticated level of mathematical analysis.

A good prototype example of theory work in quantum-gravity phenomenology is provided by the study of flat noncommutative spacetimes. Clearly the introduction of a flat noncommutative spacetime could not possibly provide a complete solution to the quantum-gravity problem, but some of the top-to-bottom approaches appear to indicate that the emergence of noncommutative geometry in quantum gravity is plausible, and if spacetime geometry is in general noncommutative then in particular the spacetimes we presently perceive as classical continuous and commutative (Minkowski spacetime) should then be described at the fundamental level in terms of noncommutative geometry. It is therefore legitimate for a bottom-to-top approach to the quantum-gravity problem to consider (together with other possibilities of course) the possibility of noncommutative versions of Minkowski spacetime. Starting "from the bottom" it is difficult to favour one version of noncommutative Minkowski over another, but one can attack the problem in stages, starting with the simplest cases. In fact, most work on noncommuative versions of Minkowski spacetime has been on the two simplest possibilities: canonical spacetimes, in which

the commutators of the spacetime coordinates are coordinate independent

$$[x_\mu, x_\nu] = i\theta_{\mu\nu} \tag{2}$$

($\mu, \nu, \beta = 0, 1, 2, 3$), and Lie-algebra noncommutative spacetimes, in which the commutators of the spacetime coordinates are linear in the coordinates

$$[x_\mu, x_\nu] = iC_{\mu\nu}^\beta x_\beta \ . \tag{3}$$

Among Lie-algebra noncommutative versions of Minkowski spacetime the desire to preserve $O(3)$ space-rotation covariance has focused most work on κ-Minkowski spacetime, with a single deformation scale κ

$$[x_m, t] = \frac{i}{\kappa} x_m \ , \qquad [x_m, x_l] = 0 \tag{4}$$

($l, m = 1, 2, 3$).

We are finding out that in order to establish what are the characteristic physical predictions of these spacetime pictures some severe mathematical challenges must be faced. For example, it has been realized that in canonical noncommutative spacetimes the Wilson decoupling between high-energy and low-energy physics does not hold[52,53], and the technical and conceptual understanding of the implications of this delicate mathematical property is still in progress[54,55]. Motivation for these studies is coming also from a top-to-bottom approach, since canonical noncommutative spacetimes are used in effective-theory descriptions[52,53] of some features of the physics of strings in certain backgrounds.

Another example of delicate mathematical analysis needed for extracting the physical predictions of these noncommutative spacetimes is the study of the symmetries of κ-Minkowski. It was realized that infinitesimal symmetry transformations in this spacetime could be described in terms of a Hopf algebra [48,51], but for a few years it appeared that several[51] Hopf algebras could describe these symmetries, and, even more concerning, it appeared that the (appropriately deformed) infinitesimal Lorentz transformations could not be combined to obtained a symmetry group of finite deformed-Lorentz transformations. This impasse was overcome only recently by identifying the right Hopf algebra and finding that the associated finite symmetry transformations do form group[49]. The possibility of these deformed symmetries can be tested with forthcoming experiments (see Section 3).

The example of flat noncommutative spacetimes is representative of other bottom-to-top mathematical-physics studies aimed at experiments, in the spirit of quantum-gravity phenomenology. Another representative example is the study of the physics of "weave states of spacetime geometry". These geometry descriptions emerged as part of the loop quantum gravity research programme, but they were taken as bottom-to-top starting point in various studies aimed at experiments (see, *e.g.*, Refs. [57,58]).

2. Characterization of Systematic and Nonsystematic Quantum-Gravity Effects

Systematic quantum-gravity effects and nonsystematic quantum-gravity effects have been introduced in the previous Section at an intuitive level. It is useful to characterize these concepts more quantitatively with the help of a specific example.

Of course we want to focus on a crisp spacetime feature. Let us consider the propagation of massless particles over a distance L in flat spacetime. In classical physics the distance L would be classical, the massless particles would be point-like and follow the classical trajectory along L. In classical physics a bunch of such particles, with energies however different among them, which were emitted along the x axis simultaneously at time $t = 0$ from position $(x_0, 0, 0)$ would reach simultaneously at time $t = L/c \equiv T$ the position $(x_0 + L, 0, 0)$.

This is a good setup because it involves propagation through spacetime, which could pick up features of spacetime structure, and it involves Lorentz symmetry through the speed-of-light constant c and the wavelength-independence of the time of travel T.

Ordinary (known) quantum properties of matter (in classical spacetime) already modify this picture: quantum-mechanical uncertainties impose that the time of emission of a particle of energy E can only be controlled with accuracy $1/E$, and there is of course a corresponding limitation on how accurately the simultaneity of the times of arrival can be established, but the relation $T = L/c$ will emerge if appropriate averaging over a large number of observations is performed.

In this setup, while classical physics (of particles and spacetime) predicts the relation $T = L/c$ (a systematic relation between the observables T and L), the quantum properties of the particles (still assuming classicality of the spacetime) introduce a nonsystematic effect, an uncertainty: $T = L/c \pm \delta T_{QM}$. How could quantum gravity affect this prediction? In order to be covered on all possible fronts we should be open to the possibility of both systematic and nonsystematic quantum-gravity effects. This can be captured in the formula

$$T = (L/c + \Delta T_{QG}) \pm \delta T_{QM} \pm \delta T_{QG} , \tag{5}$$

with self-explanatory notation.

At low energies we have very good access to this type of observations and the associated large statistics allows us to draw the safe conclusion that

$$lim_{E \to 0} \Delta T_{QG} = 0 , \tag{6}$$

but the data available to us do not allow us to exclude that $\Delta T_{QG} \neq 0$ at high energies. This would of course require a deviation from ordinary Lorentz invariance of the spacetime, a systematic effect (an effect on the observed average value of T). It turns out that such a systematic effect is predicted by certain non-classical pictures of spacetime. For example, according to the deformed Lorentz symmetries of κ-Minkowski spacetime one would predict[49] (for $\kappa = 1/L_p$) $\Delta T_{QG} \simeq L_p E T$ at energies E small compared to $1/L_p$.

Of course, our low-energy observations also constrain the admissable values of δT_{QG} at low energies, and in particular we can safely assume $\delta T_{QG} < \delta T_{QM}$ at low

energies. Because of the nature of uncertainties it would always be extremely hard to find evidence of the contribution δT_{QG} in physical contexts such that $\delta T_{QG} < \delta T_{QM}$, but we might eventually discover a physical context in which the "fuzziness of spacetime" dominates over the ordinary uncertainties of quantum mechanics: $\delta T_{QG} > \delta T_{QM}$. This would allow us to establish a fuzzy feature of spacetime itself. I propose to refer to all such nonsystematic quantum-gravity effects as "spacetime foam effects", since their nature is consistent with the intuition emerging from formal work on foam.

The issues I just introduced within the example of the relation between T and L are of course present in the analysis of all relations between spacetime-related observables. In another case the observables A, B, C will be classically related by $A = f_{classic}(B, C)$. Ordinary quantum mechanics will often introduce (unless the observables all commute with each other) an uncertainty: $A = f_{classic}(B, C) + \delta A_{QM}$. Systematic quantum-gravity effects, in particular deviations from classical symmetries, may modify the bare relation between the observables, $f_{classic}(B, C) \to f_{QG}(B, C; L_p) = f_{classic}(B, C) + \Delta A_{QG}$, and in addition nonsystematic (fuzzyness, foaminess) quantum-gravity effects may introduce an additional source of uncertainty: $A = (f_{classic}(B, C) + \Delta A_{QG}) \pm \delta A_{QM} \pm \delta A_{QG}$.

3. Lorentz Tests: an example of Studies of systematic Quantum-Gravity Effects

If the Planck length, L_p, only has the role we presently attribute to it, which is basically the role of a coupling constant (an appropriately rescaled version of the coupling G), no problem arises for FitzGerald-Lorentz contraction, but if we try to promote L_p to the status of an intrinsic characteristic of space-time structure (or a characteristic of the kinematic rules that govern particle propagation in space-time) it is natural to find conflicts with FitzGerald-Lorentz contraction.

For example, it is very hard (perhaps even impossible) to construct discretized versions or non-commutative versions of Minkowski space-time which enjoy ordinary Lorentz symmetry. Pedagogical illustrative examples of this observation have been discussed, *e.g.*, in Ref. [56] for the case of discretization and in Refs. [50,51] for the case of non-commutativity. Under ordinary Lorentz boosts, discretization length scales and/or non-commutativity length scales naturally end up acquiring different values for different inertial observers, just as one would expect in light of the mechanism of FitzGerald-Lorentz contraction. Recently there has been strong interest in the deviations from ordinary Lorentz symmetry that emerge in canonical noncommutative spacetimes[53,54,55] and in κ-Minkowski noncommutative spacetime[49]. As mentioned, the example of canonical noncommutative spacetimes is also indirectly relevant for string theory. Within loop quantum gravity deviations from ordinary Lorentz invariance have been considered in particular in Refs.[57,58].

Some dynamical mechanisms (of the spontaneous symmetry-breaking type) that can lead to deviations from ordinary Lorentz invariance have been considered in

string field theory [59] and in certain noncritical string-theory scenarios[60].

Even outside mainstream quantum-gravity approaches interest in Planck-scale deviations from Lorentz invariance is growing (see, *e.g.*, Refs. [61,62]).

In this Section I want to show that even very small, Planck-length suppressed, deviations from Lorentz invariance could be within the reach of ongoing and forthcoming experiments. Let us focus on the possible emergence of deformed dispersion relations (which is present in the large majority of quantum-gravity-motivated schemes for deviations from ordinary Lorentz invariance) and let us just consider the possibility that the standard dispersion relation $E^2 = m^2 + \vec{p}^2$ be replaced by

$$E^2 = m^2 + \vec{p}^2 + f(\vec{p}^2, E, m; L_p) \ . \tag{7}$$

If the function f is nonvanishing and nontrivial and the energy-momentum transformation rules are ordinary (the ordinary Lorentz transformations) then clearly f cannot have the exact same structure for all inertial observers. In this case one would speak of an instance in which Lorentz invariance is broken, and one could assume that, in spite of the deformation of the dispersion relation, the rules for energy-momentum conservation would be undeformed.

If instead f does have the exact same structure for all inertial observers, then necessarily the transformations between these observers must be deformed (they cannot be the ordinary linear Lorentz transformation rules). In this case one would speak of an instance in which the Lorentz transformations are deformed, but there is no preferred frame, the theory is still fully relativistic [49]. Having deformed the transformation rules between observers one must also necessarily [49] deform the rules for energy-momentum conservation (these rules are "laws of physics, in the Galilei sense, and must therefore be the same for all inertial observers).

Most work in this area has been devoted to the case in which Lorentz invariance is actually broken, the possibility that Lorentz invariance might be deformed was introduced only very recently by this author [49,63,64,65,66]. An example in which all details of the deformed Lorentz symmetry have been worked out is the one in which one enforces as an observer-indepedent statement the dispersion relation

$$L_p^{-2} \left(e^{L_p E} + e^{-L_p E} - 2 \right) - \vec{p}^2 e^{-L_p E} = m^2 \ . \tag{8}$$

In leading (low-energy) order this takes the form

$$E^2 = \vec{p}^2 + m^2 - L_p E \vec{p}^2 \ . \tag{9}$$

The Lorentz transformations and the energy-momentum conservation rules are accordingly modified [65].

While the case of deformed Lorentz symmetry might exercise a stronger conceptual appeal (since it does not rely on a preferred class of inertial observers), for the purposes of this paper it is sufficient to consider the technically simpler (and, by the way, still more popular in the quantum-gravity community) context of broken Lorentz invariance. Upon admitting a breakup of Lorentz invariance it becomes

legitimate, for example, to adopt the dispersion relation (9) without deforming the rules for energy-momentum conservation. I will use this scenario to illustrate how a tiny (Planck-length suppressed) effect, such as the one described by (9), could be observed in certain experimental contexts.

3.1. *In-vacuo dispersion*

A deformation term of order $L_p E^3$ in the dispersion relation, such as the one in (9), leads to a small energy dependence of the speed of photons of order $L_p E$, by applying the relation $v = dE/dp$.

An energy dependence of the speed of photons of order $L_p E$ is completely negligible in nearly all physical contexts, but it can be significant [32,33] in the analysis of short-duration gamma-ray bursts that reach us from cosmological distances. For a gamma-ray burst a typical estimate of the time travelled before reaching our Earth detectors is $10^{17}s$. Microbursts within a burst can have very short duration, as short as $10^{-4}s$. We therefore have one of the "amplifiers" mentioned in Section 1: the ratio between time travelled by the signal and time structure in the signal is a (conventional-physics) dimensionless quantity of order $\sim 10^{17}/10^{-4} = 10^{21}$. It turns out that this "amplifier" is sufficient to study energy dependence of the speed of photons of order $L_p E$. In fact, some of the photons in these bursts have energies in the $10 MeV$ range and higher. For two photons with energy difference of order $10 MeV$ an $L_p E$ speed difference over a time of travel of $10^{17}s$ leads to a relative time-delay on arrival that is of order $10^{-4}s$, which would be detected [32,33] upon comparison of the structure of the signal in different energy channels. The next generation of gamma-ray telescopes, such as GLAST [67], will exploit this idea to search for energy dependence of the speed of photons of order $L_p E$.

3.2. *Modified thresholds*

Let us now consider another significant prediction that comes from adopting the dispersion relation (9). While in-vacuo dispersion, discussed in the preceding Subsection, only depends on the deformation of the dispersion relation, the effects considered in this Subsection (and the next) also depends on the rules for energy-momentum conservation, which, as announced, I shall for simplicity assume to be unmodified.

The point I want to make here is that also certain types of energy thresholds for particle-production processes may be sensitive to the tiny $L_p E^3$ modification of the dispersion relation I am considering for illustrative purposes.

Let us focus on a collision between a soft photon of fixed/known energy ϵ and a high-energy photon of energy E, whose value is to be determined assuming the conditions for threshold electron-positron pair production are met. It is useful to review briefly the usual calculation of the E threshold. One can optimize the calculation by starting with the observation that the photon-photon invariant evaluated in the lab frame should be equal to (among other things) the electron-positron invariant

evaluated in the center-of-mass frame:

$$(E + \epsilon)^2 - (P - p)^2 = 4m_e^2 \ , \tag{10}$$

which, after using the ordinary dispersion relation, turns into $4E\epsilon = 4m_e^2$. So the threshold condition is

$$E\epsilon = m_e^2 \ . \tag{11}$$

Notice that in going from (10) to (11) using the ordinary dispersion relation the leading-order terms of the type E^2 have cancelled out, leaving behind the much smaller (if $\epsilon \ll E$) term of order $E\epsilon$. This cancellation provides the "amplifier". The "amplifier" is E/ϵ. If the threshold condition in modified at order $L_p E^3$ the modification will be significant if $L_p E^3$ is comparable to $E\epsilon$. While we normally expect L_p-related effects to become significant when the particles involved have energy $1/L_p$, here the effect is already significant when $E \sim (\epsilon/L_p)^{1/2}$, which can be considerably smaller than $1/L_p$ if ϵ is small. In the specific case of the deformed dispersion relation (9), applying ordinary energy-momentum conservation one finds [40] the modified threshold relation

$$E\epsilon - L_p \frac{E^3}{8} = m_e^2 \ . \tag{12}$$

For $E \sim 10 TeV$ and $\epsilon \sim 0.01 eV$ the modification of the threshold is already significant. These values of E and ϵ are relevant for the observation of multi-TeV photons from certain Markarians [39,40]. This high-energy photons travel to us from very far and they travel in an environment populated by soft photons, some with energies suitable for acting as targets for the disappearance of the hard photon into an electron-positron pair. Depending on some properties (such as the density) of the far-infrared soft-photon background (which are still not fully known) the observation of multi-TeV photons from certain Markarians may appear to be surprising [39] within conventional relativistic astrophysics. The Planck-scale induced deformation term in Eq. (12), by shifting up the value of the threshold energy, could explain these observations from Markarians.

A similar argument can be applied to cosmic rays. The puzzling fact that cosmic rays are seen above the GZK limit can also be interpreted as a violation of a relativistic threshold and again the dispersion relation (9) combined with conventional energy-momentum conservation would lead to a prediction for the relevant threshold (the photopion-production threshold) in agreement with data [38,40].

These observations, preliminary as they are, may well be the first ever manifestation of Planck-scale physics. The fact that we can finally at least contemplate this hypothesis has increased interest[f] in the whole quantum-gravity phenomenology

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[f]It is not uncommon that preliminary data generate interest in related theory subjects, and in some cases the lessons learned through those theoretical studies outlast the possible negative evolution of the experimental situation. This author is familiar [68] with the theory work that was motivated by the so-called "centauro events". It is now widely believed that centauro events were a "mirage", but in the process we did learn that the formal structure of QCD allows the vacuum to be temporarily misaligned (disoriented chiral condensates) and the RHIC collider is conducting dedicated experiments.

research programme.

3.3. *Modified decay amplitudes*

But this is not all. There is even a third opportunity for doable experiments to look for a manifestation of the tiny, Planck-length suppressed, modification of the dispersion relation which I am considering as illustrative example of quantum-gravity phenomenology exercise. This third opportunity has to do with particle-decay amplitudes and I shall discuss it through the example of the decay of a pion into two photons. First let us try to understand why pion-decay into two photons could be so sensitive. This is interesting because in this case the "amplifier" takes an unexpected form.

Again it is useful to review the relevant derivation within ordinary relativistic kinematics. One can optimize the calculation by starting with the observation that the photon-photon invariant in the lab frame should be equal to the pion invariant:

$$(E + E')^2 - (\vec{p} + \vec{p}')^2 = m_\pi^2 \ . \tag{13}$$

Using the conventional relativistic dispersion relation this can be easily turned into a relation between the energy E_π of the incoming pion, the opening angle ϕ between the outgoing photons, and the energy E of one of the photons (the energy E' of the second photon is of course not independent; it is given by the difference between the energy of the pion and the energy of the first photon):

$$\cos(\phi) \;\;=\;\; \frac{2EE' - m_\pi^2}{2EE'} \ , \tag{14}$$

where indeed $E' = E_\pi - E$. The reader should notice that $\cos(\phi) \leq 1$, as required by the fact that ϕ is a real physical angle, for all values of E. Note however that typically (unless $E \simeq 0$ or $E \simeq E_\pi$) $m_\pi^2 \ll 2EE' \sim E_\pi^2/2$ and the equation for $\cos(\phi)$ as the form $\cos(\phi) = (2EE' - \Delta)/2EE'$. So the fact that $\cos(\phi) \leq 1$ for all values of E depends only on the fact that $\Delta > 0$, which is authomatically satisfied within ordinary relativistic kinematics through the prediction $\Delta = m_\pi^2$. A new kinematics predicting that $\Delta < 0$ for some values of E would have significant implications, and in order to render Δ negative it is sufficient to introduce a relatively small correction, a correction of order m_π^2.

This is what happens in the scheme I am considering. The deformed dispersion relation (9), when combined with ordinary energy-momentum conservation, modifies the relation between ϕ, E_π and E according to the formula [42]

$$\cos(\phi) \;\;=\;\; \frac{2EE' - m_\pi^2 + 3L_p E_\pi EE'}{2EE' + L_p E_\pi EE'} \ . \tag{15}$$

This relation shows that at high energies the phase space available to the decay is anomalously reduced: for given value of E_π certain values of E that would normally be accessible to the decay are no longer accessible (they would require $cos\theta > 1$).

This anomaly starts to be noticeable at pion energies of order $(m_\pi^2/L_p)^{1/3} \sim 10^{15} eV$, but only very gradually (at first only a small portion of the available phase space is excluded). Remarkably, this type of behaviour could explain [42] certain puzzling features [69] of the longitudinal development of the air showers produced by certain high-energy cosmic-rays.

Independently of whether or not this preliminary experimental encouragement is confirmed by more refined data on pion decay, it is important for the line of argument presented in this paper that this scheme for the analysis of pion decay is another example of a Planck-scale scheme in which the effects become significant well below the Planck scale. The effects are already significant at pion energies of order $(m_\pi^2/L_p)^{1/3} \sim 10^{15} eV$. The careful reader will notice that in this case the "amplifier" is E_π/m_π.

4. Laser-interferometric Foam Studies: an example of Studies of nonsystematic Quantum-Gravity Effects

In this Section I illustrate, focusing on effects associated with distance fuzziness, the type of issues that emerge in the analysis of nonsystematic quantum-gravity effects. Some differences with respect to the method of analysis of systematic effects will emerge.

4.1. *Distance fuzziness*

Let us consider the possibility that the concept of distance be fuzzy in the intuitive sense of spacetime foam studies (and in the technical operative sense of Section 2). A robust analysis would require some model of this fuzziness and of the mechanisms that bring it about, but top-to-bottom theories provide very little guidance on this point. The type of systematic effects analyzed in the previous Section is governed by symmetry principles, and on those at least some preliminary (and vague) guidance can be gotten from top-to-bottom theories, but on distance fuzziness we lack even that level of guidance.

I resort here to a strictly phenomenological approach. Let us consider an experiment in which a distance L plays a key role, meaning that one is either measuring L itself or the observable quantity under study depends strongly on L. Let us assume (as commonly done) that in quantum gravity there should be a fundamental limitation on the measurability of L, a new uncertainty principle, and let us characterize this limitation in terms of a mean square deviation σ_L^2. We will want to analyze the implications of various hypotheses for σ_L^2 and compare them to the type of sensitivities that are achievable in relevant experiments.

Two experimental contexts which could be promising in this respect are: the gamma-ray-burst context already considered in the previous Section, where a very large distance is involved but the spread of times of arrival is relatively small, and the context of laser interferometry, where a relatively large distance can be monitored with extreme accuracy.

If σ_L^2 is independent of the time of observation (and therefore independent of L) one is naturally led to the estimate $\sigma_L^2 \sim L_p^2$. It is easy to verify that this estimate of $\sigma_L^2 \sim L_p^2$ would not be observably large, even in our two most promising experimental contexts (the relevant sensitivities are several orders of magnitude below the required level).

If one goes beyond the constant-σ_L^2 assumption, it is natural to consider also a possible dependence of σ_L^2 on the time T of observation of L required by the experiment. This can be motivated in various ways [34], and it is in the spirit of certain discretized mechanisms of space-geometry time evolution that are emerging within the loop-quantum-gravity research programme (see, *e.g.*, Refs. [70,71]). Introducing dimensionless parameters A, B (to be determined experimentally) one can then write σ_L^2 as

$$\sigma_L^2 \simeq A L_p^2 + B L_p c T \ . \tag{16}$$

The previous remark on the case in which σ_L^2 is T-independent means that experimental limits on A are not significant. Let us consider the limits on B, and let us start with the context of gamma-ray bursts. As mentioned, for a gamma-ray burst a typical estimate of the time travelled before reaching our Earth detectors is $10^{17}s$ and microbursts within a burst can have very short duration, as short as $10^{-4}s$. It is easy to realize that this imposes that whatever fundamental "uncertainty" affects the relevant distance $c \cdot 10^{17}s$ it cannot be bigger than $c \cdot 10^{-4}s$. This corresponds to a limit on B which is of order $B < 10^{13}$. This limit does not appear to be particularly interesting, but in the other context, the one of laser interferometry, a somewhat more encouraging estimate emerges.

4.2. *Laser-interferometric limits*

For the context of gamma-ray bursts the "amplifier" of distance fuzziness is of order $10^{21} = (10^{17}s)/(10^{-4}s)$. A superficial analysis of modern laser interferometers would attribute to them a comparable "amplifier" estimate. In fact, one major and well-known quality of these modern interferometers (whose primary objective is the discovery of the classical-physics phenomenon of gravity waves) is their ability to detect gravity waves of amplitude $\sim 3 \cdot 10^{-19}m$ by careful monitoring of distances of order $\sim 3 \cdot 10^3 m$. This would lead to an "amplifier" which is of order 10^{22}. However, the correct way to characterize the sensitivity of an interferometer requires [34,35,72] the analysis of the power spectrum of the strain noise which is left over after all the sophisticated noise-reduction techniques have been applied. In modern interferometers this strain power-noise spectrum is of order $10^{-44} Hz^{-1}$ at observation frequencies of about $100Hz$, and in turn this implies [34] that for a gravity wave with $100Hz$ frequency the detection threshold is indeed around $\sim 3 \cdot 10^{-18}m$. But not all fluctuation mechanisms are smooth waves. An ideal wave deposits all its energy in the frequency band of observation that includes its own frequency of oscillation. Things work differently for other fluctuation mechanisms, and particularly for discrete fluctuation mechanisms.

The ansatz $\sigma_L^2 \sim BL_pT$, on which I am focusing for illustrative purposes, has the time dependence characteristic of random-walk process. Indeed one obtains $\sigma_L^2 = L_pT$ by assuming that the distances L betweeen the test masses of an interferometer be affected by Planck-length fluctuations of random-walk type occurring at a rate of one per Planck time ($\sim 10^{-44}s$). It is easy to verify [34] that such fluctuations would induce strain noise with power spectrum given by $L_pL^{-2}f^{-2}$. For $f \sim 100Hz$ and $L \sim 3 \cdot 10^3 m$ this corresponds to strain noise at the level $10^{-37}Hz^{-1}$, well within the reach of the sensitivity of modern interferometers.

Fluctuations genuinely at the Planck scale (the simple scheme I used to illustrate my point involves Planck-length fluctuations occurring at a rate of one per Planck time) can lead to an effect that, while being very small in absolute terms, is large enough for testing with modern interferometers. The careful reader will realize that this is due to the fact that a meaningful estimate of the "amplifier" in laser interferometers is obtained by combining the characteristic frequency of observation and the noise levels aspected within conventional physics $1/(f \cdot 10^{-44}Hz^{-1}) \sim 10^{42}$.

4.3. *Significance of the laser-interferometric limits*

When discussing this type of experimental programmes at conferences and similar occasions, one is often invited to express an opinion on the significance of the forthcoming laser interferometers for spacetime-foam studies. Of course, such an opinion is beyond the scopes of quantum-gravity phenomenology. This type of exercise in quantum-gravity phenomenology can only identify large "amplifiers" and establish their possible connection with effects that are of Planck-length magnitude. The next step[g] would be to analyze the physical context in terms of a "promising quantum-gravity theory". This step cannot be taken for not one, but two reasons: (i) we have no quantum-gravity theory whose "promise" relies on the successfull prediction of some experimentally verified experimental facts, and (ii) even if we wanted to attribute "promise" to the theories which have developed into appealing conceptual/mathematical structures, such as loop quantum gravity and string theory, we are faced with the fact that these theories are still unprepared to provide this type of physical estimates.

One interesting way to address the issue of "significance" can be based on at-

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[g]Actually, there could be an intermediate step between the strictly phenomenological dimensional analysis (without even a naive picture of the supporting mechanism) and the analysis within a "promising" theory: one could construct a naive picture of a quantum-gravity mechanism generating laser-intereferometric noise. But even this intermediate step is not easily taken in this context, because of the lack of any guidance, even just in terms of some symmetry principle. For example, one can analyze quantum-gravity noise in terms of noise in the paths of each of the N_γ individual photons of the beam, but this would not be a representation of a fundamental noise (not in the sense of the noise that ordinary quantum mechanics contributes to laser interferometry) because it would inevitably disappear in the $N_\gamma \to \infty$ limit (whereas the contribution to laser-interferometric noise of ordinary quantum mechanics is composed of two terms [72], one descreasing with N_γ and one growing with N_γ). Still, as I shall discuss in detail in Ref. [37], this picture in terms of the paths of individual photons might encourage interferometric studies with low-power lasers, in order to reduce the apparent suppression of the effect by $\sqrt{N_\gamma}$.

tempting to address the following question: is the next generation of laser inter-
ferometers really entering a new region of exploration? (are the new limits to be
obtained in those experiments crossing some meaningful sensitivity boundaries?) In
this sense one can state that these forthcoming experiments are significant. This
is best stated by writing a phenomenological formula for the strain noise power
spectrum:

$$\rho_h(f) = \frac{\alpha L_p}{c} + \frac{L_p}{\Lambda_1 f} + \frac{cL_p}{\Lambda_2^2 f^2} + + \; ... \; , \tag{17}$$

where $\alpha, \Lambda_1, \Lambda_2$ parametrize our ignorance of the coefficients (they should be pre-
dicted by theory or measured) and the choice of notation emphasizes the fact that
the first term requires a dimensionless coefficient, while the second term and the
third term require coefficients with units of inverse-length and inverse-square-length
respectively.

The three terms included in (17) are just indicative. A fully general phenomenol-
goical formula should involve many more types of f depedence and the possibility
that the spectrum might not be linear in L_p (*e.g.* it could go like L_p^2). However,
(17) allows us to characterize in phenomenological quantitative terms the type of
sensitivity thresholds that are being reached with the next generation of laser in-
terferomters. The next generation of laser interferometers will have sensitivity that
goes down to α even smaller than 1, whereas until, say, a decade ago the sensitivity
was several orders of magnitude away from $\alpha = 1$. Similarly the next generation
will have sensitivity that goes down to values of Λ_1 and Λ_2 as large as the optical
length of the arms of the interferometer, whereas until a decade ago the sensitivity
was several orders of magnitude away from these levels.

The lack of theoretical guidance does not allow us to form any justifiable opinion
about the "theory significance" of the limits that will be obtained by the next gen-
eration of laser interferometers, but at the phenomenological level we can recognize
that some meaningful sensitivity thresholds will be reached by these forthcoming
experiments (and instead these sensitivity thresholds were totally unaccessible to
the previous generation of laser interferometers). Perhaps most important for future
developments is the fact that this analysis exposed the fact that a meaningful esti-
mate of the "amplifier" in laser interferometers must depend strongly on whether
the fluctuations are smooth or discretized. For discretized fluctuation mechanisms
the "amplifier" could be as high as 10^{42}. This is particularly significant since one
of the objectives of quantum-gravity phenomenology (see Section 1) should be the
one of establishing which experimental contexts are able to set the most stringent
limits (independently of the "significance" of these limits, which requires at this
stage some opinion about the workings of quantum gravity) on each of the effects
that have surfaced in the quantum-gravity literature. The list of such effects is
very limited (only very few entries) and it seems necessary from a strictly scientific
viewpoint, to establish where we are in the exploration of each of these effects.
The observations reported here (and in Refs. [34,35,37]) are in this sense noteworthy
since they indicate that in some theories of quantum gravity laser-interferometric

limits on distance fuzziness will be more stringent than the corresponding limits obtainable with gamma-ray-burst analysis, in spite of the fact that a naive estimate of the amplifiers in these two contexts would suggest that they have comparable sensitivity.

4.4. *Futility of the Salecker-Wigner debate*

The phenomenology of distance fuzziness is, as emphasized, already rendered more delicate by the fact that top-to-bottom theories are totally unable to provide us any guidance (whereas in the case of the systematic effects considered in the previous Section one could at least rely on some emerging intuition concerning the faith of Lorentz symmetry). Somehow the debate got also partly penalized by the attention some authors devoted to a potentially relevant argument due to Salecker and Wigner [73], which is however not needed in order to justify a quantum-gravity analysis of laser interferometers.

The Salecker-Wigner argument is a heuristic argument which leads to some intuition on new limits on the measurability of distances, something which of course is potentially relevant for the issue here being considered. In previous papers on this subject this author did mention this Salecker-Wigner argument as one of the arguments motivating interest in these laser-interferometric studies. The Salecker-Wigner argument, as all heuristic arguments, is not immune from criticism or at least skepticism, and it is of course not surprising that in Refs. [74,75] certain alternative ideas (actually rather naive ideas [76], but this is not the point here) on how the Salecker-Wigner setup should be properly analyzed were presented. What is really surprising is that, motivated exclusively by these views on the Salecker-Wigner setup, Refs. [74,75] argued that phenomenological interest in laser-interfeometric studies would not be justifiable. As shown above a certain level of interest (although, as emphasized, not necessarily in the sense of "theory significance") in these laser-interferometric studies can be justified at a strictly phenomenological level, without any reference to the Salecker-Wigner argument. This point had already been articulated in detail in Ref. [35] (which preceded Refs. [74,75]) but was somehow missed by the authors of Refs. [74,75] (which in fact do not include a citation of Ref. [35] in their reference lists).

As a way to reduce the confusion generated by the peculiar development of this debate involving the Salecker-Wigner argument and laser-interferometric studies, I have here chosen to refrain completely from any reference to the Salecker-wigner argument in motivating laser-interferometric quantum-gravity phenomenology.

5. Outlook

A good measure of the pace of development of quantum-gravity phenomenology can be obtained by comparing the number of ideas that needed to be covered by this author in the previous review some three years ago [1] and the number of ideas that have been covered (however briefly) in this review. Moreover, even some of the

ideas that were already under consideration three years ago are now studied and understood at a much deeper level.

On the theory side, perhaps the most promising research programme remains the one concerning symmetries. It appears safe to bet that over the next few years the theoretical study of the faith of classical-spacetime symmetries in quantized (discretized, noncommutative,...) descriptions of spacetime will produce even more exciting results.

But for the next ten years we should be even more optimistic about progress on the experimental front. Laser interferometry, discussed in the previous Section, will go through a remarkable upgrade through the operation of LIGO, VIRGO, and (hopefully) LISA. Although at a slower pace also tests of CPT symmetry will keep improving.

Perhaps the most exciting data we will get are the ones pertaining Lorentz symmetry. The preliminary evidence of deviations from conventional Lorentz invariance (here discussed in Section 3), which resides primarily (and most robustly) in cosmic-ray observations, will be put under severe scrutiny with new cosmic-ray observatories, such as Auger. If the preliminary evidence is confirmed by these more powerful cosmic-ray observatories one could then support (or disprove) the new-kinematics interpretation of the GZK puzzle through the results of searches of the corresponding in-vacuo dispersion with the next generation of gamma-ray observatories, such as GLAST [67].

Acknowledgments

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