Introduction to Quantum-Gravity Phenomenology

Giovanni AMELINO-CAMELIA

Dipart. Fisica Univ. La Sapienza and Sez. Roma1 INFN
P.le Moro 2, I-00185 Roma, Italy

ABSTRACT

After a brief review of the first phase of development of Quantum-Gravity Phenomenology, I argue that this research line is now ready to enter a more advanced phase: while at first it was legitimate to resort to heuristic order-of-magnitude estimates, which were sufficient to establish that sensitivity to Planck-scale effects can be achieved, we should now rely on detailed analyses of some reference test theories. I illustrate this point in the specific example of studies of Planck-scale modifications of the energy/momentum dispersion relation, for which I consider two test theories. Both the photon-stability analyses and the Crab-nebula synchrotron-radiation analyses, which had raised high hopes of "beyond-Plankian" experimental bounds, turn out to be rather ineffective in constraining the two test theories. Examples of analyses which can provide constraints of rather wide applicability are the so-called “time-of-flight analyses”, in the context of observations of gamma-ray bursts, and the analyses of the cosmic-ray spectrum near the GZK scale.
1 From “Quantum Gravity beauty contests” to 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 gravity and quantum mechanics. In other research areas the availability of experimental data challenging the current theories encourages theorists to propose phenomenological models which solve the experimental puzzles, even when some aspects of the models are not fully satisfactory from a conceptual perspective. 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 (occasionally mentioned at relevant seminars) 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 could give some “scientific legitimacy” to these research programmes, but this possibility never materialized. It may indeed be just a mirage. There are several occasions when a debate between advocates of different ambitious quantum-gravity theories shapes up in a way similar to the discussion between advocates of different religions. And often in the media the different approaches are compared on the basis of the “support” they have in the community: one says “the most popular approach to the quantum gravity problem” rather than “the approach that has had better success reproducing experimental results”. So, it would seem, the Quantum Gravity problem is to be solved by an election, by a beauty contest, by a leap of faith.

Over the last few years 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.

Quantum gravity phenomenology requires of course a combination of theory and experiments. It does not adopt any particular prejudice concerning the structure of spacetime at short distances (in particular, “string theory” \[2, 3\], “loop quantum gravity” \[4, 5, 6, 7\] and “noncommutative geometry” \[8, 9\] are seen as equally deserving mathematical-physics programmes), but of course must follow as closely as possible the few indications that these ambitious quantum-gravity theories provide. One here is guided by the expectation that quantum-gravity research should proceed just in the old-fashioned way of scientific work: 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).

In these lectures I give a “selected-topics” introduction to this “Quantum Gravity Phenomenology”. I will in particular stress that, while the first few years of work in this area, the “dawn” of quantum-gravity phenomenology \[10\], were necessarily based on rather preliminary analyses, with the only objective of establishing the point that Planck-scale sensitivity could be achieved in some cases, we should now gear up for a more “mature” phase of work on quantum-gravity phenomenology, in which the development and analysis of some carefully crafted test theories takes center stage.
2 Quantum Gravity Phenomenology

In this section I describe the key objectives of quantum-gravity phenomenology and sketch out its strategy in the search of the first manifestation of a quantum property of spacetime. I also start introducing my argument that we should now move from the “dawn” of quantum-gravity phenomenology to a more “mature” quantum-gravity phenomenology, in which a key role is played by the development and analysis of some carefully crafted test theories.

2.1 Planck-scale quantum properties of spacetime

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 the Planck length $L_p \sim 10^{-35}$ m (or equivalently its inverse, the Planck scale $E_p \sim 10^{28}$ eV). The key challenge for quantum-gravity phenomenology must be the one of establishing ways to provide sensitivity to Planck-scale non-classical properties of spacetime.

I will call “quantum” properties of spacetime all effects which represent departures from a classical picture of spacetime. This is after all what is commonly done in the literature, where authors often use the name “quantum properties of spacetime” because of the expectation that some of the familiar features of quantization, which showed up everywhere else in physics, should eventually also play a role in the description of spacetime. There is no guarantee that the non-classical properties of spacetime will take the shape of some sort of proper spacetime quantization. But, as long as this is understood, the use of the spacetime-quantization terminology does no harm.

Of course, the search of a solution of the quantum-gravity problem can benefit also from other types of experimental insight, and therefore the scopes of quantum-gravity phenomenology must go even beyond its key quantum-spacetime challenge. In particular, quantum gravity should also provide a consistent description of the quantum properties of particles in presence of strong (or anyway nonnegligible) classical gravity fields. This type of context at the “Interface of Quantum and [classical] Gravitational Realms” has been the subject of a rather sizeable literature for several decades. When quantum properties of spacetime are not relevant for the analysis the insight one can gain for the quantum-gravity problem is of more limited impact, but it is of course still valuable. Indeed a valuable debate on the fate of the Equivalence Principle in quantum gravity was ignited already in the mid 1970s with the renowned experiment performed by Colella, Overhauser and Werner. 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

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

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 and as a result, in spite of its per-particle weakness, the overall gravitational field is large enough\(^a\) to introduce observable effects. The relevance of these experiments for the debate on the Equivalence Principle will not be discussed here, but has been discussed in detail by several authors (see, e.g., Refs. [14, 15, 16]). I here just bring to the reader’s attention a recent experiment which appears to indicate a violation of the Equivalence Principle [17] (but the reliability of this experimental result is still being debated), and some ideas for intriguing new experiments [13, 18] of the COW type. I should also mention for completeness the related work on the interplay between classical general relativity and quantum mechanics of nongravitational degrees of freedom reported in Refs. [19, 20].

Another possibility that, even though it is in contrast with the idea of Planck-scale quantum properties of spacetime, deserves some exploratory effort by those working in quantum-gravity phenomenology is the one of scenarios in which the standard estimate of the quantum-gravity scale as the Planck scale turns out to be too pessimistic. There is (at present) no compelling argument in support of the idea that the quantum-gravity scale should be effectively lowered, but this possibility cannot be excluded. In particular, some recent studies [21] found a mechanism that would allow to lower significantly the quantum-gravity energy scale, several orders of magnitude below the Planck energy scale. This mechanism relies of the hypothesis of “large extra dimensions” which is not in any way “natural” (not even in the eyes of the scientists who proposed it), but it can be used to provide an example of a workable scenario for a low scale of quantum-gravity effects.

For the rest of these lectures I will however focus on what I described as the key challenge: the search of Planck-scale quantum properties of spacetime.

\(^a\)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 [13] involves the ratio between the wavelength of the particles used in the COW experiments and some larger length scales involved in the experimental setup.
2.2 Identification of experiments

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 in the quantum-gravity literature. 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 are not very promising for the actual (experimental) discovery of manifestations of non-classical properties of spacetime.

While it is likely that the largest quantum-gravity effects should be present in large-curvature situations, it only takes a little reasoning to realize that we should give priority to quantum-gravity effects that modify our description of (quasi-)Minkowski spacetime. The effects will perhaps be smaller 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 quasi-Minkowski spacetimes, for which the quality of the data we can obtain is extremely high.

In the analysis of flat-spacetime processes, involving particles with energies that are inevitably much lower than the Planck energy scale, we will have to deal with 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 $L_p/\lambda$ 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 of Planck-scale effects with unmitigated pessimism [22].

However, the presence of large suppression factors surely cannot suffice for drawing any conclusions. 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, where some unobservably
small micro-processes lead to an effect which is observable on macroscopic scales.

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 entire 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 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 on experiments which have something to do with spacetime structure 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.

2.3 Prehistory of quantum gravity phenomenology

Clearly a good phenomenological programme must be able to falsify theories. Although it is already noteworthy that some candidate quantum-gravity effects could at all be looked for in the data, this would not be so significant if we were not able to use these data to constrain the work of theorists, to falsify some theoretical pictures. The fact that, toward the end of the 1990s, it was convincingly argued that this could be done brought the idea of “Planck-scale tests” to center stage in quantum-gravity research. The fact that we could plausibly gain insight on Planck-scale physics is now widely acknowledged in the quantum-gravity community. Up to 1997 or 1998 there had already been some works on the possibility to find experimental evidence of some Planck-scale effects, but the relevant data analyses did not in reverse have the capability to falsify any quantum-gravity picture and the relevant research remained at the margins of the mainstream quantum-gravity literature.

A first example of these works of the “prehistory of quantum-gravity phenomenology” is provided by a certain type of investigation of Planck-scale
departures from CPT symmetry using the neutral-kaon and the neutral-B systems \[23, 24, 25, 26\]. These pioneering works were based on the realization that in the relevant neutral-meson systems a Planck-scale departure from CPT symmetry could in principle be amplified; in particular, the neutral-kaon system hosts the peculiarly small mass difference between long-lived and the short-lived kaons \(|M_L - M_S|/M_{L,S} \sim 7 \cdot 10^{-15}\). The quantum-gravity picture usually advocated in these studies is the one of a variant of the string-theory picture, which relies on noncritical strings, in the so-called “Liouville” approach \[25, 27\]. This is an ambitious attempt for a theory of everything, which, while based on an appealing view of the quantum-gravity problem, is for the most part untreatable, at least with current techniques. The departures from CPT symmetry cannot be derived from the theory, but one can provide tentative evidence that the structure of the theory should accommodate such departures. As a result one is forced to set up a multi-parameter phenomenology which looks for the new effects, but a negative outcome of the experiments could not be used to falsify the framework which is at the root of the analysis.

Similar remarks apply to the other pioneering studies reported in Refs. \[28\], which find their original motivation in some aspects of “string field theory” \[28\]. Also the string-field-theory formalism is very ambitious and too hard to handle. A multi-parameter phenomenology is necessarily set up \[28\], and a negative outcome of the experiments could not be used to falsify the framework which is at the root of the analysis. Besides the falsifiability issue, this phenomenology may not appeal to many quantum-gravity researchers because it mainly focuses on the “Standard Model Extension”, whose key assumption \[29\] is the renormalizability of the underlying field theory. The assumption of renormalizability limits one to effects that area described in terms of operators of dimension 4 and lower, whereas most quantum-gravity researchers expect Planck-scale-suppressed effects described in terms of operators of dimension 5 and higher.

A third equally-deserving entry in my list of pioneers of the “prehistory of quantum-gravity phenomenology” is the work reported in Refs. \[30, 31\] which explored the general issue of how certain effectively stochastic properties of spacetime would affect the evolution of quantum-mechanical states. The guiding idea was that stochastic processes could provide an effective description of quantum spacetime processes. The implications of these stochastic properties for the evolution of quantum-mechanical states were modelled in Refs. \[30, 31\] via the formalism of “primary state diffusion”, but only rather crude models turned out to be treatable. As also emphasized by the authors, the crudeness of the models is such that all conclusions are to be considered as tentative at best, and this is one more instance in which a negative outcome of the experiments could not be used to falsify the framework which is at the root of the analysis.

The three research lines I discussed in this subsection as examples of “prehistory of quantum-gravity phenomenology” showed convincingly that
the possibility of stumbling upon an experimental manifestation of Planck-scale effects could not be excluded. On the other hand they proved to be insufficient for the birth a genuine, fully articulated, phenomenological programme. In that regard their key common limitation was the mentioned fact that it appeared that the relevant experiments could not falsify the relevant theories. Moreover, it often appeared that these studies were establishing that some not-much-studied quantum-gravity approaches could lead to observable effects, as a way to distinguish them from the most popular quantum-gravity ideas which would remain untestable. Of course, the interest of the community grew when it became apparent that a rather large variety of quantum-gravity ideas could lead to observable effects (and could be falsified). A sizeable community now works under the assumption that the presence of observably-large quantum-gravity effects is not a peculiar feature of some out-of-mainstream quantum-gravity approaches: it is a property of most quantum-gravity approaches, including some of the most popular ones.

2.4 The dawn of quantum gravity phenomenology

The research lines discussed in the previous subsections had been establishing that it was not inconceivable to use data within our reach (inevitably involving particles with energies much lower than the Planck energy scale) to find evidence of a Planck-scale effect. However, while these research did ignite a lively interest by some experimentalists (see, e.g., Refs. [32, 33]), they went largely unnoticed by mainstream quantum-gravity research. As stressed above, this was likely due to the fact that they were incomplete proposals from the viewpoint of phenomenology, because the test theories could not be really falsified, and because the relevant test theories were all outside mainstream quantum-gravity research, so that the fact that Planck scale effects could be seen appeared to be a peculiar property of out-of-mainstream theories. On the other hand, clearly those research lines were starting set the stage for a wider and more developed phenomenological effort, which indeed came to existence toward the end of the 1990s. When, indeed starting toward the end of the 1990s, the case for falsifiability of some Planck-scale models started to be built, and first evidence of testability of mainstream quantum-gravity proposals emerged, a corresponding quick growth of interest emerged in the community. This is perhaps best illustrated by comparing authoritative quantum-gravity reviews published up to the mid 1990s (see, e.g., Ref. [22]) and the corresponding reviews published over the last couple of years [34, 35, 36, 37].

This “dawn” of quantum-gravity phenomenology has revolved around a growing number of experimental contexts in which Planck-scale effects are being sought. Among the most popular such proposals let me mention, as a few noteworthy examples, the studies of in-vacuo dispersion using gamma-ray astrophysics [38, 39], studies of laser-interferometric limits on quantum-gravity effects [40, 41, 42, 43, 44], studies of the role of quantum-gravity
effects in the determination of the energy-momentum-conservation threshold conditions for certain particle-physics processes \cite{45, 46, 47, 48, 49}, and studies of the role of quantum gravity in the determination of particle-decay amplitudes \cite{50, 51}.

The idea of looking for Planck-scale departures from CPT symmetry continues to be pursued, but in that context we are still lacking an analysis showing how a quantum-gravity model could be falsified on the basis of such CPT studies. This is essentially due to some technical challenges in establishing what exactly happens to CPT symmetry within a given Planck-scale picture. It is often easy to see that CPT is affected, but one is then unable to establish how it is affected.

As I shall stress again later, among all these research lines a special role in the development of quantum-gravity phenomenology is being played by studies of the role of quantum-gravity effects in the determination of the energy-momentum-conservation threshold conditions for certain particle-physics processes. In fact, in these studies we have stumbled upon a first example of experimental puzzle whose solution could plausibly be sought within quantum-gravity phenomenology. This of course marked an important milestone for quantum-gravity phenomenology. The relevant context is the one of the process of photopion production, \( p + \gamma \rightarrow p + \pi^0 \), which, as discussed later in these lectures, plays a crucial role in the analysis of the cosmic-ray spectrum. An apparent “anomaly” in the observed cosmic-ray spectrum could be naturally described in terms of Planck-scale effects. Of course, it is not unlikely that this “anomaly” might fade away, as better data on cosmic rays become available, but it is nonetheless an important sign of maturity for quantum-gravity phenomenology that some data invite interpretation as a possible manifestation of Planck-scale physics. Chances are the first few such “candidate anomalies” will turn out to be incorrect, but eventually one lucky instance could be encountered.

### 2.5 The maturity of Quantum Gravity Phenomenology: test theories

The fact that quantum-gravity phenomenology is already being considered in attempts to solve present experimental puzzles is indeed a clear indication of progress toward the maturity of the field, but in many respects the field is still rather immature. The first challenge for quantum-gravity phenomenologists was to establish convincingly that there is a chance to test Planck-scale effects, and this type of argument can legitimately be based on intuitive order-of-magnitude analyses. However, at this point a rather large community acknowledges that quantum-gravity phenomenology has a chance, so the first challenge was successfully overcome, and we must now shift gear. There is very little more to be gained through rudimental back-of-the-envelope analyses. The standards of quantum-gravity phenomenology
must be raised to the ones adopted in other branches of phenomenology, such as particle-physics phenomenology.

In these lectures I shall in particular emphasize the importance of adopting some reference test theories. If an effect is described only vaguely, without the support of an associated test theory, then the experimental limits that can be claimed are of correspondingly uncertain significance. As it has happened in the recent quantum-gravity-phenomenology literature, different authors may end up claiming different limits on “the same effect” simply because they are actually adopting different test theories and therefore they are truly analyzing different effects. This type of phenomenology clearly would not help us gain any insight on Planck-scale physics. The main task of phenomenology is to provide to the theorists working at the development of the theories information on what is and what is not consistent with experimental data. Phenomenology essentially provides some boundaries within which formal theorists are then forced to work. A theory which would predict effects inconsistent with some data is abandoned. But if this boundaries are not clearly drawn, if the experimental limits are placed on “effects” which are not rigorously defined within the context of a test theory, then they are correspondingly useless for the development of theories.

The discussion here reported in Section 4 will illustrate this point in a specific context.

3 Some candidate Quantum-Gravity effects

Before focusing, in the next section, on an example of “quantum-gravity-phenomenology exercise”, it seems appropriate to list at least a few of the candidate quantum-gravity effects that find motivation in the literature.

Testing these effects will be the main task of quantum-gravity phenomenology. While here I will discuss these effects at a rather rudimentary and intuitive level, so that my remarks would apply to a variety of approaches to the quantum-gravity problem, clearly in each theory these effects may take a different form, and in setting up a phenomenology for these effects it will be crucial to develop some corresponding test theories.

In providing motivation for the study of these effects I could use a large variety of arguments; however, I find preferable to show that these effects can be motivated already on the basis of the most plausible of all hypotheses concerning the quantum-gravity problem: the hypothesis that some of the incarnations of the “quantum” idea (such as discretization and noncommutativity of observables) should find place also in the description of spacetime.

3.1 Planck-scale departures from Lorentz symmetry

Perhaps the most debated possibility for a quantum spacetime, possibly intended as Planck-scale discrete or Planck-scale noncommutative spacetime, is the one of Planck-scale departures from 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 scale $c$. If one introduces additional structure in a flat spacetime its symmetries will be accordingly affected. This is particularly clear for some simple ideas concerning a Planck-scale discretization of spacetime \[52\]. Continuous symmetry transformations are clearly at odd with a discrete network of points.

For different reasons, Lorentz symmetry is also often at odds with spacetime noncommutativity. In particular, it appears that in certain cases the noncommutativity length scale \[53\] (possibly the Planck scale), in addition to $c$, affects the laws of transformation between inertial observers, and infinitesimal symmetry transformations are actually described in terms of the new language of Hopf algebras \[54, 55\], rather than by the Poincaré Lie algebra. The type of spacetime quantization provided by noncommutativity may therefore lead to a corresponding “symmetry quantization”: the concept of Lie-algebra symmetry is in fact replaced by the one of Hopf-algebra symmetry.

In a large number of recent studies of noncommutative spacetimes it has indeed been found that the Lie-algebra Poincaré symmetries are either broken to a smaller symmetry Lie algebra or deformed into Hopf-algebra symmetries.

For what concerns the idea of spacetime discretization the most developed quantum-gravity picture is the one of Loop Quantum Gravity, which does not predict a rigid discrete network of spacetime points, but introduces discretization in a more sophisticated way: the spectra of areas and volumes are discretized, while spacetime points loose all possible forms of identity. It appears that even this more advanced form of discretization is incompatible with classical Lorentz symmetry; in fact, a growing number of loop-quantum-gravity studies has been reporting \[56, 57\] evidence of Planck-scale departures from Lorentz symmetry (although the issue remains subject to further scrutiny).

### 3.2 Planck-scale departures from CPT symmetry

The fact that our low-energy\(^b\) observations are consistent with CPT symmetry is not a miracle: as codified by the CPT theorem, a Lorentz-invariant local quantum field theory is inevitably CPT invariant. The fact that quantum gravity, the “unification” of gravity and quantum theory, invites us to consider Planck-scale departures from Lorentz symmetry (as stressed above)

\(^b\)Since it is often obvious from the context, I will sometimes avoid specifying “energies that are low with respect to the Planck energy scale” and simply write “low-energy”. With “high-energy particles” instead I will not mean “particles with energy higher than the Planck energy scale” (a situation which we never encounter), but rather the case of particles with energy rather close to (but still lower than) the Planck scale.
and Planck-scale departures from locality (as natural in a discrete-spacetime theory) opens the door for Planck-scale departures from CPT symmetry.

While this general argument is rather robust, it is not always easy to establish what is the fate of CPT symmetry in a given quantum-gravity approach. For example in Loop Quantum Gravity the analysis of (the various alternative ideas) on coupling ordinary particles to gravity has not yet advanced to the point of allowing a robust description of C transformations. On the other hand there are examples in which some progress in the analysis of CPT transformations has been achieved and evidence of departures from CPT symmetry is found. This is for example the case of \( \kappa \)-Minkowski noncommutative spacetime, where one can clearly see a modification of P transformations.

Since I am not considering CPT symmetry in the remainder of these lectures let me mention here that, besides the neutral-kaon and neutral-B systems, already briefly discussed in the previous section, also neutrinos are being considered as a possible laboratory for tests of Planck-scale departures from CPT symmetry.

### 3.3 Distance fuzziness

As one last example of effect that one could plausibly expect from quantum gravity, I consider here “distance fuzzyness”. Once again one is exploring the possibility that some ideas from quantum theory would apply to spacetime physics. A key characteristic of quantum theory is the emergence of uncertainties, and one might expect that the “distance observable” would also be affected by uncertainties. Actually various heuristic arguments suggest that for such a “distance observable” the uncertainties might be more pervasive: in ordinary quantum theory one is still able to measure sharply any given observable, though at the cost of renouncing all information on a conjugate observable, but it appears plausible that a quantum-gravity “distance observable” would be affected by irreducible uncertainties. Most authors would consider a \( \delta D \geq L_p \) relation, meaning that the uncertainty in the measurement of distances could not be reduced below the Planck-length level, but measurability bounds of other forms, generically of the type \( \delta D \geq f(D, L_p) \) (with \( f \) some function such that \( f(D, 0) = 0 \)) are also being considered.

The presence of such an irreducible measurement uncertainty could be significant in various contexts. For example, these ideas would suggest that the noise levels in the readout of a laser interferometer would receive an irreducible (fundamental) contribution from quantum-gravity effects. Interferometric noise can in principle be reduced to zero in classical physics, but already the inclusion in the analysis of the ordinary quantum properties of matter introduces an extra noise contribution with respect to classical physics. A fundamental Planck-scale-induced uncertainty in the length of the arms of the interferometer would introduce another source of noise, and the possibility of testing this idea is presently under investigation (see, e.g., Refs. [40] [41] [42] [43]).
3.4 Aside on the differences between systematic and nonsystematic effects

It is perhaps useful to stress the differences between systematic and nonsystematic Planck-scale effects, which I can illustrate using the type of effects discussed in the previous parts of this section.

An example of systematic effect is given by the departures from Lorentz symmetry encountered in certain noncommutative spacetimes (on which I shall return later in these lectures). There the Planck-scale structure of spacetime can introduce a systematic dependence of the speed of photons on their wavelength. After a journey of duration $T$ the difference between the expected position of the photon and the Planck-scale-corrected position could take the form $\Delta x \sim T \delta v \sim cTL_p/\lambda$, where $\lambda$ is the photon wavelength.

If we instead focus on how “distance fuzziness” could affect the propagation of photons it is natural to expect that a group of photons would all travel the same average distance in a given time $T$ (and this average distance is still given by $cT$), but for each individual photon the distance travelled might be slightly different from the average, as a result of distance fuzziness. This is an example of nonsystematic effect. Just to be more specific let us imagine that distance fuzziness effectively introduces a Planck-length uncertainty in position per each Planck time of travel. Then the final position uncertainty would be of the type $\Delta x \sim \sqrt{cTL_p}$. The square root here (assuming a random-walk-type description) is the result of the fact that nonsystematic effects do not add linearly, but rather according to rules familiar in the analysis of stochastic processes.

4 A prototype exercise: modified dispersion relations

In the previous sections I tried to give a general, but rough, description of how one works in quantum-gravity phenomenology. I will now discuss a specific example of quantum-gravity-phenomenology study, with the objective of illustrating in more detail the type of challenges that one must face and some strategies that can be used. The example I am focusing on is the one of Planck-scale modifications of the energy-momentum dispersion relation, which has been extensively studied from the quantum-gravity-phenomenology perspective. I will start with a brief description of how modified dispersion relations arise\(^c\) in the study of noncommutative spacetimes

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\(^c\)I discuss noncommutative spacetimes and the Loop Quantum Gravity approach, which are the best understood Planck-scale frameworks in which it appears that the dispersion relation is Planck-scale modified. But other types of intuitions about the quantum-gravity problem may lead to modified dispersion relations, including some realizations of the
and in the study of loop quantum gravity. I will then discuss some test theories which might play a special role in the development of the relevant phenomenology. And finally I will discuss some observations in astrophysics which can be used to set limits on the test theories.

4.1 Modified dispersion relations in canonical noncommutative spacetime

The noncommutative spacetimes in which modifications of the dispersion relation are being most actively considered all fall within the following rather general parametrization of noncommutativity of the spacetime coordinates:

\[ [x_\mu, x_\nu] = i\theta_{\mu\nu} + i\rho^\beta_{\mu\nu} x_\beta. \]  

(2)

It is convenient to first focus on the special case \( \rho = 0 \), the “canonical noncommutative spacetimes”

\[ [x_\mu, x_\nu] = i\theta_{\mu\nu}. \]  

(3)

Of course, the natural first guess for introducing dynamics in these spacetimes is a quantum-field-theory formalism. And indeed, for the special case \( \rho = 0 \), an approach to the construction of a quantum field theory has been developed rather extensively\(^64, 65\). While most aspects of these field theories closely resemble their commutative-spacetime counterparts, a surprising feature that emerges is the so-called “IR/UV mixing”\(^64, 65, 66\): the high-energy sector of the theory does not decouple from the low-energy sector. Connected with this IR/UV mixing is the type of modified dispersion relations that one encounters in field theory on canonical noncommutative spacetime, which in general take the form

\[ m^2 \simeq \mathcal{E}^2 - \mathbf{p}^2 + \frac{\alpha_1}{p^\mu p_\mu} + \alpha_2 m^2 \ln (p^\mu \theta_{\mu\nu} \theta_{\nu\sigma} p_\sigma) + \ldots, \]  

(4)

where the \( \alpha_i \) are parameters, possibly taking different values for different particles (the dispersion relation is not “universal”), that depend on various aspects of the field theory, including its field content and the nature of its interactions. The fact that this dispersion relation can be singular in the infrared is a result of the IR/UV mixing. A part of the infrared singularity could be removed by introducing (exact) supersymmetry, which typically leads to \( \alpha_1 = 0 \).

The implications of this IR/UV mixing for dynamics are still not fully understood, and there is still justifiable skepticism\(^67\) toward the correctness idea of “spacetime foam”\(^38, 62, 27\), which allow an analogy with the laws of particle propagation in a thermal environment\(^38, 62, 53\).
of the type of field-theory construction adopted so far. I think it is legitimate to even wonder whether a field-theoretic formulation of the dynamics is at all truly compatible with the canonical spacetime noncommutativity. The Wilson decoupling between IR and UV degrees of freedom is a crucial ingredient of most applications of field theory in physics, and it is probably incompatible with canonical noncommutativity: the associated uncertainty principle of the type $\Delta x_{\mu} \Delta x_{\nu} \geq \theta_{\mu\nu}$ implies that it is not possible to probe short distances (small, say, $\Delta x_1$) without probing simultaneously the large-distance regime ($\Delta x_2 \geq \theta_{2,1}/\Delta x_1$).

In any case, the presence of modified dispersion relations in canonical noncommutative spacetime should be expected, since Lorentz symmetry is “broken” by the tensor $\theta_{\mu\nu}$. An intuitive characterization of this Lorentz-symmetry breaking can be obtained by looking at wave exponentials. The Fourier theory in canonical noncommutative spacetime is based on simple wave exponentials $e^{i p_{\mu} x_{\mu}}$ and from the $[x_{\mu}, x_{\nu}] = i \theta_{\mu\nu}$ noncommutativity relations one finds that

$$e^{i p_{\mu} x_{\mu}} e^{i k_{\nu} x_{\nu}} = e^{-\frac{i}{2} p_{\mu} \theta_{\mu\nu} k_{\nu} } e^{i (p+k)_{\mu} x_{\mu} },$$

i.e. the Fourier parameters $p_{\mu}$ and $k_{\mu}$ combine just as usual, but there is the new ingredient of the overall $\theta$-dependent phase factor. The fact that momenta combine in the usual way reflects the fact that the transformation rules for energy-momentum from one (inertial) observer to another are still the familiar, undeformed, Lorentz transformation rules. However, the product of wave exponentials depends on $p_{\mu} \theta_{\mu\nu} k_{\nu}$; it depends on the “orientation” of the energy-momentum vectors $p_{\mu}$ and $k_{\nu}$ with respect to the $\theta_{\mu\nu}$ tensor. The $\theta_{\mu\nu}$ tensor plays the role of a background that identifies a preferred class of inertial observers\(^d\). Different particles can be affected by the presence of this background in different ways, leading to the emergence of different dispersion relations. All this is consistent with indications of the mentioned popular field theories in canonical noncommutative spacetimes.

4.2 Modified dispersion relations in $\kappa$-Minkowski noncommutative spacetime

In canonical noncommutative spacetimes Lorentz symmetry is “broken” and there is growing evidence that Lorentz symmetry breaking occurs for most

\(^d\)Note that these remarks apply to canonical noncommutative spacetimes as studied in the most recent (often String-Theory inspired) literature, in which $\theta_{\mu\nu}$ is indeed simply a tensor (for a given observer, an antisymmetric matrix of numbers). I should stress however that the earliest studies of canonical noncommutative spacetimes (see Ref. \[69\] and follow-up work) considered a $\theta_{\mu\nu}$ with richer mathematical properties, notably with nontrivial algebra relations with the spacetime coordinates. In that earlier, and more ambitious, setup it is not obvious that Lorentz symmetry would be broken: the fate of Lorentz symmetry may depend on the properties (dynamics?) attributed to $\theta_{\mu\nu}$. 

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choices of the tensors $\theta$ and $\rho$. It is at this point clear, in light of several recent results, that the only way to preserve Lorentz symmetry is the choice $\theta = 0 = \rho$, i.e. the case in which there is no noncommutativity and one is back to the familiar classical commutative Minkowski spacetime. When noncommutativity is present Lorentz symmetry is usually broken, but recent results suggest that for some special choices of the tensors $\theta$ and $\rho$ Lorentz symmetry might be deformed, in the sense of the recently proposed “doubly-special relativity” scenario [53], rather than broken. In particular, this appears to be the case for the Lie-algebra $\kappa$-Minkowski [54, 55, 58, 71, 72, 73] noncommutative spacetime $(l, m = 1, 2, 3)$

$$[x_m, t] = \frac{i}{\kappa} x_m, \quad [x_m, x_l] = 0.$$  \hspace{1cm} (6)

$\kappa$-Minkowski is a Lie-algebra spacetime that clearly enjoys classical space-rotation symmetry; moreover, at least in a Hopf-algebra sense (see, e.g., Ref. [72]), $\kappa$-Minkowski is invariant under “noncommutative translations”. Since I am focusing here on Lorentz symmetry, it is particularly noteworthy that in $\kappa$-Minkowski boost transformations are necessarily modified [72]. A first hint of this comes from the necessity of a deformed law of composition of momenta, encoded in the so-called coproduct (a standard structure for a Hopf algebra). One can see this clearly by considering the Fourier transform. It turns out [58, 71] that in the $\kappa$-Minkowski case the correct formulation of the Fourier theory requires a suitable ordering prescription for wave exponentials. From

$$e^{ik\mu x_\mu} \equiv e^{ik^m x_m} e^{ik^0 x_0},$$ \hspace{1cm} (7)

as a result of $[x_m, t] = i x_m / \kappa$ (and $[x_m, x_l] = 0$), it follows that the wave exponentials combine in a nontrivial way:

$$\langle e^{ip\mu x_\mu} \rangle \langle e^{ik\mu x_\mu} \rangle = e^{i(p+k)\mu x_\mu}.$$ \hspace{1cm} (8)

The notation “$\langle \cdot \rangle$” here introduced reflects the behaviour of the mentioned “coproduct” composition of momenta:

$$p_\mu \dot{+} k_\mu \equiv \delta_{\mu,0}(p_0 + k_0) + (1 - \delta_{\mu,0})(p_\mu + e^{\lambda p_0} k_\mu).$$ \hspace{1cm} (9)

As argued in Refs. [53] the nonlinearity of the law of composition of momenta might require an absolute (observer-independent) momentum scale, just like upon introducing a nonlinear law of composition of velocities one must introduce the absolute observer-independent scale of velocity $c$. The inverse of the noncommutativity scale $\lambda$ should play the role of this absolute momentum scale. This invites one to consider the possibility [53] that the transformation laws for energy-momentum between different observers would have two invariants, $c$ and $\lambda$, as required in “doubly-special relativity” [53].

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On the basis of (9) one is led to the following result for the form of the energy/momentum dispersion relation

\[
\left( \frac{2}{\lambda} \sinh \frac{\lambda m}{2} \right)^2 = \left( \frac{2}{\lambda} \sinh \frac{\lambda E}{2} \right)^2 - e^{\lambda E} \vec{p}^2 ,
\]

which for low momenta takes the approximate form

\[
m^2 \simeq E^2 - \vec{p}^2 - \lambda E \vec{p}^2 .
\]

Actually, the precise form of the dispersion relation may depend on the choice of ordering prescription for wave exponentials (10) follows form (7), and this point deserves further scrutiny, but even setting aside this annoying ordering ambiguity, there appear to be severe obstructions for a satisfactory formulation of a quantum field theory in \( \kappa \)-Minkowski. The techniques that were rather straightforwardly applied for the construction of field theory in canonical noncommutative spacetime do not appear to be applicable in the \( \kappa \)-Minkowski case. It is not implausible that the “virulent” \( \kappa \)-Minkowski noncommutativity may require some departures from a standard field-theoretic setup.

### 4.3 Modified dispersion relation in Loop Quantum Gravity

Loop Quantum Gravity is one of the most ambitious approaches to the quantum-gravity problem, and its understanding is still in a relatively early stage. As presently understood, Loop Quantum Gravity predicts an inherently discretized spacetime, and this occurs in a rather compelling way: it is not that one introduces by hand an \textit{a priori} discrete background spacetime; it is rather a case in which a background-independent analysis ultimately leads, by a sort of self-consistency, to the emergence of discretization. There has been much discussion recently, prompted by the studies (38, 56, 57), of the possibility that this discretization might lead to broken Lorentz symmetry and a modified dispersion relation. Although there are cases in which a discretization is compatible with the presence of continuous classical symmetries (74, 75), it is of course natural, when adopting a discretized spacetime, to put Lorentz symmetry under careful scrutiny. Arguments presented in Refs. (56, 57) suggest that Lorentz symmetry might indeed be broken in Loop Quantum Gravity.

Moreover, very recently Smolin, Starodubtsev and I proposed (76) (also see the follow-up study in Ref. (77)) a mechanism such that Loop Quantum Gravity would be described at the most fundamental level as a theory that in the flat-spacetime limit admits deformed Lorentz symmetry, in the
sense of the “doubly-special relativity” scenario \[53\]. Our argument originates from the role that certain quantum symmetry groups (“\(q\)-deformed algebras”) have in the Loop-Quantum-Gravity description of spacetime with a cosmological constant, and observing that in the flat-spacetime limit (the limit of vanishing cosmological constant) these quantum groups might not contract to a classical Lie algebra, but rather contract to a quantum (Hopf) algebra.

All these studies point to the presence of a modified dispersion relation, although different arguments lead to different intuition for the form of the dispersion relation. A definite result might have to wait for the solution of the well-known “classical-limit problem” of Loop Quantum Gravity. We are presently unable to recover from this full quantum-gravity theory the limiting case in which the familiar quantum-field-theory description of particle-physics processes in a classical background spacetime applies. Some recent studies appear to suggest \[78\] that in the same contexts in which departures from Lorentz symmetry may be revealed one should adopt a density-matrix formalism, and then the whole picture would collapse to the familiar Lorentz-invariant quantum-field-theory description in contexts involving both relatively low energies and relatively low boosts with respect to the center-of-mass frame (e.g. the particle-physics collisions studied at several particle accelerators).

4.4 Some issues relevant for the proposal of test theories

In these lectures I am attempting to stress in particular the need for quantum-gravity phenomenology to establish that some Planck-scale pictures of spacetime are falsifiable and the need to rely on some reference test theories in the analysis of the progress of experimental limits as better data become available.

The results I briefly summarized in the previous three subsections provide a good indication of the fact that falsifiability is within reach. Both in the analysis of noncommutative spacetimes and in the analysis of Loop Quantum Gravity there are a few open issues which do not at present allow us to describe in detail a falsifiable prediction, but, in light of the progress achieved over the last few years, the nature of these open issues encourages us to think that we should soon achieve falsifiability.

In the meantime quantum-gravity phenomenology will have to push the limits on the type of effects that are emerging, and this effort should be guided by the objective of falsifiability. The analyses should avoid relying on assumptions which are likely to prove incorrect for the relevant formalisms. And when the open issues confront us with some alternative scenarios, the phenomenology work should attempt to “cover all possibilities”, \textit{i.e.} push
the limits in all directions that are still compatible with the present understand-
ing of the formalism (so that when the ambiguity is resolved there will be a class of data ready for comparison with theory).

In this situation it will be crucial for the development of the phenomeno-
 logical programme to adopt some suitably structured test theories, which should also be useful for bridging the gap between the experimental data and the, still incomplete, falsifiability analysis. These test theories should be our common language in assessing the progresses made in improving the sensitiv-
 ity of experiments, a language that must also be suitable for access from the side of those working at the development of the quantum-gravity/quantum-
 spacetime theories.

As we contemplate the challenge of developing such carefully-balanced test theories it is important to observe that the most robust part of the results I summarized in the previous three subsections is clearly the emergence of modified dispersion relations. Therefore if one could set up experiments testing directly the dispersion relation the resulting limits would have wide applicability. In principle one could investigate the form of the dispersion relation directly by making simultaneous measurements of energy and space-
momentum; however, it is easy to see that achieving Planck-scale sensitivity in such a direct test is well beyond our capabilities.

Useful test theories on which to base the relevant phenomenology must therefore combine the ingredient of the dispersion relation with other ingre-
dients. As I shall discuss in greater detail later in this section, there are three key issues for this test-theory development:

(i) in presence of the modified dispersion relation should we still assume the validity of of the relation $v = \frac{dE}{dp}$ between the speed of a particle and its dispersion relation? (here $\frac{dE}{dp}$ is the derivative of the function $E(p)$ which of course is implicitly introduced through the dispersion relation)

(ii) in presence of the modified dispersion relation should we still assume the validity of the standard law of energy-momentum conservation?

(iii) in presence of the modified dispersion relation which formalism should be adopted for the description of dynamics?

The fact that these are key issues is also a consequence of the type of data that we expect to have access to, as I shall discuss later in this section.

Unfortunately on these three key points the quantum-spacetime pictures which are providing motivation for the study of Planck-scale modifications of the dispersion relation, reviewed in the previous three subsections, are not providing much guidance yet.

For example, in Loop Quantum Gravity, while we do have evidence that the dispersion relation should be modified, we do not yet have a clear indi-
cation concerning whether the law of energy-momentum conservation should
also be modified and we also cannot yet robustly establish whether the relation \( v = dE/dp \) should be preserved. Moreover, perhaps most importantly, some recent studies \([78]\) invite us to consider the possibility that in the same contexts in which Loop-Quantum-Gravity departures from Lorentz symmetry may be revealed one should also adopt a density-matrix formalism, and then the whole picture might reduce to the familiar Lorentz-invariant quantum-field-theory description in contexts involving both relatively low energies and relatively low boosts with respect to the center-of-mass frame. We should therefore be prepared for surprises in the description of dynamics.

Similarly in the analysis of noncommutative spacetimes we are close to establishing in rather general terms that some modification of the dispersion relation is inevitable, but other aspects of the framework have not yet been clarified. While most of the literature for canonical noncommutative spacetimes assumes \([64, 65]\) that the law of energy-momentum conservation should not be modified, most of the literature for \(\kappa\)-Minkowski spacetime argues in favour of a modification (perhaps consistent with the corresponding doubly-special-relativity criteria \([53]\)) of the law of energy-momentum conservation. There is also still no consensus on the relation between speed and dispersion relation, and particularly in the \(\kappa\)-Minkowski literature some departures from the \( v = dE/dp \) relation are actively considered \([79, 80, 81, 82]\). And concerning the formalism to be used for the description of dynamics in a noncommutative spacetime, while everybody’s first guess is the field-theoretic formalism, the fact that attempts at a field theory formulation encounter so many difficulties (the IR/UV mixing for the canonical-noncommutative spacetime case and the even more pervasive shortcomings of the proposals for a field theory in \(\kappa\)-Minkowski) must invite one to consider possible alternative formulations of dynamics.

Clearly the situation on the theory side invites us to be prudent: if a given phenomenological picture relies on too many assumptions on Planck-scale physics it is likely that it might not reproduce any of the mentioned quantum-gravity and/or quantum-spacetime models (when these models are eventually fully understood they will give us their own mix of Planck-scale features, which is difficult to guess at the present time). On the other hand it is necessary for the robust development of a phenomenology to adopt well-defined test theories. Without reference to a well-balanced set of test theories it is impossible to compare the limits obtained in different experimental contexts, since each experimental context may require different “ingredients” of Planck-scale physics. And it is of course meaningless to compare limits obtained on the basis of different conjectures for the Planck-scale regime, especially since our very limited understanding of the Planck scale regime should encourage us to be prudent when formulating any assumption (virtually any assumption about the Planck-scale regime could turn out to be incorrect, once theories are better understood).
4.5 A test theory for pure kinematics

The majority (see, e.g., Refs. [39, 45, 46, 47, 48]) of studies concerning Planck-scale modifications of the dispersion relation adopt the phenomenological formula

\[ m^2 \simeq E^2 - \vec{p}^2 + \eta \vec{p}^2 \left( \frac{E^n}{E_p^n} \right) + O\left( \frac{E^{n+3}}{E_{QG}} \right), \tag{12} \]

with real \( \eta \) of order 1 and integer \( n \). This formula is compatible with some of the results obtained in the Loop-Quantum-Gravity approach and reflects the results obtained in \( \kappa \)-Minkowski and other noncommutative spacetimes (but, as mentioned, in the special case of canonical noncommutative spacetimes one encounters a different, infrared singular, dispersion relation).

As stressed above, on the basis of the status on the theory side, a prudent approach in combining the dispersion relation with other ingredients is to be favoured. Since basically all experimental situations will involve some aspects of kinematics that go beyond the dispersion relation (while there are some cases in which the dynamics, the interactions among particles, does not play a role), and taking into account the mentioned difficulties in establishing what is the correct formalism for the description of dynamics at the Planck scale, most authors prefer to prudently combine the dispersion relation with other “purely kinematical” aspects of Planck-scale physics.

Already in the first studies [38, 83] that proposed a phenomenology based on (12) it was assumed that even at the Planck scale the familiar description of “group velocity”, obtained from the dispersion relation according to \( v = \frac{dE}{dp} \), should hold.

\footnote{I am here using the expression “dynamics at the Planck scale” with some license. Of course, in our phenomenology we will not be sensitive directly to the dynamics at the Planck scale. However, as I discuss in greater detail in the next subsection, if the arguments that encourage the use of new descriptions of dynamics at the Planck scale are correct, then a sort of “order of limits problem” clearly arises. Our experiments will involve energies much lower than the Planck scale, and we know that in the infrared limit the familiar formalism with field-theoretic description of dynamics and Lorentz invariance will hold. So we would need to establish whether experiments that are sensitive to Planck-scale departures from Lorentz symmetry could also be sensitive to Planck-scale departures from the field-theoretic description of dynamics. Since we still know very little about this alternative descriptions of dynamics a prudent approach, avoiding any assumption about the description of dynamics is certainly preferable.}

\footnote{As mentioned, this assumption is not guaranteed to apply to the formalisms of interest, and indeed several authors have considered alternatives [47, 80, 81, 82]. While the studies advocating alternatives to \( v = \frac{dE}{dp} \) rely of a large variety of arguments (some more justifiable some less), in my own perception [84] a key issue here is whether quantum gravity leads to a modified Heisenberg uncertainty principle, \([x, p] = 1 + F(p)\). Assuming...}
In other works motivated by the analysis reported in Ref. [38], another key kinematical feature was introduced: starting with the studies reported in Refs. [45, 46, 47, 48] the dispersion relation (12) and the velocity relation \( v = dE/dp \) were combined with the assumption that the law of energy-momentum conservation should not be modified at the Planck scale, so that, for example, in a \( a + b \rightarrow c + d \) particle-physics process one would have

\[
E_a + E_b = E_c + E_d ,
\]

(13)

\[
\vec{p}_a + \vec{p}_b = \vec{p}_c + \vec{p}_d .
\]

(14)

Most authors work within this kinematic framework assuming “universality” of the dispersion relation (on which I shall return in the next subsection), but some have allowed [49, 85] for a particle-dependence and possibly an helicity-polarization dependence of the coefficients \( \eta, n \) of the dispersion relation.

The elements I described in this subsection compose a quantum-gravity phenomenology test theory that has already been widely considered in the literature, even though it was never previously characterized in detail. In the following I will refer to this test theory as the “AEMNS test theory”, and I will assume that experimental bounds on this test theory should be placed by using only the following assumptions:

(AEMNS.1) the dispersion relation is of the form

\[
m^2 \simeq E^2 - \vec{p}^2 + \eta_a \frac{p^2}{p_a} \left( \frac{E^{n_a}}{E^{n_a}} \right) + O\left( \frac{E^{n+3}}{E^{n+1}_{QG}} \right) ,
\]

(15)

where \( \eta_a \) and \( n_a \) can in general take different values for different particles and for different helicities/polarizations of the same particle (the index spans over particles and helicities/polarizations);

(AEMNS.2) the velocity of a particle can be obtained from the dispersion relation using \( v = dE/dp \);

(AEMNS.3) the law of energy-momentum conservation is not modified;

(AEMNS.4) nothing is assumed about dynamics (i.e. the analysis of this test theory will be limited to classes of experimental data that involve pure kinematics, without any role for dynamics).

a Hamiltonian description is still available, \( v = dx/dt \sim [x, H(p)] \), the relation \( v = dE/dp \) essentially follows from \( [x, p] = 1 \). But if \( [x, p] \neq 1 \) then \( v = dx/dt \sim [x, H(p)] \) would not lead to \( v = dE/dp \). And there is much discussion in the quantum-gravity community of the possibility of modifications of the Heisenberg uncertainty principle at the Planck scale.

\(^8\)I am using “AEMNS” on the basis of the initials of the names of the authors in Ref.[38], which first proposed a phenomenology based on the dispersion relation [12]. But as mentioned the full test theory, as presently used in most studies, only emerged gradually in follow-up work. In particular, there was no discussion of energy-momentum conservation in Ref. [38]. Unmodified energy-momentum conservation was introduced in Refs. [45, 46, 47, 48].
4.6 The minimal AEMNS test theory

On the basis of the results we presently have, at least within Loop Quantum Gravity and the study of certain noncommutative spacetimes, the formulation of the “AEMNS test theory” discussed in the previous subsection is general enough that we should expect it to be relevant for most quantum-spacetime pictures in which Lorentz symmetry is broken. Since, as mentioned, the analysis of these models is still in progress we might eventually be forced to consider further generalizations, including a possible modifications of the energy-momentum conservation law and/or of the law $v = dE/dp$.

Rather then prematurely considering this possible even wider parameter space, at present it is more reasonable to focus on a “minimal version” of the AEMNS test theory, in which the universality of the dispersion relation is assumed. It is in fact natural to expect that universality will be preserved in most of the relevant quantum-spacetime pictures. Moreover, as long as this minimal AEMNS test theory is not ruled out, clearly its more general nonuniversal version discussed in the previous subsection cannot be ruled. And it will be very useful to have a simple two-parameter space to use as reference in keeping track of the gradual improvement of the experimental sensitivities.

In order to be self-contained let me list here the characteristics of this “minimal AEMNS test theory”:

(minAEMNS.1) the dispersion relation is of the form

$$m^2 \simeq E^2 - \vec{p}^2 + \eta \vec{p}^2 \left( \frac{E^n}{E_p} \right) + O\left(\frac{E^{n+3}}{E_{QG}^{n+1}}\right),$$

where $\eta$ and $n$ are universal (same value for every particle and for both helicities/polarizations of a given particle);

(minAEMNS.2) the velocity of a particle can be obtained from the dispersion relation using $v = dE/dp$;

(minAEMNS.3) the law of energy-momentum conservation is not modified;

(minAEMNS.4) nothing is assumed about dynamics (i.e. the analysis of this test theory will be limited to classes of experimental data that involve pure kinematics, without any role for dynamics).

In a doubly-special-relativity framework with modified dispersion relation the law of energy-momentum conservation must be correspondingly modified in order to preserve the equivalence of inertial observers. Instead in a framework in which Lorentz symmetry is actually broken, with the associated loss of equivalence among inertial observers, modifications of the dispersion relation are in principle compatible with an unmodified law of energy-momentum conservation. Still, even in the broken-Lorentz-symmetry case, a modification of the law of energy-momentum conservation is possible.
4.7 A test theory based on low-energy effective field theory

The AEMNS test theory has the merit of relying only on a relatively small network of assumptions on kinematics, without assuming anything about the role of the Planck scale in dynamics. However, of course, this justifiable prudence turns into a severe limitation on the class of experimental contexts which can be used to constrain the parameters of the test theory. It is in fact rather rare that a phenomenological analysis can be completed without using (more or less explicitly) any aspects of the interactions among the particles involved in the relevant processes. The desire to be able to analyze a wider class of experimental contexts is therefore providing motivation for the development of test theories more ambitious than the AEMNS test theory, with at least some elements of dynamics. This is understandable but, in light of the situation on the theory side, work with one of these more ambitious test theories should proceed with the awareness that there is a high risk that it may turn out that none of the quantum-gravity approaches which are being pursued is reflected in the test theory.

One reasonable possibility to consider, when the urge to analyze data that involve some contamination from dynamics cannot be resisted, is the one of describing dynamics within the framework of low-energy effective field theory. In this subsection I want to discuss a test theory which is indeed based on low-energy effective field theory, and has emerged from the work recently reported in Ref. [56] (which is rooted in part in the earlier Ref. [86]).

Before a full characterization of this test theory I should first warn the reader that there might be some severe limitations for the applicability of low-energy effective field theory to the investigation of Planck-scale physics, especially when departures from Lorentz symmetry are present.

A significant portion of the quantum-gravity community is in general, justifiably, skeptical about the results obtained using low-energy effective field theory in analyses relevant for the quantum-gravity problem. After all the first natural prediction of low-energy effective field theory in the gravitational realm is a value of the energy density which is some 120 orders of magnitude greater than allowed by observations1. Somewhat related to this “cosmological constant problem” is the fact that a description of possible Planck-scale departures from Lorentz symmetry within effective field theory can only be developed with a rather strongly pragmatic attitude; in fact, while one can introduce Planck-scale suppressed effects at tree level, one expects that loop corrections would typically lead to inadmissibly large departures from ordinary Lorentz symmetry. Indeed some studies, notably

1And the outlook of low-energy effective field theory in the gravitational realm does not improve much through the observation that exact supersymmetry could protect from the emergence of any energy density. In fact, Nature clearly does not have supersymmetry at least up to the TeV scale, and this would still lead to a natural prediction of the cosmological constant which is some 60 orders of magnitude too high.
Refs. [87, 88], have shown mechanisms such that, within an effective-field-theory formulation, loop effects would lead to inadmissibly large departures from ordinary Lorentz symmetry, which could be avoided only by introducing a large level of fine tuning.

It is rather amusing that alongside with numerous researchers who are skeptical about any results obtained using low-energy effective field theory in analyses relevant for the quantum-gravity problem, there are also quite a few researchers interested in the quantum-gravity problem who are completely serene in assuming that all quantum-gravity effects should be describable in terms of effective field theory in low-energy situations. The (quasi-)rationale behind this assumption is that field theory works well at low energies without gravity, and quantum gravity of course must reproduce field theory in an appropriate limit, so one might expect that at low energies the quantum-gravity effects could be described in the language of field theory as correction terms to be added to standard lagrangians.

I feel that, while of course an effective-field-theory description may well turn out to be correct in the end, the a priori assumption that a description in terms of effective low-energy field-theory should work is rather naive. If the arguments that encourage the use of new descriptions of dynamics at the Planck scale are correct, then a sort of “order of limits problem” clearly arises. Our experiments will involve energies much lower than the Planck scale, and we know that in some limit (a limit that characterizes our most familiar observations) the field-theoretic description and Lorentz invariance will hold. So we would need to establish whether experiments that are sensitive to Planck-scale departures from Lorentz symmetry could also be sensitive to Planck-scale departures from the field-theoretic description of dynamics. As an example, let me mention the possibility (not unlikely in a context which is questioning the fate of Lorentz symmetry) that quantum gravity would admit a field-theory-type description only in reference frames in which the process of interest is essentially occurring in its center of mass (no “Planck-large boost” with respect to center-of-mass frame). The field theoretic description could emerge in a sort of “low-boost limit”, rather than the expected low-energy limit. The regime of low boosts with respect to the center-of-mass frame is often indistinguishable with respect to the low-energy limit. For example, from a Planck-scale perspective, our laboratory experiments (even the ones conducted at, e.g. CERN, DESY, SLAC...) are both low-boost (with respect to the center of mass frame) and low-energy. However, the “UHE cosmic-ray paradox”, for which a quantum-gravity origin has been conjectured (see later), occurs in a situation where all the energies of the particles are still tiny with respect to the Planck energy scale, but the boost with respect to the center-of-mass frame (as measured by the ratio $E/m_{\text{proton}}$ between the proton energy and the proton mass) could be considered to be “large” from a Planck-scale perspective ($E/m_{\text{proton}} \gg E_p/E$).

These concerns are strengthened by looking at the literature available on the quantum pictures of spacetime that provide motivation for the study of modified dispersion relations, which usually involve either noncommutative
geometry or Loop Quantum Gravity, where, as mentioned, the outlook of a low-energy effective-field-theory description is not encouraging. The construction of field theories in noncommutative spacetimes requires the introduction of several new technical tools, which in turn lead to the emergence of several new physical features, even at low energies. I guess that these difficulties arise from the fact that a spacetime characterized by an uncertainty relation of the type $\delta x \delta y \geq \theta(x, y)$ never really behaves has a classical spacetime, not even at very low energies. In fact, some low-energy processes will involve soft momentum exchange in the $x$ direction (large $\delta x$) which however is connected with the exchange of a hard momentum in the $y$ direction ($\delta y \geq \theta/\delta x$), and this feature cannot be faithfully captured by our ordinary field-theory formalisms. In the case of canonical noncommutative spacetimes one does obtain a plausible-looking field theory\[65\], but the results actually show that it is not possible to rely on an ordinary effective low-energy quantum-field-theory description. In fact, the “IR/UV mixing” \[64, 65, 66\] is such that the high-energy sector of the theory does not decouple from the low-energy sector, and this in turn affects very severely\[66\] the outlook of analyses based on an ordinary effective low-energy quantum-field-theory description. For other (non-canonical) noncommutative spacetimes we are still struggling in the search of a satisfactory formulation of a quantum field theory \[71, 72\], and it is at this point legitimate to suspect that such a formulation of dynamics in those spacetimes does not exist.

Incidentally let me observe that the issues encountered in dealing with the IR/UV mixing may be related to my concerns about the large-boost limit of quantum gravity. In a theory with IR/UV mixing nothing peculiar might be expected for, say, a collision between two photons both of $MeV$ energy, but the boosted version of this collision, where one photon has, say, energy of $100TeV$ and the other photon has energy of $10^{-2}eV$, could be subject to the IR/UV mixing effects, and be essentially untreatable from a low-energy effective-field-theory perspective.

And noncommutative spacetimes are not the only cases where an ordinary field-theory description may be inadequate. As mentioned, the assumption of availability of an ordinary effective low-energy quantum-field-theory description finds also no support in Loop Quantum Gravity. Indeed, so far, in Loop Quantum Gravity all attempts to find a suitable limit of the theory which can be described in terms of a quantum-field-theory in background spacetime have failed. And on the basis of the recent results of Ref. \[78\] it appears plausible that in several contexts in which one would naively expect a low-energy field theory description Loop Quantum Gravity might instead require a density-matrix description.

Of course, in phenomenology this type of concerns can be set aside, since one is primarily looking for confrontation with experimental data, rather than theoretical prejudice. It is clearly legitimate to set up a test theory exploring the possibility of Planck-scale departures from Lorentz symmetry within the formalism of low-energy effective field theory. But one must then keep in mind that the implications for most quantum-gravity research lines
of the experimental bounds obtained in this way might be very limited. This will indeed be the case if we discover that, as some mentioned preliminary results suggest, the limit in which the full quantum-gravity theory reproduces a description in terms of effective field theory in classical spacetime is also the limit in which the departures from Lorentz symmetry must be neglected.

Having provided this long warning, let me now proceed to a characterization of the test theory which I see emerging from the works reported in Refs. [86, 56]. These studies explore the possibility of a linear-in-$L_p$ modification of the dispersion relation

$$m^2 \simeq E^2 - \vec{p}^2 + \eta \gamma \vec{p}^2 L_p E,$$

(17)

i.e. the case $n = 1$ of Eq. (12). The key assumption in Refs. [86, 56] is that such modifications of the dispersion relation should be introduced consistently with an effective low-energy field-theory description of dynamics. The implications of this assumption were explored in particular for fermions and photons. It became quickly clear that in such a setup universality cannot be assumed, since one must at least accommodate a polarization dependence for photons: in the field-theoretic setup it turns out that when right-circular polarized photons satisfy the dispersion relation $E^2 \simeq p^2 + \eta \gamma p^2$ then necessarily left-circular polarized photons satisfy the “opposite sign” dispersion relation $E^2 \simeq p^2 - \eta \gamma p^3$. For spin-1/2 particles the analysis reported in Ref. [86] does not necessarily suggest a similar helicity dependence, but of course in a context in which photons experience such a complete correlation of the sign of the effect with polarization it would be awkward to assume that instead for electrons the effect is completely helicity independent. One therefore introduces two independent parameters $\eta_+$ and $\eta_-$ to characterize the modification of the dispersion relation for electrons.

In the following I will refer to this test theory as the “GPMP test theory” (from the initials of the authors of Refs. [86, 56]), and I will assume that experimental bounds on this test theory should be placed by using only the following assumptions:

(GPMP.1) for right-circular polarized photons are governed by the dispersion relation

$$m^2 \simeq E^2 - \vec{p}^2 + \eta_+ \vec{p}^2 \left( \frac{E}{E_p} \right),$$

(18)

while left-circular polarized photons are governed by the dispersion relation

$$m^2 \simeq E^2 - \vec{p}^2 - \eta_- \vec{p}^2 \left( \frac{E}{E_p} \right);$$

(19)

(GPMP.2) for fermions the dispersion relation takes the form

$$m^2 \simeq E^2 - \vec{p}^2 + \eta_0 \vec{p}^2 \left( \frac{E}{E_p} \right),$$

(20)
in the positive-helicity case, while for negative-helicity fermions
\[ m^2 \simeq E^2 - \vec{p} \cdot \vec{p} + \eta^a L \vec{p} \cdot \vec{p} \left( \frac{E}{E_p} \right); \] (21)
the index \( a \) here reflecting a possible particle dependence;

(GPMP.3) dynamics is described in terms of effective low-energy field theory.

4.8 The minimal GPMP test theory

As in the case of the AEMNS test theory, while a large parameter space should be considered in order to achieve full generality, it appears wise to first focus the phenomenology on a reduced version of the test theory, reflecting some natural physical assumptions. As in the case of the AEMNS test theory, a reduced two-parameter space would be ideal for the first-level description of the gradual improvement of the experimental sensitivities. As usual, once the reduced version of the test theory is falsified one can contemplate its possible generalization (if the developments on the pure-theory side still justify such an effort from the perspective of falsification of the theories).

In introducing a reduced GPMP test theory I believe that a key point of naturalness comes from the observation that the effective-field-theory setup imposes for photons a modification of the dispersion relation which has the same magnitude for both polarizations but opposite sign: it is then natural to give priority to the hypothesis that for fermions a similar mechanism would apply, \( i.e. \) the modification of the dispersion relation should have the same magnitude for both signs of the helicity, but have a correlation between the sign of the helicity and the sign of the dispersion-relation modification. This would correspond to the natural-looking assumption that the Planck-scale effects are such that in a beam composed of randomly selected particles the average speed in the beam is still governed by ordinary special relativity (the Planck scale effects average out summing over polarization/helicity).

A further “natural” reduction of the parameter space is achieved by assuming that all fermions are affected by the same modification of the dispersion relation.

The reduced GPMP test theory that emerges from this requirements is perhaps the most natural among the possible two-parameter reduction of the GPMP test theory. In the following I refer to this reduced GPMP test theory as the “minimal GPMP test theory”\(^3\), characterized by the following

\(^3\) Whereas for the AEMNS test theory there is clearly only one obvious way to set up the reduction to a two-dimensional parameter space, within the GPMP test theory, with its automatic polarization dependence of the effects for photons, one could probably envision more than one way to set up the reduction to a two-dimensional parameter space. In a certain sense the two-dimensional parameter space on which I propose to focus for the AEMNS test theory is the minimal AEMNS test theory, whereas here I am proposing a minimal GPMP test theory.
ingredients:

(minGPMP.1) right-circular polarized photons are governed by the dispersion relation

\[ m^2 \simeq E^2 - \vec{p}^2 + \eta \gamma \vec{p}^2 \left( \frac{E}{E_p} \right), \quad (22) \]

while left-circular polarized photons are governed by the dispersion relation

\[ m^2 \simeq E^2 - \vec{p}^2 - \eta \gamma \vec{p}^2 \left( \frac{E}{E_p} \right); \quad (23) \]

(minGPMP.2) for fermions the dispersion relation takes the form

\[ m^2 \simeq E^2 - \vec{p}^2 + \eta_f \vec{p}^2 \left( \frac{E}{E_p} \right), \quad (24) \]

in the positive-helicity case, while for negative-helicity fermions

\[ m^2 \simeq E^2 - \vec{p}^2 - \eta_f \vec{p}^2 \left( \frac{E}{E_p} \right), \quad (25) \]

with the same value of \( \eta_f \) for all fermions;

(minGPMP.3) dynamics is described in terms of effective low-energy field theory.

4.9 Derivation of limits from analysis of gamma-ray bursts

Both in the AEMNS test theory and in the GPMP test theory one expects a wavelength dependence of the speed of photons, by combining the modified dispersion relation and the relation \( v = dE/dp \). At “intermediate energies” \( m < E \ll E_p \) this velocity law will take the form

\[ v \simeq 1 - \frac{m^2}{2E^2} + \eta \frac{n + 1}{2} \frac{E^n}{E_p^n}. \quad (26) \]

Whereas in ordinary special relativity two photons \( (m = 0) \) emitted simultaneously would always reach simultaneously a far-away detector, according to (26) two simultaneously-emitted photons should reach the detector at different times if they carry different energy. Moreover, in the case of the GPMP test theory even photons with the same energy would arrive at different times.
if they carry different polarization. In fact, while the minimal AEMNS test theory assumes universality, and therefore a formula of this type would apply to photons of any polarization, in the GPMP test theory, as mentioned, the sign of the effect is correlated with polarization. As a result, while the AEMNS test theory is best tested by comparing the arrival times of particles of different energies, the GPMP test theory is best tested by considering the highest-energy photons available in the data and looking for a sizeable spread in times of arrivals (which one would then attribute to the different speeds of the two polarizations).

This time-of-arrival-difference effect can be significant \[38, 39\] in the analysis of short-duration gamma-ray bursts that reach us from cosmological distances. For a gamma-ray burst it is not uncommon that the time travelled before reaching our Earth detectors be of order \( T \sim 10^{17} \text{s} \). Microbursts within a burst can have very short duration, as short as \( 10^{-3} \text{s} \) (or even \( 10^{-4} \text{s} \)), and this means that the photons that compose such a microburst are all emitted at the same time, up to an uncertainty of \( 10^{-3} \text{s} \). Some of the photons in these bursts have energies that extend at least up to the GeV range. For two photons with energy difference of order \( \Delta E \sim 1 \text{GeV} \) a \( \eta \Delta E / E_p \) speed difference over a time of travel of \( 10^{17} \text{s} \) would lead to a difference in times of arrival of order

\[
\Delta t \sim \eta T \Delta E / E_p \sim 10^{-2} \text{s},
\]

which is significant (the time-of-arrival differences would be larger than the time-of-emission differences within a single microburst).

For the AEMNS test theory the Planck-scale-induced time-of-arrival difference could be revealed \[38, 39\] upon comparison of the “average arrival time” of the gamma-ray-burst signal (or better a microburst within the burst) in different energy channels. The GPMP test theory would be most effectively tested by looking for a dependence of the time-spread of the bursts that grows with energy (at low energies the effect is anyway small, so the polarization dependence is ineffective, whereas at high energies the effect may be nonnegligible and an overall time-spread of the burst could result from the dependence of speed on polarization).

The sensitivities achievable \[90\] with the next generation of gamma-ray telescopes, such as GLAST \[90\], could allow to test very significantly \[20\] in the case \( n = 1 \), by possibly pushing the limit on \( \eta \) far below 1 (whereas the effects found in the case \( n = 2 \), \( |\eta| \sim 1 \) are too small for GLAST). Whether or not these levels of sensitivity to the Planck-scale effects are actually achieved may depend on progress in understanding other aspects of gamma-ray-burst physics. In fact, the Planck-scale-effect analysis would be severely affected if there were poorly understood at-the-source correlations between energy of the photons and time of emission. In the recent Ref. \[91\] it was emphasized that it appears that one can infer such an energy/time-of-emission correlation from available gamma-ray-burst data. The studies
of Planck-scale effects will be therefore confronted with a severe challenge of “background/noise removal”. At present it is difficult to guess whether this problem can be handled successfully. We do have a good card to play in this analysis: the Planck-scale picture predicts that the times of arrival should depend on energy in a way that grows in exactly linear way with the distance of the source. One may therefore hope that, once a large enough sample of gamma-ray bursts (with known source distances) becomes available, one might be able disentangle the Planck-scale propagation effect from the at-the-source background.

An even higher sensitivity to possible Planck-scale modifications of the velocity law could be achieved by exploiting the fact that, according to current models \[92\], gamma-ray bursters should also emit a substantial amount of high-energy neutrinos. Some neutrino observatories should soon observe neutrinos with energies between \(10^{14}\) and \(10^{19}\) eV, and one could, for example, compare the times of arrival of these neutrinos emitted by gamma-ray bursters to the corresponding times of arrival of low-energy photons. One could use this strategy to test rather stringently\(^k\) the case of \((26)\) with \(n = 1\), an even perhaps gain some access to the investigation of the case \(n = 2\).

In order to achieve these sensitivities with neutrino studies once again some technical and conceptual challenges should be overcome. Also this type of analysis requires an understanding of gamma-ray bursters good enough to establish whether there are typical at-the-source time delays. The analysis would loose much of its potential if one cannot exclude some systematic tendency of gamma-ray bursters to emit high-energy neutrinos with, say, a certain delay with respect to microbursts of photons. But also in this case one could hope to combine several observations from gamma-ray bursters at different distances in order to disentangle the possible at-the-source effect.

### 4.10 Derivation of limits from analysis of UHE cosmic rays

With a given dispersion relation and a given rule for energy-momentum conservation one has a complete “kinematic scheme” for the analysis of the kinematical requirements for particle production in collisions or decay processes. Both the AEMNS test theory and the GPMP test theory involve modified dispersion relations and unmodified laws of energy-momentum conservation (the fact that the law of energy-momentum conservation is not modified is explicitly among the ingredients of the AEMNS test theory, while in the

\(^{k}\)Note however that in an analysis mixing the properties of different particles the sensitivity that can be achieved will depend strongly on whether universality of the modification of the dispersion relation is assumed. For example, for the GPMP test theory a comparison of times of arrival of neutrinos and photons could only introduce a bound on some combination of the dispersion-relation-modification parameters for the photon and for the neutrino sectors.
GPMP test theory it follows from the adoption of low-energy effective field theory.

In these lectures I am not discussing in detail the case of modified dispersion relations introduced within a “doubly-special relativity” scenario \([53, 70]\). For clarity of the presentation, I thought it would be best to limit to two the number of test theories I consider. Test theories for doubly-special relativity scenarios with modified dispersion relations are under consideration (see, e.g., Ref. [93]), but I will not make room for them here. It is appropriate however to stress here that the assumption of modified dispersion relations and unmodified laws of energy-momentum conservation is inconsistent with the doubly-special relativity principles, since it inevitably gives rise to a preferred class of inertial observers. A doubly-special relativity scenario with modified dispersion relations must necessarily have a modified law of energy-momentum conservation.

Going back to the AEMNS and GPMP test theories which I am considering, in this subsection I want to stress that combining a modified dispersion relation with unmodified laws of energy-momentum conservation one naturally finds a modification of the threshold requirements for certain particle-producing processes. Let us for example consider, from the AEMNS perspective, the dispersion relation \((12)\), with \(n = 1\), in the analysis of a collision between a soft photon of energy \(\epsilon\) and a high-energy photon of energy \(E\) that creates an electron-positron pair: \(\gamma\gamma \rightarrow e^+e^-\). For given soft-photon energy \(\epsilon\), the process is allowed only if \(E\) is greater than a certain threshold energy \(E_{th}\) which depends on \(\epsilon\) and \(m_e^2\). For \(n = 1\), combining \((12)\) with unmodified energy-momentum conservation, this threshold energy (assuming \(\epsilon \ll m_e \ll E_{th} \ll E_p\)) is estimated as

\[
E_{th}\epsilon + \frac{E_{th}^3}{8E_p} = m_e^2.
\]  

The special-relativistic result \(E_{th} = m_e^2/\epsilon\) corresponds of course to the \(\eta \rightarrow 0\) limit of \((28)\). For \(|\eta| \sim 1\) the Planck-scale correction can be safely neglected as long as \(\epsilon > (m_e^4/E_p)^{1/3}\). But eventually, for sufficiently small values of \(\epsilon\) and correspondingly large values of \(E_{th}\), the Planck-scale correction cannot be ignored \([46, 47, 48, 49, 50]\).

And the process \(\gamma\gamma \rightarrow e^+e^-\) is not the only case in which this type of Planck-scale modification can be important. There has been strong interest \([45, 46, 47, 48, 49, 50, 51, 94]\) in “photopion production”, \(p\gamma \rightarrow p\pi\), where again the combination of \((12)\) with unmodified energy-momentum conservation leads to a modification of the minimum proton energy required by the process (for fixed photon energy). In the case in which the photon energy is the one typical of CMBR photons one finds that the threshold proton energy can be significantly shifted upward (for negative \(\eta\)), and this in turn should affect at an observably large level the expected “GZK cutoff” for the observed cosmic-ray spectrum. Observations reported by the AGASA \([95]\) cosmic-ray
observatory provide some encouragement for the idea of such an upward shift of the GZK cutoff, but the issue must be further explored. Forthcoming cosmic-ray observatories, such as Auger, should be able to fully investigate this possibility.

In this context the comparison of the AEMNS test theory and the GPMP test theory is rather straightforward. We are in fact considering a purely kinematical effect: the shift of a threshold requirement. For the minimal AEMNS test theory there is a clear prediction that for negative $\eta$ there should be an upward shift of the GZK threshold. For the GPMP test theory one would predict an increase of the GZK threshold if any one (or both) of the two helicities of the proton has dispersion relation of “negative $\eta$” type. If both helicities have dispersion relation of negative-$\eta$ type then the effect looks rather similar to the corresponding effect in the AEMNS test theory. For the situation which I proposed as the “minimal GPMP test theory”, where for one of the helicities the dispersion relation is of negative-$\eta$ type and for the other helicity the dispersion relation is of positive-$\eta$ type, one would expect roughly one half of the UHE protons to evade the GZK cutoff, so the cutoff would still be violated but in a softer way than in the case of the AEMNS test theory with negative $\eta$.

It appears likely that, if the Auger data should actually show evidence of the expected GZK cutoff, we would then be in a position to rule out the case of negative $\eta$ for the minimal AEMNS test theory, and to rule out both the positive-$\eta_f$ and negative-$\eta_f$ case for the minimal GPMP test theory. In fact, in the minimal AEMNS test theory violations of the GZK cutoff are predicted for negative $\eta$ (while they are not present in the positive-$\eta$ case), while in the minimal GPMP test theory violations of the GZK cutoff (although less numerous than expected in the minimal AEMNS test theory with negative $\eta$) are always expected, independently of the sign of $\eta_f$ (depending on the sign of $\eta_f$ the protons that violate the GZK cutoff would have a corresponding helicity).

I should stress that these studies of the cosmic-ray GZK threshold provide an example in which the fact that we do not really identify some of the particles in the relevant particle-physics processes, an analysis which could in principle be involving pure kinematics, ends up being exposed to the risk of contamination from some aspects of dynamics. If the only background radiation available for photopion production was the CMBR, then the prediction of an upward shift of the GZK cosmic-ray cutoff within the AEMNS test theory, for negative $\eta$, would be completely robust. But background radiation has many components and one could contemplate the possibility to combine AEMNS kinematics with an unspecified description of dynamics such that interactions of cosmic rays with other components of the background radiation would lead to a net result that does not change the numerical value of the GZK threshold. While this possibility must be contemplated, I also want to stress that, at least for $n = 1$ and negative $\eta$ of order 1, this “conspiracy scenario” is so unbelievable that it should be dismissed. In fact, for
\( n = 1 \) and negative \( \eta \) of order 1 the AEMNS kinematics allows the interaction of cosmic rays only with photons of energy higher than the TeV scale (see Ref. [48]), and the density of such high-energy background photons is so low that, even in a prudent phenomenology, this “conspiracy scenario” can indeed be dismissed.

For the GPMP test theory there is of course no issue of possible conspiracies, since the field-theoretic setup allows to evaluate cross sections.

### 4.11 Derivation of limits from analysis of photon stability

As in the case of the GZK cutoff for UHE cosmic rays there are several examples in which a given process is allowed in presence of exact Lorentz symmetry but can be kinematically forbidden in presence of certain departures from Lorentz symmetry. The opposite is also possible: some processes that are kinematically forbidden in presence of exact Lorentz symmetry become kinematically allowed in presence of certain departures from Lorentz symmetry. The fact that a process is kinematically allowed of course does not guarantee that it occurs at an observable rate: it depends on the laws of dynamics and the amplitudes they predict.

Certain observations in astrophysics, which allow us to establish that photons of energies up to \( \sim 10^{14} \) eV are not unstable, can be particularly useful in setting limits on some schemes for departures from Lorentz symmetry. Let us for example analyze the process \( \gamma \rightarrow e^+ e^- \) from the AEMNS perspective, using the dispersion relation [12], with \( n = 1 \), and unmodified energy-momentum conservation. One easily finds a relation between the energy \( E_\gamma \) of the incoming photon, the opening angle \( \theta \) between the outgoing electron-positron pair, and the energy \( E_+ \) of the outgoing positron (of course the energy of the outgoing electron is simply given by \( E_\gamma - E_+ \)). For the region of phase space with \( m_e \ll E_\gamma \ll E_p \), this relation takes the form

\[
\cos(\theta) \simeq \frac{E_+(E_\gamma - E_+) + m_e^2 - \eta E_+ E_+(E_\gamma - E_+)/E_p}{E_+(E_\gamma - E_+)},
\]

(29)

where \( m_e \) is the electron mass.

The fact that for \( \eta = 0 \) Eq. (29) would require \( \cos(\theta) > 1 \) reflects the fact that, if Lorentz symmetry is preserved, the process \( \gamma \rightarrow e^+ e^- \) is kinematically forbidden. For \( \eta < 0 \) the process is still forbidden, but for positive \( \eta \) high-energy photons can decay into an electron-positron pair. In fact, for \( E_\gamma \gg (m_e^2 E_p/|\eta|)^{1/3} \) one finds that there is a region of phase space where \( \cos(\theta) < 1 \), i.e. there is a physical phase space available for the decay.

The energy scale \( (m_e^2 E_p)^{1/3} \sim 10^{13} \) eV is not too high for testing, since, as mentioned, in astrophysics we see photons of energies up to \( \sim 10^{14} \) eV that are not unstable (they clearly travel safely some large astrophysical distances).
Within AEMNS kinematics, for $n = 1$ and positive $\eta$ of order 1, it would have been natural to expect that such photons with $\sim 10^{14}\text{eV}$ energy would not be stable. Once again, before claiming that $n = 1$ and positive $\eta$ of order 1 is ruled out, one should be concerned about possible conspiracies. The fact that the decay of $10^{14}\text{eV}$ photons is allowed by AEMNS kinematics (for $n = 1$ and positive $\eta$ of order 1) of course does not guarantee that these photons should rapidly decay. It depends on the relevant probability amplitude, whose evaluation goes beyond the reach of kinematics. I am unable to provide an intuition for how big of a conspiracy would be needed to render $10^{14}\text{eV}$ photons stable compatibly with AEMNS kinematics with $n = 1$ and $\eta = 1$. My tentative conclusion is that $n = 1$ with positive $\eta$ of order 1 is ruled out “up to conspiracies”, but unlike the case of the GZK-threshold analysis I am unprepared to argue that the needed conspiracy is truly unbelievable.

For the GPMP test theory the photon stability analysis is weakened because of other reasons. There one does have the support of the effective-field-theory description of dynamics, and within that framework one can exclude huge suppression by Planck scale effects of the interaction vertex needed for $\gamma \rightarrow e^+e^-$ around $\sim 10^{13}\text{eV}$, $\sim 10^{14}\text{eV}$. So the limit-setting effort is not weakened by the absence of an interaction vertex. However, as mentioned, consistency with the effective-field-theory setup requires that the two polarizations of the photon acquire opposite-sign modifications of the dispersion relation. We observe in astrophysics some photons of energies up to $\sim 10^{14}\text{eV}$ that are stable over large distances, but as far as we know those photons could be all, say, right-circular polarized (or all left-circular polarized). I postpone a detailed analysis to future work, but let me note here that there is a region of minimal-GPMP parameter space where both polarizations of a $\sim 10^{14}\text{eV}$ photon are unstable (a subset of the region with $|\eta_f| > |\eta_\gamma|$). That region of the minimal-GPMP parameter parameter space is of course excluded by the photon-stability data.

4.12 Derivation of limits from analysis of synchrotron radiation

A recent series of papers[98, 99, 100, 101, 102, 85, 103] has focused on the possibility to set limits on Planck-scale modified dispersion relations focusing on their implications for synchrotron radiation. By comparing the content of the first estimates\footnote{Ref. [98] is at this point obsolete, since the relevant manuscript has been revised for the published version[104] and the recent Ref. [103] provides an even more detailed analysis. It is nevertheless useful to consider this series of manuscripts [98, 100, 101, 102, 85, 103] as an illustration of how much the outlook of a phenomenological analysis may change in going from the level of simplistic order-of-magnitude estimates to the level of careful comparison with meaningful test theories.} produced in this research line [98] with the understanding that emerged from follow-up studies [99, 100, 101, 102, 85, 103] one can
gain valuable insight on the risks involved in analyses based on simplistic order-of-magnitude estimates, rather than careful comparison with meaningful test theories. In Ref. [98], the starting point is the observation that in the conventional (Lorentz-invariant) description of synchrotron radiation one can estimate the characteristic energy $E_c$ of the radiation through a heuristic analysis [104] leading to the formula

$$E_c \approx \frac{1}{R \cdot \delta \cdot |v_\gamma - v_e|},$$

(30)

where $v_\gamma$ is the speed of the photon, $v_e$ is the speed of the electron, $\delta$ is an angle obtained from the opening angle between the direction of the electron and the direction of the emitted photon, and $R$ is the radius of curvature of the trajectory of the electron.

Assuming that the only Planck-scale modification in this formula should come from the velocity law (described using $v = dE/dp$ in terms of the modified dispersion relation), one finds that in some instances the characteristic energy of synchrotron radiation may be significantly modified by the presence of Planck-scale departures from Lorentz symmetry. As an opportunity to test such a modification of the value of the synchrotron-radiation characteristic energy one can hope to use some relevant data [98, 100] on photons detected from the Crab nebula. This must be done with caution since the observational information on synchrotron radiation being emitted by the Crab nebula is rather indirect: some of the photons we observe from the Crab nebula are attributed to synchrotron processes on the basis of a promising conjecture, and the value of the relevant magnetic fields is also conjectured (not directly measured).

Assuming that indeed the observational situation has been properly interpreted, and relying on the mentioned assumption that the only modification to be taken into account is the one of the velocity law, one could basically rule out [98] the case $n = 1$ with negative $\eta$ for a modified dispersion relation of the type (12).

This observation led at first to some excitement, but more recent papers are starting to adopt a more prudent viewpoint. The lack of comparison with a meaningful test theory represents a severe limitation of the original analysis. In particular, synchrotron radiation is due to the acceleration of the relevant electrons and therefore implicit in the derivation of the formula (30) is a subtle role for dynamics [99]. From a field-theory perspective the process of synchrotron-radiation emission can be described in terms of Compton scattering of the electrons with the virtual photons of the magnetic field. One would therefore be looking deep into the dynamical features of the theory.

The minimal AEMNS test theory does assume a modified dispersion relation of the type (12) universally applied to all particles, but it is a pure-kinematics framework and, since the analysis crucially involves some aspects of dynamics, it cannot be tested using a Crab-nebula synchrotron-radiation analysis.
The GPMP test theory relies on a description of dynamics within the framework of effective low-energy theory, but, as mentioned, this in turn ends up implying that it is not possible to assume that a dispersion relation of the type (12) universally applies to all particles. Actually the two polarizations of photons must, within this framework, satisfy different (opposite-sign Planck-scale corrections) dispersion relations. And for the description of electrons one naturally encounters at least two more free parameters. The only constraint that one could conceivably obtain for the GPMP test theory from the Crab-nebula synchrotron-radiation analysis would simply exclude that both the electron-dispersion-relation parameters be negative (i.e. exclude that both helicities of the electron would be characterized by a dispersion relation of the type (12) with negative $\eta$ and $n = 1$).

In particular, the case which I characterized as the “minimal GPMP test theory”, where the two helicities of the electrons carry opposite-sign modifications of the dispersion relation, would automatically evade this type of constraint from the Crab-nebula synchrotron-radiation analysis (since the two helicities are affected by opposite-sign modifications of the dispersion relation, at least one of them must be a positive-sign-type modification).

5 Summary and outlook

Quantum-Gravity Phenomenology has already reached its first goal: a sizable community now works on the quantum-gravity problem with the awareness that there is a chance to test (at least some) Planck-scale effects. In reaching this first goal it was sufficient (and even, in a certain sense, necessary) to proceed with simple intuitive arguments, but the further development of quantum-gravity phenomenology requires us to adopt the standards of other branches of phenomenology, such as particle-physics phenomenology. In particular, the progress of experimental limits must be charted in terms of commonly-adopted, and carefully crafted, test theories of the new Planck-scale effects.

The fact that some Planck-scale pictures of spacetime physics are falsifiable is more and more robustly established, but in many cases we only see a path toward falsifiability rather having achieved already the results needed for a “critical test of a theory” (a test that could be used, in case of contrary experimental results, to discard the relevant Planck-scale picture of spacetime physics). This point of the falsifiability of some relevant theories is crucial for establishing quantum-gravity research as a truly scientific endeavor. The proposal of test theories must of course reflect the status of our analysis of the

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**Footnote:** Even the possibility to derive any sort of constraint on the electron-dispersion-relation parameters is not guaranteed. In fact, as observed in the latest version of Ref. [103], one might be unable to exclude the possibility that the Crab-nebula synchrotron radiation be due to positron (rather than electron) acceleration.
falsifiability of quantum-spacetime/quantum-gravity theories. In an appropriate sense the test theories must bridge the gap between quantum-gravity theories and experiments. They must be such that the experimental limits on the parameters of the test theories will naturally translate into direct limits on some relevant quantum-gravity theories, as soon as some falsifiable features of the quantum-gravity theory are fully established.

It is of course meaningless to compare limits obtained within different test theories. And there is no scientific content in an experimental limit claimed on a vaguely defined test theory. For example, in the recent literature there has been a proliferation of papers claiming to improve limits on Planck-scale modifications of the dispersion relation, but the different studies were simply considering the same type of dispersion relation within significantly different test theories. These results, which were presented as a gradual improvement in the experimental limits on Planck-scale modifications of the dispersion relation, were actually only a series of papers proposing more and more (some better, some worse) different examples of test theories in which a Planck-scale modification of the dispersion relation can be accommodated. Each paper was proposing a different test theory and deriving limits on that specific test theory.

In order to illustrate these issues in the context of a specific example of quantum-gravity-phenomenology work, in the second part of these lectures I focused on the example of the phenomenology of Planck-scale modifications of the dispersion relation. I considered two examples of test theories, the AEMNS test theory and the GPMP test theory. These two test theories, although usually not explicitly fully characterized in the relevant papers, are among the most studied in the case of Planck-scale modifications of the dispersion relation.

I also stressed that a phenomenology should build its strength gradually. Within a given set of hypothesis one first sets up a reduced parameter space, and only once that reduced parameter space is ruled out by data one considers the possibility of wider parameter spaces. In the context here of interest the minimal AEMNS test theory, described in Subsection 4.6, and the minimal GPMP test theory, described in Subsection 4.8, appear to provide valuable starting points.

In particular, these two test theories can be representative of two types of attitudes that are emerging in the quantum-gravity-phenomenology community concerning the possibility of describing dynamical effects within the framework of effective low-energy field theory. The fact that both in the study of noncommutative spacetimes and in the study of Loop Quantum Gravity, the two quantum pictures of spacetime that provide the key sources of motivation for research on Planck-scale modifications of the dispersion relation, we are really only starting to understand some aspects of kinematics, but we are still missing any robust result on dynamics, encourages an approach to phenomenology which is correspondingly prudent with respect to the description of dynamics. The phenomenologist is therefore confronted with two
options: For those who are most concerned about the status of the description of dynamics, the pure-kinematics minimal AEMNS test theory provides a rather reasonable starting point for phenomenology work. For those who are willing to set aside these concerns, and go ahead with the effective-field-theory description, the minimal GPMP test theory could provide a valuable starting point. It is interesting that, while the phenomenology based on pure kinematics is allowed to start with the assumption of full universality of the modification of the dispersion relation, the choice of describing dynamics in terms of an effective low-energy field theory forces upon us from the very beginning a nonuniversality of the effects, with the correlation between polarization and sign of the modification for photons (and, with the additional natural assumption of no net effect on randomly composed beams, one then can introduce for fermions an analogous correlation between helicity and sign of the modification). This plays a key role in the phenomenology.

In the Subsections 4.9, 4.10, 4.11, 4.12 I have considered a few examples of phenomenological analyses which exposed very clearly the type of differences that one can encounter comparing the indications of preliminary sensitivity estimates and the outcome of more robust analyses supported by test theories. The time-of-travel analyses described in Subsection 4.9 can be used to constrain the photon dispersion relation both in the AEMNS and in the GPMP test theory, but the strategy may be somewhat different: while in the AEMNS test theory one can only exploit the energy dependence of the new effects, in the GPMP test theory the additional polarization dependence can also be exploited. The type of analysis of the cosmic-ray spectrum described in Subsection 4.10 is also applicable to both test theories, but also in that case some differences must be taken into account. In particular, by obtaining good-quality data on the cosmic-ray spectrum around the GZK scale we might be in a position to completely rule out the minimal GPMP test theory, and to rule out the negative-$\eta$ case for the minimal AEMNS test theory. The photon-stability analysis described in Subsection 4.11, which received much attention in the literature, actually turned out to be affected by severe limitations in constraining the parameter spaces of the minimal AEMNS and the minimal GPMP test theories: photon-stability analyses must be treated prudently from a AEMNS perspective because in principle kinematics is insufficient for establishing the probability of particle decay (whereas kinematics is enough for establishing stability), and photon-stability analyses only lead to rather weak limits on the minimal-GPMP parameter space because of the polarization dependence expected in that test theory (one would need an ideally polarized beam of ultra-high-energy photons in order to be able to infer some constraint on the GPMP test theory). The Crab-nebula synchrotron-radiation analysis, whose preliminary analysis had also raised high hopes, when set up within the test theories here of interest also proves to be largely ineffective: it is not applicable to the AEMNS test theory (once again because of the role that some aspects of dynamics play in the analysis) and it also leads to no constraint on the minimal GPMP test theory.
While, consistently with the objectives of these lectures, it was for me sufficient here to discuss this comparison of test theories to data at a semi-quantitative level, the striking results of this comparison, showing that the analysis at the test-theory level can have very different outcome with respect to the usual preliminary sensitivity estimates, should provide motivation for future publications with detailed quantitative analyses of the emerging experimental bounds.

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