physics/0311037

Quantum Gravity Phenomenology

Giovanni AMELINO-CAMELIA

Dipartimento di Fisica,
Università di Roma "La Sapienza"
and INFN Sez. Roma1
P.le Moro 2, 00185 Roma, Italy

ABSTRACT

I give a brief non-technical review of "Quantum Gravity Phenomenology" and in particular I describe some studies which should soon allow to establish valuable data-based constraints on the short-distance structure of spacetime.

The "quantum-gravity problem" has been studied for more than 70 years, but we still do not have a single experimental result whose interpretation requires us to advocate a quantum theory of gravity. The search of a first case in which data provide some evidence of a quantum property of spacetime is at this point understandably characterized by a certain anxiety. Even the status of this research as a truly scientific endeavor could be questioned if the present lack of confrontation with experiments persists. And it does not help that one can still encounter in the literature (although it is fortunately becoming increasingly rare) some descriptions of the "quantum-gravity problem" that are not of the type that one expects in introducing a truly scientific problem. For example, as motivation for reseach in quantum gravity it is sometimes mentioned that it is unsatisfactory to have on one side our present description of electromagnetic, weak and strong forces, unified within the Standard Model of particle physics, which is a quantum field theory, and on the other side gravity described by General Relativity, which is governed by the very different rules of classical mechanics.

This type of "human discomfort", this type of urgency to correct the lack of elegance of our present worldview, does not of course define a scientific problem. But there is a well-defined scientific problem which can be naturally called "quantum-gravity problem". This is the problem of obtaining quantitative predictions for processes in which both gravity effects and Standard-Model effects cannot be neglected. The (quantum) Standard Model of particle physics has been hugely successful in describing the microscopic phenomena involving fundamental particles, where gravity can be ignored, and (classical) General Relativity has been equally successful in describing the motions of macroscopic bodies, whose quantum and Standard-Model properties can be safely neglected. We have no available data on situations in which neither can be neglected, but we already know that we would not be able to describe those data with our current theories, because some logical and mathematical inconsistecies are encountered even before getting to the point of obtaining a numerical prediction for these processes. Like two pieces of different jigsaw puzzles General Relativity and the Standard Model cannot be merged without modification.

What would happen if we managed to collide two photons each with energy of, say, $10^{50}eV$? This is a typical question to which we are unable to provide even a tentative answer. According to our present theories there should not be anything peculiar in the setup of such a collision (just a higher-energy version of the collisions we already set up at CERN and other laboratories), but then those same theories do not allow us to obtain a consistent prediction for the outcome of the collision. One might argue that $10^{50}eV$ photons should be the least of our concerns, since we are never going to be able to produce and/or observe them, but in cosmology there are some key issues which require the understanding of ultra-high-energy processes. Moreover, the fact that our theories fail to generate consistent predictions in some hard-to-produce contexts makes us concerned in general about the robustness of these theories. Since we know that new elements would have to be introduced in our theories for the description of ultra-high-energy processes, it is natural then to wonder whether those new elements can affect also some contexts in which our present theories do appear to make sense, contexts that perhaps are more reacheable than, say, the one of $10^{50}eV$ photons.

The fact that we know so little about the quantum-gravity problem has a simple explanation. One of the few (perhaps the only) relatively robust hints that we have about quantum gravity is that our conventional description of spacetime should start to break down at the Planck length $L_p \sim 10^{-35}m$ (which can be also described as the inverse of the Planck energy scale $E_p \sim 10^{28}eV$). At this scale, gravitational interactions between particles are no longer negligible. And, based on our experiences in other similar situations, we expect that this scale should also dictate how big the effects in the new quantum-gravity theory will be: for example in processes involving two particles both with wavelength λ the magnitude of the new effects should be set by some power of the ratio between the Planck length and the characteristic wavelength of the process L_p/λ. Since in all cases accessible to us experimentally L_p/λ is extremely small, it is not surprising that the quantum-gravity effects are nearly inevitably negligible.

While we lack any experimental insight on the quantum-gravity problem, it is undeniable that quantum-gravity research has produced some results that appear to be relevant for the solution of the problem. If nothing else, some theory results over the last few decades have changed significantly our intuition. For example, results obtained in string theory [1] provide encouragement for the idea that quantum gravity could admit a perturbative treatment, at least in certain contexts. Before these string-theory results it appeared that quantum gravity should in all cases be treated using (to-be-determined) nonperturbative techniques, with obvious associated difficulties. The Loop Quantum Gravity [2, 3] research programme has looked at the problem from a complementary perspective, and its development provides encouragement for the idea that a truly background-spacetime-independent quantum theory can be constructed. Before these studies it appeared that there would be a more profound conflict between the background-spacetime independence of general relativity and the fact that quantum field theory assumes from the start a background spacetime. Another example of a change of intuition comes from research on certain types of noncommutative spacetimes (see later in these notes and Refs. [4, 5, 6, 7, 8, 9]), which were the first examples of quantum spacetimes in which it appeared plausible that the Planck scale might affect some of the familiar spacetime symmetries, most notably Lorentz symmetry.

Unfortunately, Loop Quantum Gravity can only be treated (so far) as a fundamentally nonperturbative theory, without access to the tools of perturbative analysis which are so valuable when we attempt to obtain quantitative predictions. And on the other hand (in spite of recent advances [1]) we do not have a genuinely background-independent formulation of string theory (which, I expect, should introduce an *a priori* limitation to the class of conceivable measurement procedures for which string theory can provide consistent predictions). The development of approaches to the quantum-gravity problem based on noncommutative geometry is at an even earlier stage of development, since we do not even have, at present, a compelling candidate for "noncommutativegeometrodynamics" (a generalization of General Relativity that could apply to noncommutative geometries). Some of these (and other) unsolved issues are present in all approaches to the quantum-gravity problem, and of course the fact that these approaches have had no experimental success so far keeps them in the limbo of purely theoretical speculations. But finally we have some quantum-gravity approaches which are developed to a point where it appears plausible that they might have managed to capture at least some features of the correct theory. The history of the development of physics tells us that in many cases theories which ended up not giving a full solution of the problem of interest nevertheless managed to provide the right intuition for certain aspects of the full solution.

In attempting to profit from these hints coming from theory we are of course faced with the mentioned problem of the smallness of the expected effects, due to the smallness of the Planck length. For decades it had been assumed that such small effects could never be seen, but recently some strategies for overcoming the challenges posed by the smallness of the effects have been developed. The type of strategy which is used in this "Quantum Gravity Phenomenology" [10] can be described in analogy with certain experimental studies of proton stability. The prediction of proton decay in 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 the grandunification scale (an energy scale which should be only some three orders of magnitude smaller than the Planck energy scale). In spite of this horrifying suppression, with a simple idea we have managed to acquire remarkable sensitivity to the new effect: we keep under observation a large number of protons, so that, although the probability of decay of any given proton is very small, the probability that one of the many monitored protons would decay is observably large.

Analogously in Quantum Gravity Phenomenology we should focus on experiments which probe spacetime structure and host an ordinary-physics dimensionless quantity large enough that (like the number of monitored protons in proton-stability studies) it could amplify the extremely small effects we hope to discover. And, using this strategy, over the last few years several research lines within Quantum Gravity Phenomenology have been developed.

Perhaps the most exciting of these research lines are the ones connected with the idea of a "quantum spacetime". In unifying general relativity and quantum mechanics it is of course natural to contemplate the idea that space-time itself is quantized. In ordinary quantum mechanics the space that contains the three components of a particle's angular momentum is a "quantum space": one cannot make "sharp" (error-free) measurements of more than one of the components of angular momentum ("angular-momentum non-commutativity"), and a sharp measurement of a single component can take only certain discrete values ("angular-momentum discretization"). Similarly one may expect that quantum gravity should lead to spacetime noncommutativity and/or spacetime discretization. Space-time noncommutativity would, in its simplest form, prevent one from making error-free measurements of more than one of the coordinates of a point in spacetime. And if spacetime were discrete certain sharp spacetime measurements (of, say, volume or one of a point's coordinates) could then take only certain discrete values.

String Theory does not necessarily lead to spacetime quantization, at least in the sense that its background spacetime is commonly taken as a classical geometry. I fear that the assumption of a classical background spacetime may point to an incompleteness of String Theory. In fact, the formalism eventually leads to the emergence of a fundamental limitation on the localization of a spacetime event (see, *e.g.*, Refs. [11, 12]), and this might be in conflict with the assumption of a physically-meaningful classical background spacetime (whose points acquire operative meaning only if they can be sharply localized). In any case (even assuming that a classical spacetime background should be acceptable for String Theory) it has been found that under appropriate conditions (a vacuum expectation value for certain tensor fields) it is appropriate to set up an effective description in terms of a noncommutative spacetime [9].

In Loop Quantum Gravity spacetime is inherently discretized [13] and this occurs in a rather compelling way: it is not that one introduces by hand an *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 spacetime discretization. Moreover, some recent results [14, 15] could open the way for a role for noncommutative spacetimes also in the description of some aspects of Loop Quantum Gravity.

A description of space-time that is "fundamentally quantum" (quantized, in the sense of noncommutativity and/or discretization, even at the level of the background geometry) would require a profound renewal of fundamental physics. In particular, it should naturally have striking implications for the propagation of particles. As usual in quantum gravity, we expect that these effects would be very small, characterized by the Planck scale. A useful analogy is the one of the surface of a wooden table. We usually perceive the surface of the table as perfectly flat, but this flatness is the result of some sort of averaging over the short-distance irregularities of the table's surface. If we take a ball with diameter much larger than the short-distance scale of roughness of the table surface and roll it over the surface, we find no evidence of the roughness, but if we use a ball whose diameter is not much larger than the scale of roughness we will see disturbances in the path of the ball due to the roughness. Similarly, a quantization of spacetime at the Planck scale can affect the propagation of particles, in a way that depends on the "size" (wavelength) of the particles. In several recently-studied scenarios for spacetime quantization (most notably in Loop Quantum Gravity [2, 16] and in noncommutative spacetimes [5, 6, 7, 8, 9]), it has been shown that the "dispersion relation" between energy, E, and momentum, p, acquires energy/wavelength-dependent corrections: instead of the familiar classical-spacetime formula, $E^2 = p^2 + m^2$, one often finds[1] a formula of the type $E^2 + \eta E^{2+n}/E_p^n = p^2 + m^2$ (where η is of order 1 and n is an integer).

One context in which spacetime quantization can have observably large implications is the one of ultra-high-energy cosmic rays. According to the present understanding, cosmic rays are emitted by distant active galaxies and produce showers of elementary particles when they pass through the Earth's atmosphere. Before reaching our Earth detectors cosmic rays travel gigantic (cosmological) distances and could interact with photons in the cosmic microwave background. These interactions, which can cause energy loss through photopion production, are kinematically forbidden at low energies, but they are very efficient when the cosmic-ray energy is above the photopion-production energy threshold E_{th}. This leads to the expectation that by the time they reach the Earth a cosmic ray should have energy which does not exceed significantly E_{th}. However, the estimate of E_{th} depends very sensitively on the structure of the dispersion relation. Even a minute Planck-scale modification of the dispersion relation can induce a significant shift of the cosmic-ray threshold. Adopting the familiar $E^2 = p^2 + m^2$ one obtains an estimate of the cosmic-ray threshold that appears to be too low in comparison with the observations reported by the AGASA cosmic-ray observatory in Japan. Planck-scale modified dispersion relations can instead naturally fit [19, 20, 21] the AGASA data.

There are other plausible explanations for the AGASA "cosmic-ray puzzle", and even on the experimental side the situation must be considered as preliminary while waiting for more powerful observatories, such as Auger, in Argentina. Still, the possibility that in these cosmic-ray studies we might be witnessing the first manifestation of a quantum property of spacetime is of course very exciting; moreover, whether or not they end up being successful in describing cosmic-ray observations, these analyses provide an explicit example of a minute Planck-scale effect that can leave observable traces in actual data.

---

[1] While familiar contexts in which one encounters deformed dispersion relations (such as the case of the study of the propagation of light in certain crystals) are such that there is a preferred frame, in quantum gravity it is conceivable that such dispersion relations would emerge as observer-independent features of spacetime structure [17]. I attempted to discuss this point nontechnically in the recent paper in Ref. [18].

Besides the cosmic-ray threshold, another key implication of Planck-scale-modified dispersion relations is the prediction that the time needed by a massless particle to propagate over a given distance should depend on the wavelength/energy of the particle. It has been established [22, 23, 16] that observations on gamma-ray bursts can be used to look for this effect. Gamma-ray bursts provide us clusters of photons emitted by the source roughly simultaneously, with millisecond uncertainty[2]. If there was even a tiny (Planck-scale suppressed, as usual) dependence of the speed of photons on their wavelength the fact that these photons travel over huge cosmological distances would lead to a significant correlation between time of arrival on Earth and wavelength (a correlation that, for photons with energies higher than $10MeV$, could lead to an effect which is significant in comparison with the millisecond simultaneity available at the source). The next generation of gamma-ray telescopes, most notably the GLAST space telescope, should allow a noteworthy investigation [24] of this scenario.

These studies of Planck-scale deformations of the dispersion relation (studies based on observations of cosmic rays and gamma-ray bursts) constitute the best developed research line in quantum-gravity phenomenology, but there are several other topics which are under intense investigation. One noteworthy example are studies of the popular quantum-gravity intuition that at a fundamental level the concept of distance should be "fuzzy", it should be affected by unavoidable quantum fluctuations. Spacetime noncommutativity was introduced also as a possible way to describe mathematically this intuition, but it remains extremely hard to deduce from a given proposal of spacetime noncommutativity the associated picture of spacetime fuzziness. In Loop Quantum Gravity it is natural to expect such quantum fluctuations (whereas in String Theory this point might be affected by the *a priori* assumption of a classical background spacetime), but again there is still no success in characterizing the effect in a way that is useful for experiments. This impasse on the theoretical front of course does not prevent us from pursuing the issue experimentally, although the phenomenology of course pays a price for the lack of guidance from fundamental theories. Phenomenology of "distance fuzziness" has primarily focused on its possible implications for interferometry. Conventional interferometry relies on the assumption that there is a reference sharp (classical) concept of length of the arms of the interferometer. Preliminary studies in which the classical length is replaced by a "fuzzy" length have considered both atom interferometers [25] and laser-light interferometers [26, 27]. These studies showed that it is not unconceivable that small Planck-scale fuzzy-distance effects would be seen in the foreseeable future, but since we are limited to purely phenomenological considerations (because of the mentioned lack of guidance from more developed quantum-gravity theories) it is difficult to have an intuition for the robustness of the estimates.

Related analyses based on spacetime fuzziness, and the associated subject of quantum-gravity-induced decoherence, can also be performed in the context of neutral-kaon experiments [28]. Instead of the high precision of interferometry, these studies exploit an experimental technique in which a crucial role is played by the remarkable smallness of the mass difference between the long-lived and the short-lived neutral kaons. Also in this case purely-phenomenological sensitivity analyses lead to tentatively encouraging estimates, but so far there is no evidence of the effect in data [29].

The typical exercise of the "quantum-gravity-phenomenology community" is a situation in which the smallness of the Planck scale is the key challenge. But I should stress here that recently there has been some work on the possibility that quantum gravity might show up at lower energy scales. There is no good reason for this to happen, but it is a possibility, and of course it should be explored. In particular, over these past few years there has been strong interest in experiments looking for a key feature of String Theory: the fact that its mathematical consistency requires the existence of some extra spacetime dimensions, in addition to the four we experience directly. It turns out to be rather natural to assume

---

[2] Here I am actually thinking of a microburst, within the whole gamma-ray-burst event [22].

that these extra dimensions be of finite Planck-length size. For this most studied scenario for String Theory, in which the extra dimensions are, as expected for typical quantum-gravity effects, characterized by the Planck length, all attempts to find opportunities for experimental tests have failed. However, over these past few years there has been interest [30] in the possibility that some of the extra dimensions might have a size which is much larger than the Planck length. The most studied scenario assumes two large extra dimensions and has been shown to lead to important effects already at the level of classical gravity, effects which would be significant in experimental studies of Newton's $1/r^2$ law when the distance r between two test masses is at or below the $10^{-4}m$ scale. Experiments looking for these effects adopt various strategies. For example, Newtonian gravity predicts that there is no net force between a cylinder and a cylindrical shell that surrounds it, but any deviation from the inverse-square law would give rise to a net force. Experiments looking for such a force have improved significantly over the last few years, but we still have no evidence of departures from Newton's inverse-square law even for distances down to $10^{-4}m$ [31]. It is amusing that these classical-gravity tests can provide an important hint on the quantum-gravity problem, since the discovery of extra dimensions would of course be of encouragement for String Theory, which is the only known theory that requires extra dimensions for its consistency.

In closing I want to stress that finally an experimental programme for quantum gravity is starting to be developed, but it remains extremely difficult. The fact that String Theory and Loop Quantum Gravity have matured to the point of providing us some guidance concerning the nature of the effects that might characterize quantum gravity, and the recent advances in noncommutative geometry (both as a fundamental quantum picture of spacetime and as an effective-theory description of some aspects of String Theory and Loop Quantum Gravity) have been very valuable for Quantum Gravity Phenomenology. It is difficult to even attempt guessing where we might be in 15 or 20 years. By then we will know, through studies of cosmic rays and gamma-ray bursts, whether or not there is a Planck-scale modified dispersion relation. If that response is negative perhaps one of the other reasearch lines I here mentioned will stumble upon the much needed first experimental evidence of a quantum property of spacetime. As usual in phenomenology, no promises can be made concerning the time needed for a first discovery. But one goal that Quantum Gravity Phenomenology will surely achieve in the short term is the one of forcing the quantum-gravity debate to focus on the potentially observable aspects of any given quantum-gravity theory. For too long this research had been in a limbo at the interface between physics, mathematics and phylosophy. The excuse for avoiding the difficulties of a more genuinely scientific attitude toward the problem was provided by the smallness of the Planck length. Now that we are planning the first few experiments with genuine Planck-length sensitivity, we can no longer hang on to our old excuse.

Further Reading

These notes were prepared while working on an invited contribution to vol.16 no.11 (November 2003 issue) of Physics World, which focused on quantum gravity. While I here focused on "Quantum Gravity Phenomenology", other areas of quantum-gravity research were discussed in Physics World vol.16 no.11 (2003) 29-34 (by L. Susskind) and Physics World vol.16 no.11 (2003) 37-41 (by C. Rovelli). Ref. [32] contains a few recent descriptions of the status of quantum-gravity research. Ref. [33] contains a few papers on some strategies for searching for Planck-scale effects which I did not discuss in these short notes. Ref. [34] contains some references on the possible role of Planck-scale-modified dispersion relations in cosmology.

References

[1] L. Susskind, Physics World vol.16 no.11 (2003) 29-34.

[2] L. Smolin, Physics World vol.12 no.12 (1999) 79-84; hep-th/0303185;

[3] C. Rovelli, Physics World vol.16 no.11 (2003) 37-41.

[4] S. Doplicher, K. Fredenhagen and J.E. Roberts, Phys. Lett. B331 (1994) 39; D. Bahns, S. Doplicher, K. Fredenhagen and G. Piacitelli, hep-th/0301100, Commun. Math. Phys. 237 (2003) 221.

[5] S. Majid and H. Ruegg, Phys. Lett. B334 (1994) 348.

[6] J. Lukierski, H. Ruegg and W.J. Zakrzewski, Ann. Phys. 243 (1995) 90.

[7] G. Amelino-Camelia and S. Majid, hep-th/9907110, Int. J. Mod. Phys. A15 (2000) 4301.

[8] J. Madore, S. Schraml, P. Schupp and J. Wess, hep-th/0001203, Eur. Phys. J. C16 (2000) 161.

[9] N.R. Douglas and N.A. Nekrasov, Rev. Mod. Phys. 73 (2001) 977.

[10] G. Amelino-Camelia, gr-qc/9910089, Lect. Notes Phys. 541 (2000) 1; gr-qc/0204051, Mod. Phys. Lett. A17 (2002) 899.

[11] E. Witten, Phys. Today 49 (1996) 24.

[12] G. Veneziano, Europhys. Lett. 2 (1986) 199; D.J. Gross and P.F. Mende, Nucl. Phys. B303 (1988) 407; D. Amati, M. Ciafaloni, G. Veneziano, Phys. Lett. B216 (1989) 41; K. Konishi, G. Paffuti, P. Provero, Phys. Lett. B234 (1990) 276; D. Kabat and P. Pouliot, Phys. Rev. Lett. 77 (1996) 1004. M.R. Douglas, D. Kabat, P. Pouliot and S.H. Shenker, Nucl. Phys. B485 (1997) 85; T. Yoneya, hep-th/0010172, Int. J. Mod. Phys. A16 (2001) 945.

[13] C. Rovelli and L. Smolin, Nucl. Phys. B442 (1995) 593.

[14] G. Amelino-Camelia, L. Smolin and A. Starodubtsev, hep-th/0306134.

[15] L. Freidel, J. Kowalski-Glikman and L. Smolin, hep-th/0307085.

[16] R. Gambini and J. Pullin, gr-qc/9809038, Phys. Rev. D59 (1999) 124021; J. Alfaro, H.A. Morales-Tecotl and L.F. Urrutia, gr-qc/9909079, Phys. Rev. Lett. 84 (2000) 2318.

[17] G. Amelino-Camelia, gr-qc/0012051, Int. J. Mod. Phys. D11 (2002) 35; J. Kowalski-Glikman, hep-th/0102098, Phys. Lett. A286 (2001) 391; J. Magueijo and L. Smolin, gr-qc/0207085, Phys. Rev. D67 (2003) 044017.

[18] G. Amelino-Camelia, gr-qc/0207049, Nature 418 (2002) 34.

[19] T. Kifune, astro-ph/9904164, Astrophys. J. Lett. 518 (1999) L21.

[20] R. Aloisio, P. Blasi, P.L. Ghia and A.F. Grillo, astro-ph/0001258, Phys. Rev. D62 (2000) 053010.

[21] G. Amelino-Camelia and T. Piran, astro-ph/0008107, Phys. Rev. D64 (2001) 036005; G. Amelino-Camelia, gr-qc/0012049, Nature 408 (200) 661.

[22] G. Amelino-Camelia, J. Ellis, N.E. Mavromatos, D.V. Nanopoulos and S. Sarkar, astro-ph/9712103, Nature 393 (1998) 763.

[23] S.D. Biller et al, gr-qc/9810044, Phys. Rev. Lett. 83 (1999) 2108; B.E. Schaefer, astro-ph/9810479, Phys. Rev. Lett. 82 (1999) 4964.

[24] J.P. Norris, J.T. Bonnell, G.F. Marani and J.D. Scargle, astro-ph/9912136; A. de Angelis, astro-ph/0009271.

[25] I.C. Percival, Physics World vol.10 no.3 (1997) 43-48; I.C. Percival and W.T. Strunz, Proc. R. Soc. A453 (1998) 431.

[26] G. Amelino-Camelia, gr-qc/9808029, Nature 398 (1999) 216; gr-qc/9903080, Phys. Rev. D62 (2000) 024015; gr-qc/0104086, Nature 410 (2001) 1065; gr-qc/0104005.

[27] Y.J. Ng and H. van Dam, gr-qc/9906003, Found. Phys. 30 (2000) 795.

[28] J. Ellis, J.S. Hagelin, D.V. Nanopoulos and M. Srednicki, Nucl. Phys. B241 (1984) 381; P. Huet and M.E. Peskin, Nucl. Phys. B434 (1995) 3; V.A. Kostelecky and R. Potting, Phys. Rev. D51 (1995) 3923; O. Bertolami, D. Colladay, V.A. Kostelecky and R. Potting, Phys.Lett. B395 (1997) 178; J. Ellis, J. Lopez, N.E. Mavromatos and D.V. Nanopoulos, Phys. Rev. D53 (1996) 3846; F. Benatti and R. Floreanini, Nucl. Phys. B488 (1997) 335; A. Apostolakis et al, Phys. Lett. B452 (1999) 425.

[29] N.E. Mavromatos, hep-ph/0309221.

[30] N. Arkani-Hamed, S. Dimopoulos and G.R. Dvali, Phys. Lett. B429 (1998) 263; G.F. Giudice, hep-ph/9912279, Int. J. Mod. Phys. A15S1 (2000) 440.

[31] S. Abel and J. March-Russell, vol.13 no.11 (2000) 39-44; E.G. Adelberger, B.R. Eckel and A.E. Nelson, hep-ph/0307284.

[32] C. Rovelli, gr-qc/0006061; S. Carlip, gr-qc/0108040, Rept. Prog. Phys. 64 (2001) 885; E. Alvarez, gr-qc/0307090.

[33] M. Gasperini, Phys. Rev. D38 (1988) 2635; R. Brustein, M. Gasperini, M. Giovannini and G. Veneziano, Phys. Lett. B361 (1995) 45; D.V. Ahluwalia, gr-qc/0202098.

[34] J. Kowalski-Glikman, astro-ph/0006250, Phys. Lett. B499 (2001) 1; S. Alexander and J. Magueijo, hep-th/0104093; S. Alexander, R. Brandenberger and J. Magueijo, hep-th/0108190, Phys.Rev. D67 (2003) 081301.