Reply to arXiv:1006.2126 by Giovanni Amelino-Camelia et al.

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Abstract

It was previously shown that models with deformations of special relativity that have an energy-dependent yet observer-independent speed of light suffer from non-local effects that are in conflict with observation to very high precision. In a recent preprint it has been claimed that this conclusion is false. This claim was made by writing down expressions for modified Lorentz-transformations the use of which does not reproduce the result. I will show here that the failure to reproduce the result is not a consequence of a novel and improved calculation, but a consequence of repeating the same calculation but making an assumption that is in conflict with the assumptions made to produce the original scenario. I will here explain what the physical meaning of either assumption is and why the original assumption is the physically meaningful one. I will then further explain why even making the differing assumption does not remove but merely shift the problem and why the bound derived by Amelino-Camelia et al is wrong.

1 Introduction

Deformations of special relativity (DSR) [1, 2, 3, 4] allegedly make it possible to introduce an energy-dependent speed of light in position space while still preserving observer-independence. In [5, 6] it has been argued that such an energy-dependent and observer-independent speed of light is in conflict with observations, at least to first order in energy over Planck mass, $E/m_p$. Problems with locality in DSR had been pointed out previously by Unruh and Schützhold [7] but had not been quantified. The main complication in making any statements about locality in DSR has been that no (agreed upon) formulation of the theory in position space is available.

Even in the absence of a formulation of the theory in position space, it has been claimed that if DSR was realized in Nature, we might be on the brink of observing first quantum gravitational effects manifesting themselves in a delay in the arrival time of high energetic photons from distant gamma ray bursts. This prediction that allegedly followed from DSR

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has received a lot of attention [8, 9, 10, 11]. In a nutshell, the prediction is that the time delay between two photons is of the order

\[ \Delta T = \frac{\Delta E_\gamma}{m_p} L, \]

where \( \Delta E_\gamma \) is the energy difference between the photons, \( m_p \) is the Planck scale, and \( L \) is the distance to the source. Usually, the energy of the low energetic photon is chosen to be much smaller than that of the high energetic one, such that the energy difference is essentially equal to the energy of the high energetic photon. The above formula has to be corrected when one takes into account the cosmological expansion [12, 13] and there might be an additional factor of order one present, but these details will not matter for the following.

Absent a satisfactory formulation of DSR in position space, the conclusion in [5, 6] was reached by making very few assumptions. In order to circumvent having to transform points in space-time whose transformation is unknown and whose meaning might be ambiguous, only worldlines have been transformed and events were defined by the intersection of worldlines. Starting from this, it is very easy to understand how inevitably problems with locality arise.

The observer-independent and energy-dependent speed of light \( c(E) \) together with knowing the transformation of the energy in the argument determines what the transformation of the speed of the photon must be. If \( E \) is transformed to \( E' \) then \( c(E) \) is transformed to \( c(E') \). The DSR transformations for the energy (momentum) are well known, and consequently, if \( c(E) \) is to have a physical meaning as a speed in position space, one knows how the speed of the photon transforms. The speed of the particle corresponds in the space-time diagram to an angle. The problem is that this angle which follows from the transformation of \( c(E) \) is not the same as the angle one would get from transforming the worldline under the usual special relativistic transformation. As a consequence, when one uses three worldlines that meet in one point to define a space-time event, two of which are defined by very low energetic particles for which a modification is negligible, then the unusually DSR transforming worldline generically causes the point to split up. The notion of a point, and with it a local interaction, then becomes ill-defined because it depends on the observer.

One should note here that of course knowing the angle of the transformed worldline, which follows from the transformation of \( c(E) \), does not entirely fix the worldline. In addition, one also needs to know at least one point on the worldline, and thus needs an additional assumption. If one considers the original worldline, one can do a usual Lorentz-transformation on it. The worldline that has the funny transformation behavior whose angle we know will not be parallel to the worldline obtained with the normal transformation. But these two lines will (in 1+1 dimensions and flat spacetime) always cross in exactly one point. The point in which the both lines meet is the one point for which one does not have the problem with splitting up. We will henceforth refer to this point that will play a role in our further explanation as the ‘fixed point’ of the transformation, indicating that it is the

\(^1\)In 3+1 dimensions the worldline with the angle obtained by the deformed transformation does not gen-
the only point on the particle’s worldline whose position is unaffected by whether one does the DSR transformation or the usual special relativistic transformation. It should be clear from this diagrammatic explanation that no matter where one chooses this point, the problem remains the same up to a translation.

In [14] and [15], attempts had been made to circumvent the conclusion drawn in [5, 6] that DSR is in conflict with already made experiments to high precision. In [16] it has been demonstrated that these attempts are either inconsistent or just reproduce the problem. Now, a new preprint [17] has been put forward with another attempt to circumvent the problem. It is good to see that the issue of nonlocality in DSR and the lacking formulation in position space is now being taken seriously. Unfortunately, as we will see, the new approach is neither new nor does it solve the problem.

In the following we use units in which $c = \hbar = 1$.

Please be advised that section 2 is a reply to the first version of [17] that appeared on the arXiv. In section 3 I will comment on the second version of [17]. The update has not affected the conclusions of this reply.

## 2 Amelino-Camelia et al’s paper

The first misunderstanding in Amelino-Camelia et al’s argument is easy to spot. They claim:

“*In Ref. [5] it was assumed [...] that there should inevitably be wild nonlocality near the origin of Bob and no anomalous nonlocality at the source of the high-energy photon, far away from Bob.*”

In fact however no such assumption was made in [5]. The assumption that was made instead is that Eq. (1) holds in all reference frames$^2$. This assumption that was made for physical reasons is equivalent to there being no nonlocality at the emission point (in the chosen example: the gamma ray burst) and it results, as laid out in [5], in the nonlocality at the detector. The rationale behind the choice made in [5] that Eq. (1) holds in all reference frames is first that if it did not hold in all frames, the frame in which it held would be a special frame. Second, in no paper about the conjectured time-delay in photons from gamma ray bursts was there any mentioning of whether the equation holds in Bob’s or Alice’s frame.

It is easy to see that these both assumptions – Eq. (1) holding in all reference frames and the emission point not being split – are equivalent. Recall what was mentioned earlier. We know the angle of the worldline after the DSR transformation since we know $c(E')$. Generally cross the worldline obtained by the usual transformation, but there too one can chose them to intersect in one point which for the following is the only relevant aspect.

$^2$One should understand the $L$ in this equation as being obtained in the very low-energy limit, such that it transforms just under the usual Lorentz transformation. One can consider this simply as a definition of the quantity in Eq. (1). It is possible to change this definition, but it does not change the conclusions as will become clear later.
We need to fix exactly one point on the worldline to know the entire worldline. This point can either be fixed at the detection (by use of the time-delay with Eq. (1)) or at the source, but one does not need both assumptions.

The difference between Amelino-Camelia et al’s calculation and the calculation in [5, 6] is then simply the choice of the fixed point of the transformation. Amelino-Camelia et al chose the fixed point of their transformation in the detector. They neither point out that there is a freedom of choice and that a different choice would have reproduced the result in [5, 6], nor do they examine the consequences of that choice, that being that Eq. (1) does not hold in all reference frames. Close to the fixed point the absolute difference between the special relativistic and the DSR result of a transformation is small. Consequently, close to the fixed point the problem is negligible. It is obvious however that the problem just moves elsewhere, in this case to the gamma ray burst. This is convenient for Amelino-Camelia et al because the gamma ray burst is far away and a messy, macroscopic process that is not well understood, but it doesn’t solve the underlying problem. For the sake of the argument, one can replace the gamma ray burst with some elementary scattering or decay process emitting the photons. Then one has the same problem for elementary particle physics as previously in the detector, just that it’s now at the source.

In Amelino-Camelia et al’s formalism, the choice of the fixed point corresponds to choosing the origin of the coordinate system. One can easily see this from Eq. (8) in their paper. The additional terms are proportional to $x$ and $t$ and consequently the usual special relativistic transformation is reproduced to good precision close by the origin of the coordinate system. The authors write:

"[T]he correct version [...] provides zero nonlocality in the origin, and even the most creative argument could not amplify that."

That is correct in their formalism, but actually not what the authors eventually claim to have shown. They want to claim that there is no nonlocality at the detector. One only has to put the origin of the coordinate system into the source to have exactly the same scenario that was previously discussed in [5, 6] and get back the nonlocality at the detector. Besides that, there is no good reason why the origin of the coordinate system has to be identified with the fixed point of the transformation.

Amelino-Camelia et al then attempt to derive constraints from their calculation. Since their setup and calculation is the same as [5, 6] up to a translation, it is apparent that the nonlocality that is caused by a relative velocity of $v \approx 10^{-4}$ over the distance $L \approx 4$ Gpc is exactly the same than what was computed in [5, 6]. The nonlocality is, for $v \ll 1$, given by

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3 Changing the definition mentioned in Footnote 2 has the same effect of just moving the fixed point which is why this change does affect the scenario by translating it, but does not affect the existence and magnitude of the problem.

4 The calculation in [5, 6] does depend on the location of the fixed point but not on the choice for the origin of the coordinate system.
We have inserted here a dimensionless factor $\alpha$ that is to be constrained by experiment. It can alternatively be absorbed in the mass scale $m_{\text{pl}}$. This equation is, not surprisingly, the same as the expression $(\lambda \xi Lp)$ in [17] (up to a factor 3).

The derivation of a constraint (on $\lambda$) that Amelino-Camelia et al put forward however is a very confused argument and their conclusion is wrong. They actually state that the limit on the nonlocality is given by the duration of the gamma ray burst, which is of the order seconds and consequently leads to a very weak bound. They fail to see that the nonlocality, if it was real, would instead blow up any vertex of any elementary interaction to the size of a km (for the relative velocity they have chosen). The actual bound they should have derived is the bound that one gets from knowing elementary particle physics applies in distance sources to very good precision. The typical timescale for these interactions is of the order fm, not seconds, which explains why Amelino-Camelia et al’s bound is many orders of magnitude weaker than the bound in [5,6]. The some orders of magnitude difference that are not explained by the mismatch from a second to a fm are due to the fact that they have not, as [5,6], used boosts up to the limit where we have experimental tests.

Now, to be fair, it might very well be that elementary particle interactions in 4 Gpc distance are not known to such a high precision as they have been tested in Earth laboratories. To my best knowledge however, we have so far no reason to believe there is an extreme difference between both. Our explanations of astrophysical processes back to the early universe work in fact astonishingly well with the local quantum field theories of the standard model. The situation for DSR looks at the present stage so hopeless that it seems moot to consider in more detail these astrophysical bounds for the case in which the nonlocality is shifted to the source. In any case, the bound that Amelino-Camelia et al put forward is based on their misunderstanding of the disastrous consequences of macroscopic nonlocality for elementary particle physics and grossly wrong.

Further, Amelino-Camelia et al make several incorrect remarks about the papers [5,6]. They claim that in [5,6] transformations have been performed for points. Instead, the whole point of the somewhat cumbersome construction presented in [5,6] was to not transform points whose transformation or even meaning is unknown. Instead, merely worldlines have been transformed and all points been constructed by their intersections. Amelino-Camelia et al go so far to proclaim in their conclusion:

"We have here provided the first ever explicit description of the worldlines of free classical particles in a DSR framework."

Their “first ever explicit description” however is merely a repetition of the calculation in [5,6], which made use of exactly the same simple fact that they now claim originality.

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5The relative velocity in the original equation was negative, which is why there is a minus missing here.
on: if one knows how the speed transforms, one knows how the angle of the worldline transforms. The only difference between both calculations is the choice of the fixed point of the transformation. As discussed previously, the assumption that Amelino-Camelia et al are making is unphysical or at least unmotivated. The assumption is also in direct conflict with the first author’s recent argument in [14], which used the same assumption as [5,6], namely that the time-delay is observer-independent. Either way, even if one buys the differing assumption, it does not solve the problem, as explained previously.

Amelino-Camelia et al further quote the following from my paper [5]:

“Now if one would use a modified transformation also on the coordinates, a transformation depending on the energy of the photon [...] This would imply that the distance between any two objects would depend on the energy of a photon that happened to propagate between them. The distance between the source and the detector was then energy-dependent such that it got shortened in the right amount to allow the slower photon to arrive in time together with the electron. That however would mean that the speed of the photon would not depend on its energy when expressed in our usual low-energetic and energy-independent coordinates.”

On which they comment:

“The remark we here reported in quotes assumes that a momentum dependence of the boost of a worldline should introduce a momentum dependence of the distance between points on that worldline. Instead, as explicitly shown by our formalization, even with laws of transformation of worldlines from Alice to Bob that do include a momentum dependence, both Alice and Bob have well-defined (momentum-independent) distances between points of a worldline, and even between points on two different worldlines.”

This comment first of all shows that the authors did not understand my remark. My remark was referring to an additional transformation on the coordinates, a transformation in addition to the one that had already been considered in [5,6], which is the same that Amelino-Camelia et al consider. This becomes clear in the part of the quotation that has been left out.

My remark was meant to open a door, going through which the conclusion drawn in [5,6] would no longer apply and DSR could be made consistent. That door being that if in addition to the energy-dependent transformation of the speed which causes the problem there was an energy-dependence of distances (as suggested by approaches with an energy-dependent metric [18, 19, 20, 21]), then the actual effective speed in position space could remain constant, and there was no troublesome nonlocality. It is quite ironic that the authors of [17] slam that kindly opened door in their own face by insisting their calculation, which does not solve but merely shift the problem, is the correct DSR result.

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The authors of [14] failed to acknowledge that the result they derive for the transformation of the delay is in fact the same as what was obtained earlier in [5,6].
The relevant remark that they should have quoted from [5] is instead this one:

“[O]ne might want to argue that maybe in the satellite frame the both photons were not emitted at the same time, such that still the electron could arrive together with the high energetic photon. This however just pushes the bump around under the carpet by moving the mismatch in the timescales in the satellite frame away from the detector and towards the source. One could easily construct another example where the mismatch at the source had macroscopic consequences. This therefore does not help solving the problem.”

With this, I anticipated the present attempt of Amelino-Camelia et al to circumvent the conclusion, and I have here explained in more details what I meant.

I leave it as an exercise to the reader what might happen to Amelino-Camelia et al’s solution attempt if the source dares to emit several photons in different spatial directions that are detected in different locations. It becomes very difficult then to put the origin of the coordinate system in all these detectors.

3 The update of Amelino-Camelia et al’s paper

After the first version of this reply was published, Amelino-Camelia et al updated their paper [17]. Unfortunately, the authors have not used the opportunity to correct the misleading statements pointed out here. Instead, the update contains additional confusions. For example, in the caption of the (added) Figure 5, one can read:

“In the incorrect argument of [5, 6] a key role is played by the assumption that in a DSR relativistic framework [the transformation of the emission point] is obtained by classical/undeformed boost of [the emission point] with speed obtained from [...] the DSR deformed boost. (Emphasis as in original)

First, as has hopefully become clear from the previous section, the key assumption for the argument presented in [5, 6] is that the speed of light is energy-dependent yet observer-independent. That is the only assumption necessary to arrive at the conclusion that the model is already ruled out by experiment to excellent precision. The additional assumption that the emission point is the fixed point of the transformation is only necessary to arrive at the exact scenario (with bomb and all) in [5, 6]. One needs this assumption in addition to the transformation of the speed (the angle of the worldline) to entirely determine the worldline of the particle. It is possible to exchange this specific assumption with some other assumption as Amelino-Camelia et al have done. This does change the scenario (it is then a translation of the one considered in [5, 6]), but it does not (significantly) change the bound. It does merely move, but not remove the nonlocality. Amelino-Camelia et al’s believe that the bound is (significantly) changed stems from having made a mistake in the derivation of the bound, as explained in the previous section.

Needless to say, also in their scenario there is one point whose transformation is the same no matter whether one uses the deformed or undeformed transformation. That’s by
definition the previously introduced fixed point, and in 1+1 dimensions it does always exist. In [17] that point is at \((0,0)\). Since in addition Amelino-Camelia et al have put the detector in the origin of the coordinate system, and there is negligible nonlocality close to the fixed point, they see no problem in the detector. After identifying the fixed point with the origin of the coordinate system, to reproduce the scenario in [5, 6], they would just need to consider the origin to be at the source. One can see this from simply looking at the figures in [17].

In the note added to the update the authors further write:

"[I]t should be even clearer for our readers that our results do not depend in any way upon hidden assumptions about peculiar properties of the spacetime point at the origin [...]"

Of course the properties of the origin, which is in their case the fixed point of the transformation are not ‘peculiar,’ since this fixed point does in 1+1 dimensions always exist for the simple reason that two straight non-parallel lines will (in flat space) cross in exactly one point. There is however no reason why this fixed point should be at the origin, and neither is there any reason why the origin should be located in some object called a detector. These identifications do certainly not follow from the invariance of \(c(E)\) alone but are additional assumptions.

As sketched here in the introduction, the transformation of the worldline that has been used in [5, 6] has been obtained by making use directly of the invariance of \(c(E)\) and the invariance of the equation for the time delay. Since also the update of [17] raises the impression the calculation done there is substantially different from the one previously done in [5, 6], it seems necessary to write down the relevant equations. In one restframe (say, the Earth restframe) with undashed coordinates, the photon is moving on the worldline

\[
(x - x_0) = c(E)(t - t_0) \quad ,
\]

(3)

where the point \((t_0, x_0)\) can be freely chosen. (In the scenario considered in [5, 6], it is \((t_0, x_0) = (0,0)\).) In the other restframe (the satellite frame) with dashed coordinates, the photon is moving on the worldline

\[
(x' - x'_1) = c(E')(t' - t'_1) \quad ,
\]

(4)

where \(E'\) is the DSR-transformation of \(E\) and \(x'_1, t'_1\) are unknown. This is what one gets just from knowing how \(c(E)\) transforms. As previously explained, to entirely determine the worldline one needs an additional assumption; one needs one point on the worldline. Since the fixed point is by definition always on the worldline, it is most convenient to use it for this purpose. In the scenario considered in [5, 6] the fixed point is the emission point, consequently we know that the ordinary Lorentz-transformation of that point is also on the DSR-transformed line, which then entirely determines it. Since there is only one fixed point on this worldline, this has the consequence that the point \((0,0)\) can no longer be on the line which is why there is a miss at the detector. Eqs. (3,4) are such a trivial consequence.
from the assumption that $c(E)$ is the speed of the particle and transforms into $c(E')$ that they were not explicitly written down in [5, 6]. The interested reader will however verify easily that these are the equations that have been used to derive the results in [5, 6].

Needless to say, Eq. (4) is similar to Eq. (14) in [17]:

$$(x' - x'_0) = c(E')(t' - t'_0) \ .$$

The equation is not exactly the same because Amelino-Camelia et al identify $(t'_0, x'_0) = (t'_1, x'_1)$ and later set $t'_0 = x'_0 = 0$. This does not follow from requiring the invariance of the speed of light alone. Or maybe more obvious, there is no reason why the origin should be the detection rather than the emission event.

Note that to arrive at Eq. (14) [17] all of the previous steps in Amelino-Camelia et al’s paper are entirely unnecessary. One can literally write down the equation directly from the central assumption that $c(E)$ is observer-independent, together with choosing the fixed point.

Finally let me comment on the remark (caption of Fig 5):

"The logical inconsistency of such criteria of "selective applicability" of deformed boosts could have been easily spotted by contemplating the possibility of several photons sharing the same worldline but emitted from points at different distances from the origin of Alice [...]."

First, we note that this is a repetition of the final remark in the previous section of this reply. It should be clear by now that Amelino-Camelia et al’s scenario just moves the problems from the detector to the source. Consequently my remark in the previous section that their scenario becomes highly problematic when there’s more than one detector (where to put the fixed point?) is the same as their now added insight that my scenario becomes problematic when there’s more than one source (same problem, where to put the fixed point?). Thus, they actually point out an inconsistency in their own scenario.

The difference between my argument and that of Amelino-Camelia et al is that they are the ones who are trying to show that DSR can be made consistent, whereas it is irrelevant for my claim that DSR in conflict with experiment whether it can be made consistent in the first place. There are in fact other indications than the arising non-locality that create doubts about the consistency of the theory, for example the so called soccer-ball problem (difficulties with the multi-particle description). These additional problems have just been left aside for the argument in [5, 6] in order to arrive at a particularly simple scenario from which the constraint could easily be extracted.

Footnotes:

7 For the massless case $\Pi'/\sqrt{m^2 + \Pi'^2} - \lambda \Pi'$ is in first order $c(E')$.

8 If one does not restrict oneself to experimental parameters that are achievable today, but extends the analysis to all boosts and distances, one arrives at the conclusion that there is an arbitrarily large non-locality everywhere and consequently all points of the universe must be understood as the same point. This poses a serious conceptual problem for the model, in particular for the interaction picture.
4 Conclusion

I have shown here why the attempt of Amelino-Camelia et al to circumvent the problems with nonlocality in DSR fails. For this, I have pointed out exactly which hidden assumption they are making as a consequence of which they arrive at a different result. The result of the calculation depends on their choice for the origin of the coordinate system and they chose it such that, in the original example that I used, the problem is shifted away from the detector and towards the gamma ray burst. A different choice would have exactly reproduced the original setup. I have explained why their hidden assumption is unphysical and why, even if one accepts it, it does not solve the problem. Finally, I have shown that the weakness of the bound derived by Amelino-Camelia et al is due to inappropriately neglecting the disastrous consequences that nonlocality has for elementary particle physics.

Acknowledgements

I thank Stefan Scherer and Lee Smolin for helpful discussions. Despite this reply making very clear that the attempted solutions to the problem are so far unsuccessful, I would like to acknowledge that I appreciate the issue of nonlocality in DSR is now given appropriate attention.

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