Temat: Your manuscript DQ12278 Krasinski
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Re: DQ12278
Short-lived flashes of gamma radiation in a quasispherical Szekeres metric
by Andrzej Krasiński

Dear Professor Krasinski,

The above manuscript has been reviewed by two of our referees. In addition to the original referee (Referee A), we also consulted a new referee (Referee B). Comments from the reports appear below.

We regret that in view of these comments we cannot accept the paper for publication in the Physical Review.

Yours sincerely,
Ansar Fayyazuddin
Associate Editor
Physical Review D
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Second Report of Referee A – DQ12278/Krasinski

I do not recommend the paper for publication.

I answer to author's reply below:

1. '"Unlikely to be true", without solid arguments, is not a valid justification for rejection.'

My arguments were:

a) I pointed out that there is a logical fallacy in the author's approach. This may be clarified as follows. The author claims

'The whole construction is meant to be a *proof of existence* of a mechanism that shortens the GRB duration, and a single example is all that is needed for such a proof. Thus, the reprimand for "particular elements carefully chosen to match basic properties of GRBs" is a logical error, in addition to being false.'

In a series of papers the author provides the construction (using spacetime gluing technique) of a single example of the gamma-ray burst (GRBs) which 'arise from suitably shaped nonuniformities in the Big Bang in a quasi-spherical Szekeres (QSS) model'. (I copied this from the abstract.) The author assumes that 'a single example is all that is needed for such a proof' (a proof of a mechanism that generates GRBs). Unfortunately, existence of this example, which has been constructed a posteriori (we know the properties of GRBs) is meaningless. The author's point of view is a common scientific mistake, as nicely explained by Richard Feynman:

'That brings me to the fourth kind of attitude toward ideas, and that is that the problem is not what is possible. That's not the problem. The problem is what is probable, what is happening. It does no good to demonstrate again and again that you can't disprove that this could be a flying saucer. We have to guess ahead of time

whether we have to worry about the Martian invasion. We have to make a judgment about whether it is a flying saucer, whether it's reasonable, whether it's likely. And we do that on the basis of a lot more experience than whether it's just possible, because the number of things that are possible is not fully appreciated by the average individual. And it is also not clear, then, to them how many things that are possible must not be happening. That it's impossible that everything that is possible is happening. And there is too much variety, so most likely anything that you think of that is possible isn't true.'

Similarly, the author constructs an example of GRBs mechanism trying to show that it cannot be disproved, but as Feynman said such demonstration does no good. Flying saucers are not promising explanation of crop circles, because there is no other reliable evidence for existence of flying saucers. It does not matter that this hypothesis cannot be easily disproved. Similarly, we do not have any evidence that the dust QSS model describes nonuniformities in the Big Bang, so artificial GRBs mechanism based on this assumption is very unlikely to be true.

b) I pointed out that the author's construction is artificial. Going into details: it starts with a 'suitably shaped nonuniformities in the Big Bang in a quasi-spherical Szekeres (QSS) model' and, necessarily, goes through additional QSS region. A lot of goodwill is needed to believe that a dust solution may be applied to the cosmological era in which radiation cannot be neglected. (There is even no empirical evidence that one should go beyond perturbation theory.) We are not told why there should be only one additional QSS region and there is no other empirical evidence that GRB on its way from the progenitor goes through any region that cannot be described by a linear perturbation theory. The author study only geodesic equation and neglects the effect of gravitational lensing which very likely cannot be neglected in his scenario.

c) I pointed out that the big part of the paper was a 'copy and paste' from the previous author's article. The author disagree: 'the repeated text was contained in less than 4 pages out of 18'. I think that my comment was fair: a rough estimation shows that repeated equations constituted 40-50 percent of all equations in the paper. I did not accuse the author of a self-plagiarism. I indicated that I know why the text has been repeated and that the paper contains new results. My comment was merely to point out that there is something wrong with the construction of the paper if almost half of all equations appeared in the previous article.

2. 'The referee did not verify the paper's physical and mathematical correctness. He only formulated some vague general objections (mostly mistaken - see below), without references to specific parts of the paper, and performed a routine pattern-comparison using an electronic text comparator.'

Indeed, my objections were not related to the mathematical correctness. In my opinion, the paper is based on a logical fallacy (see 1a above). I also clearly explained why I think the model is unphysical. I did not use an electronic text comparator. Some time ago, when I was writing another review I have discovered that some other authors use a 'copy and paste method' to produce a large number of papers which do not include any substantial improvements. Now, I routinely compare old and new papers of the author whenever I am writing a review.

3. 'Contrary to what the referee implies, Fig. 2 here is NOT the same as Fig. 2 in Ref. [3]; the difference is explained in my footnote 2.'

Contrary to what the author implies I wrote:

Figures (A) $\leftrightarrow$ (B)

1 $\leftrightarrow$ 1 (the same caption)

2 $\leftrightarrow$ 2 (small differences)

so I never claimed that Fig. 2 is exactly the same as Fig. 2 in Ref. [3].

3. 'The referee said (apparently after being asked for some details by the Editor) "Of course, there are also new results in the paper", but said nothing more about them. A serious report would spell out the new results and say whether they are correct or not, instructive or not, clearly presented or not.'

I was not asked about new results by the Editor. I wrote this sentence to indicate that the paper is not a simple example of plagiarism. I also indicated in my report that the results are clearly presented (in a questionnaire) and spelled them out:

'The duration of GRBs which followed from the previous work on this model did not fit observations, thus the author enriched the model in the last paper to match the data. The one more inhomogeneous interaction region with appropriate density profile has been added on the way of the GRB.'

It does not follow from the results presented in the paper if the model will work for a different density profile. For sure it cannot work if the QSS2 inhomogeneities will mild or very strong and for sure it must work differently, if more similar inhomogeneous regions will be added. If fine-tuning is not hidden in the QSS2 density profile than it is hidden in the number of QSS regions.

In my opinion, the amendments introduced by the author improved the paper, but I cannot recommend it for publication (as explained in the point 1a above).

Report of Referee B – DQ12278/Krasinski

The author studies how the short duration of observed gamma-ray bursts (GRB) may be explained in a speculative model where GRBs are blueshifted emission from the last scattering surface. The explanation requires the blueshifted ray to traverse two extremely inhomogeneous regions, modelled using the Szekeres metric.

There is no reason presented for the origin of such regions and there is no observational evidence for them. Also, requiring the light ray to go through two such regions seems like implausible extra tuning. (Surely there would be many more rays that go through only one region.) The author, in his reply to referee A, justifies this with reference to the fact that "the real Universe is full of voids and condensations". However, the known over- and underdensities are not strong enough to produce such strong effects as required in the author's GRB model. (With the exception of black holes.)

Therefore, the model does not seem plausible enough to warrant such a study of fixing its problem that the GRB duration in the model is usually too long, especially as there are plausible astrophysical candidates for GRBs.

In summary, I do not find the manuscript of sufficient scientific interest for publication in Physical Review D.