This paper presents an extended analysis of a candidate gamma-ray burst (GRB) mechanism, based on localized extreme lensing (blueshifting) of the CMB. This was originally proposed by the author in a previous work. This paper includes new calculations to resolve some technical issues with the previous incarnation of the model, including reducing the angular size and increasing the blueshift of the GRB 'sources'.

While the author should be applauded for attempting a completely novel explanation of GRBs, the proposed solution contains several highly speculative elements, and appears to grossly contradict a number of well-established observational facts. I do not think it will be possible to resolve these issues with the model, even in principle, and so it does't seem worthwhile to continue its development. I recommend that the paper should not be published.

I have provided several points to support this conclusion below.

(1) The model works by placing large QSS regions close to the last-scattering surface of the observer. Extreme lensing events along a preferred axis of these regions then causes a large blueshift that shifts CMB photons into the gamma-ray part of the electromagnetic spectrum. The regions are of order a degree across as seen by the observer. The CMB photons that are deflected/blueshifted by the QSS regions are presumably not observed in the microwave band today. As the author points out (p15), there will therefore be large holes in the observed CMB. The primary CMB is extremely well-observed on degree scales, and there are no hints of any such holes. I see no way that such large, extreme lenses could fail to seriously disrupt the CMB, even if the model were substantially modified. CMB observations essentially rule out this entire class of models.

(2) It is unclear how this model could reproduce the observed frequency spectra of GRBs. Gravitational redshifts preserve blackbody spectra, changing only the temperature of the radiation. The author uses extreme blueshifts in QSS models to shift the peak frequency into approximately the correct range for observed GRBs, but the spectra will nevertheless remain in a blackbody form - quite unlike observed GRB spectra. It may be possible to construct a more realistic spectrum from the superposition of many blackbodies with different temperatures/blueshifts, but this was not explored in the paper, and would likely require a lot of fine tuning (if it is even possible). Without a plausible mechanism to explain the non-blackbody spectra of GRBs, it is hard to see how this mechanism can be considered viable. This is without mentioning the duration of the GRBs in this model, which are of order $10^4$ years according to Ref. 1 (versus ~ $10^4$ seconds for the longest lived population of GRBs detected so far). In short, the observable consequences of the proposed mechanism bear little resemblance to a GRB.

(3) It appears that the author can only produce an appropriate number of GRB sources on the sky if the configuration of QSS regions is highly fine-tuned, essentially covering the whole sky (i.e. most of the last-scattering surace). Furthermore, all of these sources must be aligned such that the blueshift axis is almost exactly pointing towards the observer. What are the odds that such a highly-aligned shell of QSS regions would have appeared so close to our last-scattering surface?

And why do we not see such regions at lower redshift? There are optical, IR, and UV backgrounds that could also be blueshifted into the gamma band. And why should all the QSS regions have profiles that are so finely tuned to give such extreme blueshifts? We might expect to see a population of objects with less extreme blueshifts in the X-ray or UV, for example. Surely a population of QSS regions with randomly-chosen profile shapes, positions, alignments etc. would result in a much larger number of non-GRB-like objects on the sky?

The fact that the size, spatial distribution, profile shape, and alignment of the QSS regions must all be separately highly fine-tuned to make this model even approximately work is rather devastating. Each of these aspects requires a tremendous degree of special pleading that would individually throw the viability of the model into question; taking them all together stretches the plausibility of the model far beyond the bounds of what is reasonable. It is fair to conclude that the model just doesn't work.

(4) The Summary and Conclusions section contains a number of dubious statements that are intended to support the plausibility of the model. For example, the author states that "Since general relativity clearly predicts that some of the light generated during last scattering should reach us with strong blueshift, consequences of this prediction have to be worked out and submitted to tests." The wording of this is somewhat misleading - the author has established that this scenario can be modelled in GR, but this is quite far from being able to say that there is a clear prediction that some of the CMB light in our actual Universe should be strongly blueshifted. That will only happen if the rather contrived conditions required by this model exist, but there is ample evidence to show that they do not (see above). So, this is not really a clear prediction of the kind that astronomers should go out and look for - it is more a theoretical possibility, unlikely to be realized in nature, along the same lines as a wormhole or a closed timelike curve.

On p15, the author also makes an argument that FLRW models are highly fine-tuned, and more generic models like LTB or QSS are less symmetric, and should therefore be more likely to be realized in nature. In some sense this can be considered true, but of course there are plausible physical mechanisms that invalidate this argument (e.g. a period of inflation would make it highly likely that the Universe would be driven to a locally almost-FLRW solution). It is also unclear what the proper probabilistic measure on the space of GR solutions should be - is FLRW really less likely than the highly fine-tuned model proposed by the author? Quantifying this would require the author to somehow define a sensible prior over the space of all manifolds allowed by GR. There is also the issue that the class of models usually used to describe the Universe on large scales is really perturbed FLRW, not FLRW in the strict sense. These allow much more freedom, invalidating at least the first and second points made by the author.

Finally, also on p15, the author makes a strange remark about "inflationary propaganda". This turn of phrase seems inappropriate for a scientific paper, and the remark is also technically incorrect - most inflationary models drive the spatial curvature to a small, but not necessarily zero value (e.g. see arXiv:1202.5037 or arXiv:1203.6876).