Third Report of Referee A – DD12236/Krasinski

The manuscript still doesn't address the previous review comments (with the exception of one minor point), and so continues to leave several serious issues unaddressed. As such, I continue to recommend against publication.

First, allow me to reassure the author that I took the preliminary nature of the model into account when preparing my comments. The problem is that the general concept is flawed (not just the specific instantiation described in this paper).

I have discussed why I think this is the case in some detail: the model has serious implications for other observables that are mostly being ignored (e.g. CMB anisotropies, shorter-wavelength observations); makes no predictions, or ambiguous predictions, for several key properties of GRBs (e.g. the shape of the frequency spectrum); and is fine-tuned in several ways (e.g. spatial distribution, choices of profiles).

In these respects, the model is a significantly worse explanation of GRBs than many others, and the author hasn't made a very compelling case that this can be corrected. I've asked for several changes that would potentially address this, mostly in the form of simple calculations and suggestions for physical mechanisms that could possibly explain certain features of the model. The author hasn't acted on these comments or made suitable changes to the manuscript though.

I don't understand why – the requested changes are pretty straightforward, and would either quickly confirm my recommendation to reject the paper, or result in only an extra paragraph or two being added to the text. They would certainly make the status of the model much clearer to readers. Instead, most of the author's responses in the last two rounds have been to argue that this small amount of work isn't needed, or can wait until some indefinite time in the future. What good is that to other scientists? If the model doesn't work, we'd rather find out sooner rather than later. It is well within the author's capacity to make a few simple, reasonable investigations to clarify these issues now, and so it seems inexcusable to continue dismissing them and insisting on making no meaningful changes to the manuscript whatsoever!

I see two possible paths to move forward with this paper:

(1) Address the review comments by making at least some of the small number of simple calculations and clarifications I have asked for. Make clear statements about what the model predicts for the frequency spectrum, CMB observations, and possible UV/X-ray phenomena. Describe any obvious discrepancies between these predictions and existing observations. Suggest plausible ways of fixing the discrepancies, or highlight them as flaws of the model that need to be solved. Discuss how these predictions might change if certain changes were made to the model (e.g. if the QSS regions were smaller). Also, improve the introduction to give a more comprehensive statement of the content of the model and assumptions that it makes (e.g. what the original source of the emission is). Address the fine-

tuning criticisms (whether you agree or not, others will think it is an issue).

(2) Reword the paper to remove most of the discussion about GRBs. There doesn't seem to be anything wrong with the QSS calculations themselves; pretty much all of the issues are with the interpretation of them as a GRB model. In this case, GRBs could be mentioned briefly as a possible motivation for the calculation, but anything more than that will require the various issues I have highlighted to be properly addressed.

I really can't recommend this for publication without substantial changes along the lines of (1) or (2).

Responses to specific points:

A1: Yes, the emission mechanism is of crucial interest to anyone interested in models of GRB emission, and should be included in the introduction.

A2: I am quite aware of the difference between blueshifting and lensing, but that was not my question. The QSS regions act as a large lens, deflecting light rays, and altering the pattern of CMB anisotropies in that region. There is also a blueshift, which also affects the CMB. Both are happening – Figs. 4-8 show a large amount of deflection of light rays passing through the region, for example. The CMB anisotropies are well-observed on scales *larger* than 1 arcmin. There is no evidence for this lensing/blueshifting in observations from Planck, ACT, SPT etc. etc. Even if the CMB photons are sourced within the QSS region, there will still be significant deflection.

The question remains: What would CMB experiments see in your scenario? We have lots of data from these experiments. What would it take for your model not to contradict those data?

A3: What would a CMB experiment see in this scenario? Presumably another region of the last-scattering surface would become visible when the second QSS region intervenes? Would that region be distorted in any way? I'm glad that you may have found a solution for the duration problem though.

A4: The author seems to agree with my comment.

A5: My concern is that the CMB will be noticeably distorted even before/after the flash. It would also be distorted (blacked out) by other, similar, QSS regions that are closer to us than the surface of last scattering. As stated elsewhere, it seems highly unlikely that these QSS regions would only be located exactly at our surface of last scattering and not also found at lower distances. These distortions would have already been observed by CMB experiments unless the regions are very small. It seems hard to get around this. The author hasn't offered a convincing argument otherwise.

A6: The point is that the regions are large with respect to the current resolution of CMB detectors.

A7: This issue could be put to rest if the author performed a simple estimate of what the maximum size of a region (and duration of burst) could be without being ruled out by CMB observations. We can then judge if the numbers that come out are reasonable or not. We would know exactly what counts as "sufficiently small" and what counts as a "brief period". As it stands, the author is just making assertions, which I don't find convincing.

A8: This was a response to an unreasonable remark by the author.

A9 / A10 / A11 / A19: Observers measure the photon flux from a GRB as a function of time and wavelength. My question is what this function would look like in your model. As an example of what this kind of observation looks like, see Fig. 1 of arXiv:1311.5867. I don't see how you can explain something this using recombination lines with different blueshifts, even in principle. What would the current instantiation of your model look like if plotted on this figure?

A12 / A13 / A14 / A15: OK, let's leave this as a research question.

A16 / A17: This misses the point. Do you expect there to be QSS regions on our past lightcone that don't intersect the surface of last scattering, or not? If there are such regions, this has obvious implications for the CMB (holes; see previously). If there aren't, why is the spatial distribution of QSS regions so special that they only appear at the surface of last scattering?

A18: As stated, I'm referring to the CMB issue here.

A20: The model has a number of issues that seriously impact its explanatory power and observational viability. Most of your arguments are just assertions that these problems either don't matter or can probably be worked out in future, without giving convincing reasons why, or for how this could be done. I've been trying to offer constructive feedback on how your arguments could be made more convincing – even suggesting specific calculations – but you haven't acted on it. The manuscript has barely changed since I first saw it.

I won't recommend an article for publication if there are serious flaws that haven't been suitably addressed, or at least acknowledged! That doesn't mean you have to outright solve all of the problems right away; it just means that you have to include a frank discussion of the issues, and give good reasons for why you think they don't rule out the model. This manuscript doesn't meet that standard. There are too many issues to ignore.

A21: Yes: there is a problem with preferred orientations in your model, but not in the usual picture of GRBs being sourced by compact objects. In the latter, there is a large population of GRB progenitors, small in size (e.g. neutron stars), scattered throughout the Universe with random orientations. Only a small fraction of these will be pointed directly towards us, the observer, and so we only see a small fraction of all of the GRB events that happen. Yet, because there is a large number of them, the rate at which we observe GRBs is still substantial. In your model, there is a much smaller number of sources. We would only observe them at a sufficient rate if they were preferentially pointed towards Earth (and not randomly oriented). This affects the argument in Section XI. Your model implies a large amount of fine-tuning of the spatial distribution and orientations of the QSS regions.

A22 / A23 / A27: My question is whether QSS regions with less extreme blueshifts would be easier to realize in nature. If so, wouldn't we see many more of these than the GRB phenomenon? (This ultimately comes down to the question of initial conditions – what formed these QSS regions and determined their properties?)

A23(ii): Given a physical model, it is reasonable to ask what its implications

might be for other phenomena besides the one you are trying to model. You can't just cherrypick which phenomena it should and shouldn't apply to without a good reason. Your model predicts UV/X-ray flashes if there are regions/lines of sight with smaller blueshifts. Why shouldn't we see them? It's the same model making this prediction, just with different parameters (e.g. the same region, but seen from a different observing angle).

A24: The current wording in your manuscript implies that this phenomenon is a general prediction of GR: "general relativity clearly predicts that some of the light generated during last scattering should reach us with strong blueshift". In fact, this is only predicted if a very particular set of initial conditions is imposed. The statement is misleading; it is actually a prediction of GR plus a specific set of initial conditions, not of GR alone.

A25 / A26: It is hard to argue these points without a plausible suggestions for how these QSS regions are formed. Perhaps one could come up with a physical mechanism that preferentially creates regions that are almost perfectly crafted to give extreme blueshifts. I don't see why or how, and without such a mechanism, people are likely to remain thoroughly unconvinced – it looks fine-tuned, and that makes it hard to believe. You could at least comment on this, although I don't expect to change your mind.

A28: Statements in scientific articles should be defensible. Your statement was not, so I was duty-bound to comment on it. See arXiv:1511.05143 for an answer to your point.

A29: I have written several papers on LTB models and well understand their advantages and disadvantages. It does a disservice to not admit that perturbed FLRW models are better at some things (and, obviously, worse at others). This is a minor point, so I'm happy to not discuss it any further.

A30: Yes, I am aware of these conditions. Perhaps I should have said that they can't easily be used to explain the hierarchy of virialized structures that host galaxies in the late Universe. It doesn't matter, this isn't an important point for your paper.