The author has provided an extensive response to my initial report, which I have studied carefully. Unfortunately, it does not satisfactorily resolve any of the main points of my previous report, and my recommendation remains to reject the paper. The author has raised several grievances about the previous report however, so I will try my best to address those below.

(1) Overview

The author's response, and this second review, have become very long and detailed. In this section, I will try to give a very high level summary of my reasons for rejecting the paper.

- I contend that the author's model would necessarily cause significant disturbances of the CMB. These would have been observed already if the model, or even a more refined version of it, is correct. They have not, so the model is effectively falsified already.

- The details of the emission process that is supposed to produce the (subsequently blueshifted) GRB photons are very vague, and it is not clear that it can produce a GRB-like spectrum even in principle.

- The author has not satisfactorily addressed other, less serious, review comments.

(2) Reiteration of the most serious issue identified in the previous report

First, allow me to reiterate what I consider to be the most serious issue with the author's model, which is point (1) of my original report.

[In my first report, I had not realized that the source photons were supposed to be the non-thermal ones (recombination lines). While it is explained in Ref. 1, this important fact was omitted from the present paper. Still, my point remains unchanged, as explained below.]

Point (1) of my report asks a simple question - if there were such large strong lenses between us and the last scattering surface, what would the CMB anisotropies look like as seen in the microwave band? These anisotropies are very well measured by experiments such as WMAP and Planck, which find the CMB to be an almost perfect blackbody, with observed monopole temperature T ˜ 2.75 K, and anisotropies on the order of 1 part in 1000 or less. The anisotropies have now been observed down to ˜ 1 arcmin angular resolution. As per the observations, any redshifting caused by localized inhomogeneities between last scattering and the observer must perturb the CMB monopole temperature by ˜ 1 mK or less on angular scales larger than ˜ 1 arcmin. Otherwise, they would contradict the observations - the CMB would be more anisotropic than observed.

Based on Fig. 4 and Tables I-IV, it seems that an observer on Earth, looking in the direction of the QSS region, should see a large anisotropy in the CMB in the microwave band. This is because most of the CMB photons coming from directions close to the QSS region (e.g. within ˜ 1 degree from its center) will be observed with substantially lower redshifts, and so higher temperatures, than the CMB monopole - that is, almost all of the rays listed in Tables I-IV have redshifts to last scattering that differ from the homogeneous value by much more than 1 part in 1000, and so would be seen as large anisotropies in CMB temperature maps. (This argument applies to CMB photons that were emitted along non-radial rays in the QSS region, regardless of what the radial rays are doing.)

I contend that any model that relies on extreme red/blueshifting events from large inhomogeneous regions will necessarily induce strong CMB anisotropies unless the regions can be made extremely small, and so is unlikely to match observations. The GRB mechanism proposed by the author necessarily relies on there being quite large inhomogeneous regions, which will necessarily cause reasonably strong redshift effects in the vicinity of the QSS region, and so will necessarily induce strong localized anisotropies in the CMB. We do not observe such anisotropies, and so we can conclude that this whole class of models is probably already ruled out by existing data. This is not my "prediction", but what I believe to be an implication of the general structure of the model (unless the QSS regions can be made orders of magnitude smaller, perhaps).

Now, the author has stated that I have misread various statements in the paper, and that my arguments are complicated in structure. In the above, I have tried to give a clear account of my reasoning, and what I believe the author's model implies. I hope the author will find this clearer, and will be able to point out if I have misunderstood certain statements in their paper, or implications of previous papers that were not reported here.

To make sure that there is a clear path forward, avoiding further misunderstandings, I also suggest performing the following simple experiment, which should not require the author to perform any more time-consuming numerical calculations. Using the same data as in (e.g.) Fig. 4 or Tables I-IV, the author can plot the mean temperature of thermal CMB photons that would be observed on Earth, as a function of (observed) angle from the center of the QSS region. If my point is correct, the plot will show a large CMB temperature anisotropy, larger than 1 part in 1000, over angles of ˜ 1 degree. Such anisotropies are not observed in reality.

(3) Response to selected points from the author

* "If my paper is not published, then the prediction of the referee [...] will fulfil itself automatically, without exposing my proposal to any kind of public scrutiny. This would be unfair."

I note that the paper is available on arXiv, and so is already completely open to public scrutiny - perhaps more so than if it were only published in a paid-subscription journal like the Physical Review. It is my responsibility to subject the paper to rigorous peer review before publication. Papers are commonly rejected if they are flawed or do not meet reasonable standards of quality or clarity. I believe that the model has sufficiently serious flaws to warrant not publishing the paper. I do not see this as being unfair.

* "The blueshifted rays [...] are not members of any specific spectrum".

This statement is confusing, as all electromagnetic radiation should have some frequency spectrum. This spectrum depends on the emission mechanism. Is the author saying the emission mechanism is arbitrary? If so, this does not seem like a justifiable assumption. Please clarify.

* "In his second paragraph the referee says that my paper appears to 'grossly contradict a number of well-established observational facts'. This is again not

true. In Ref. [1] I listed 6 directly observed properties of the GRBs and I am striving to explain all of them by consecutive improvements of the model. I do not see at which point I contradicted any fact."

Point (1) of my original review makes clear that the effect of the QSS region on the CMB is the primary concern. See above. This is not a model-dependent statement.

Point (2) questions how the GRB spectrum can be reproduced by the blueshifting mechanism, even in principle. I concede that I had not realized that the blueshifted radiation is supposed to be from recombination lines rather than blackbody radiation (this point was omitted from the paper, and is only stated in Ref. 1). Nevertheless, I was unable to find a statement of a plausible mechanism for how this could produce a broadly realistic GRB spectrum, in either this paper or Ref. 1.

[From reading Ref. 1, it appears that the author has discounted (blueshifted) thermal CMB radiation as possible source of the GRB emission because (a) GRBs do not have thermal spectra; and (b) the intensity of the blueshifted CMB is much larger than measured GRB intensities. The discussion on p7 of Ref. 1 implies that H/He emission lines are the assumed source instead. There appears to be no discussion of how these discrete emission lines could produce the continuous GRB spectra that are obtained, and how blackbody CMB photons could escape being strongly blueshifted. This seems dubious; please clarify the intended emission mechanism.]

(4) Response to numbered points

[1a] Please provide some estimate of how small the regions can be made while still preserving the strong blueshifts. Based on the current paper, it seems that only slight changes in size can be made, while several of the issues I've raised would still be present even if the regions subtended angles of only an arcminute on the sky (i.e. a factor of 60 or so smaller). Can the regions be made this small?

[1b] We are the observer, and see a last scattering surface that is unique to us. This surface is a slice through the last scattering hypersurface, i.e. the intersection of the LSH with our past null cone. My point is that, to sustain the calculation made in Section XI, the QSS regions would have to have quite a special distribution close to this last-scattering surface. If this is not the case, please clarify (a diagram of the positions of the regions on our past null cone would be particularly welcome).

[1c] "I have nowhere implied that there would be 'large holes' in the CMB."

On p15 of the paper, the author states: "It [the QSS region] should black out all CMB rays within some angle around the central gamma ray, and the blackout should continue even after the gamma-ray flash is over. The redshift distribution across the image of the source would have no reason to continuously match the CMB at the edge."

This sounds very much like a hole, or at least a large anisotropy, to me. See my point (1) above for why this is troubling observationally. Phrased another way: If I observe a patch of the CMB at ~ 200 GHz with a QSS region is in front of it, will I see a T ~ 2.75 K blackbody in that direction, or won't I? It seems pretty clear from the paper and the author’s response that I will see a hole or some other strong disturbance.

Also, the author may be assuming that the regions should be ˜ sub-degree scales, to match the resolution of current GRB detectors. As explained above, ˜ arcminute scales are probably the maximum allowed by CMB observations.

[2a] I have discussed the issue of the frequency spectrum above. What is the source of the photons that travel along the preferred axis, and what is their initial frequency spectrum? Ideally, the author would provide a clear description of this mechanism, and how the photons can be converted into the continuous GRB spectra that we observe. The spectral behavior is an important aspect of any GRB model.

[2b] ”The referee essentially demands that I solve all possible problems of my model in a single paper. This is both unrealistic (the paper would be very long, and it would take a very long time to prepare it) and unfair (does one require the same from all other papers that introduce new ideas?).”

I am only asking for broad plausibility arguments, not a fully worked out model. These could be at the level of ”back of the envelope” calculations, where things are worked out using approximations etc. and need only be accurate to an order of magnitude or so. These are perfectly reasonable things to ask for when evaluating any model. It’s also reasonable to hope that one or two aspects of a model that don’t work can be fixed in future. This model seems to fail in several important ways however, with little or no discussion of how these issues might be addressed. Please provide reasonable plausibility arguments that address my points.

[3a] Most GRB models assume that there is a large population of GRB progenitors distributed throughout the Universe, with some small subset being randomly aligned with their preferred/collimation axis pointing towards Earth. These sources subtend a very small angle on the sky, and are transient, so it is perfectly possible for multiple GRBs to be observed coming from the same direction over the course of many years. I do not see why this is a problem - the same is true of pulsars, active galactic nuclei, etc. etc. The existence of a large multitude of unobserved GRB progenitors seems perfectly reasonable.

[3c-d] I am happy to accept these points. It is not clear to me that the QSS regions being constructed by the author will necessarily evolve into realistic voids at late times, but this is a minor point.

[3e-f] The author is specifically constructing the QSS models to produce extreme blueshifts. My question is whether one should also expect regions with lower maximum blueshifts to be realized in this scenario. My suspicion is that regions with less extreme blueshifts are easier to construct, and so might be more likely to be realized in nature. If this is that case, shouldn’t we see this phenomenon in the optical/UV too?

If so, then the author should consider whether the model is consistent with existing observations in those bands. If not, then there must be some underlying assumption about the distribution of properties of the QSS regions that could be realized in nature, and the range of blueshifts that they should be able to produce. My suspicion is that this distribution would have to be quite fine-tuned to only produce GRB-like phenomena, but not significantly larger numbers of optical/X-ray phenomena.

[4a] The author hasn't really engaged with my point here. QSS regions causing strong blueshifts are not necessarily realized in nature, even if they are allowed by GR. Furthermore, the test of the redshift pattern on the CMB is precisely what Point (1) of my original review was about.

[4b] See [3e], above. The QSS regions need to have specific properties to produce sufficiently extreme blueshifts, and these properties are not necessarily representative of the general class of QSS models. What causes only those QSS regions with appropriate properties to be realized in nature? Or, alternatively, why are the implications of models with less extreme blueshifts not discussed?

[4c] This statement is untrue. Models of inflation are reasonably well supported by observations, and provide a plausible mechanism for driving the Universe from an almost arbitrarily inhomogeneous initial state to a smoother almost-homogeneous and isotropic state. Whether they offer a correct description of the early Universe remains to be seen, but it is disingenuous to imply that they are not scientifically legitimate.

[4d] Exact LTB and QSS models are quite restrictive compared to perturbed FLRW. First, they cannot model arbitrary distributions of matter as seen in (e.g.) the galaxy distribution - only spherically symmetric, offset shells, or similarly structured matter distributions can be modelled. Second, they only include dust, and so cannot describe the Universe arbitrarily far into the future, as they suffer from unphysical shell crossings (that would otherwise be prevented by e.g. virialization and pressure from baryonic matter). I am puzzled by the final comment - I am neither an astronomer, nor violently objecting to the use of inhomogeneous models. This point was only included to flag up an incorrect statement in the paper.

[4e] The only necessary conditions for accepting the paper are that the concerns that I have highlighted are satisfactorily addressed, and that the paper is brought up to a reasonable, scientifically-defensible standard. Please address the review comments.

In future, please also refrain from using gender-specific pronouns when responding to anonymous referees.