This is my reply to the second report of referee A.

Referee A again wiggled himself out of the duty to read my paper. He used an irrelevant anecdote to dismiss one sentence from my abstract, which he found to be a "logical fallacy" justifying the rejection of the whole paper. He implies that investigation of certain phenomena should never be undertaken because he finds them not sufficiently probable. This is not a fact-based criticism, but subjective censorship. In serious science there is no place for a priori dismissals based on subjective judgements. Every hypothesis should be open for testing, and none should be rejected a priori as improbable. By testing I mean serious investigation of its consequences, and not instantly shooting it down by shallow wisecracks.

The other statements of referee A are either erroneous, or falsely represent what I did, or are irrelevant for evaluating the contents of my paper. In the following, I mark the quotations from his second report by [Q], and my replies by [R]; they are ordered by the order of appearance in the report. I wish to stress that he is wrong in all of his negative comments.

[Q1] "the author constructs an example of GRBs mechanism trying to show that it cannot be disproved"

[R1] This is a rhetoric distortion of my intention. My real motivation is this: Relativity is a well-tested theory, so whatever it predicts should be taken seriously and thoroughly investigated. Through the Lemaitre - Tolman and Szekeres models, relativity predicts that some rays emitted at early cosmological times will be reaching later observers with blueshift instead of redshift. So, it must be investigated whether blueshifted light can be found among the observed phenomena. In search for it, my first suspicion fell on long gamma-ray bursts because it is by now generally accepted that most of their sources lie several billion years to the past from now - about halfway to the Big Bang. My hypothesis is that they are emitted at last scattering - still closer to the BB. Technically, the question is whether any rays emitted at last scattering can be blueshifted to the gamma frequencies and acquire the characteristics of the GRBs that I listed in Paper [1], while obeying the constraints imposed by other observations (observations of CMB among them).

The present paper has a meaning for relativity theory, independent of its connection with the GRBs. Its main implication is: If strongly blueshifted rays are emitted from the last-scattering hypersurface in a QSS region, then a present observer may see them in the gamma-range only for a brief time interval, of the order of 10 minutes. The reason for this brief duration is the direction-drift in other inhomogeneous regions between the source of the rays and the observer. In the paper, I avoided making direct connections to the GRBs as much as possible, in order not to offend astrophysical fundamentalists. (I have now counted the appearances of the term "GRB" in the paper - there are 6 altogether including two in the abstract, the word "gamma" in connection to the observed GRBs is used only within these 6 instances. I could not remove all references to the GRBs without hiding my initial motive.)

[Q2] "...we do not have any evidence that the dust QSS model describes nonuniformities in the Big Bang"

[R2] This has nothing to do with my paper. What would be objectionable would be extending a dust model down to the Big Bang to describe our real Universe - but I did not do that. All my considerations are applied to the epoch after the last scattering, where the dust approximation is acceptable. I stated this explicitly, and it is also seen from all the numerical calculations - the referee would know it if he had read the paper. The function $t_B(r)$ *would be* the BB time if a Szekeres model were extended back to the initial singularity, so for brevity it is called the bang-time function in all other situations. But in a realistic model this is just one of the parameters describing the inhomogeneity, and not the actual Big Bang time. For the epoch before last scattering, another model should be used - but this is a different research topic which I did not enter in my paper.

[Q3] "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."

[R3] I did not do this. As I said in [R2], I applied the Szekeres models only to the post-last-scattering era. The referee either deliberately distorts the truth here, or does not know what my paper contains.

[Q4] "We are not told why there should be only one additional QSS region"

[R4] In the paper I considered one such region in order to understand what happens to a light ray that goes through it. In reality, more such regions should exist. Any additional QSS region would cause additional redirecting of the light rays on top of the one I discussed. As I showed, such redirecting even in one intermediate region is sufficient to make a high-frequency radiation stream disappear from observer's view within a period of the order of 10 minutes. Nothing in my paper depends on the absence of other QSS regions. See [R12] below for more on this.

For referee B even one additional Szekeres region is too many; he says: "requiring the light ray to go through two such regions seems like implausible extra tuning." How can one satisfy both referees when they contradict each other? And what is the measure of "plausibility"?

[Q5] "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"

[R5] I did the computation on the basis of the exact Einstein theory, but of course one can repeat it in linearized theory and compare the results. But this is irrelevant for my paper - unless the referee is proposing a new research principle: If something can be done approximately, then doing it exactly is prohibited.

[Q6] "The author study only geodesic equation and neglects the effect of gravitational lensing which very likely cannot be neglected in his scenario."

[R6] This is a two-tiered nonsense. (1) There is no other way to study gravitational lensing than calculating null geodesics (i.e. paths of light rays). So, by studying "only geodesic equation" (actually, equations) lensing is not neglected. (2) Lensing occurs when light rays are deflected by an object between the light source and the observer. The deflection angles and their changes with cosmic time are exactly what I calculated. This IS lensing.

[Q7] "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."

[R7] Like in the first report, referee A again counts what can be mechanically counted instead of seriously analysing the contents of the paper. Moreover, this comment contradicts the previous report. In the previous one, the referee was in despair over the number of repeated equations and presented this as a fundamental flaw of my paper (literally, he said "repetition of material goes beyond my imagination"). Now he says "I know why the text has been repeated and that the paper contains new results". So was the repetition justified or not? See below for comment [B2] of referee B - it illustrates what can happen when quotations from earlier papers are omitted to avoid repetitions. I stress again that the repeated equations are contained only in the introductory part of the paper (3 pages out of 17). I did not claim them to be among the new results - I said in the introductory section "Sections II - IV are partly repeated after Ref. [3]". Implying, like referee A did in the first report, that I tried to inflate the contents of my paper by repetitions is a bad-willed action.

[Q8] "I did not use an electronic text comparator."

[R8] This is just irrelevant. See [R7] above for the relevant part.

[Q9] "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]."

[R9] The quoted statement was within the list of "copy and paste" examples ["the big part of the paper (including figures) is a 'copy and paste' from the previous author's article" the referee said], which means that he counted Fig. 2 as one of the repetitions, so he contradicts himself once more. My paper contains 23 figures altogether, so reprimanding me for copying one of them (in the introductory part) from a previous paper is petty malice.

[Q10] "It does not follow from the results presented in the paper if the model will work for a different density profile."

[R10] The only thing that can be different with a different profile is the rate of direction-drift of the light rays. The drift is present in *all* Szekeres regions for all generic rays, see Ref. [10] in the paper. (Non-generic rays are those that go along preferred directions, for example the symmetry axis if one exists. In a fully nonsymmetric Szekeres region no such directions exist.) It is rather obvious that many (probably all) other profiles would be also sufficiently good - see Ref. [10] again for explicit examples of the drift in LT regions. How many examples are needed to convince the referee that the direction drift shortens the duration of the observed gamma ray impulse?

[Q11] "For sure it cannot work if the QSS2 inhomogeneities will mild or very strong."

[R11] "For sure" is an empty rhetorical figure. With a stronger inhomogeneity the rate of direction drift will be still greater, thus doing the job even more efficiently than in my paper. With a less pronounced inhomogeneity in QSS2, the rays will drift more slowly - but where is the problem? The real GRBs do have different durations. My aim here was to prove that direction drift shortens the gamma-ray observation period compared to the (unrealistic!) situation when the blueshifted ray proceeds undisturbed all the way from the source to the observer.

It is interesting to compare [Q11] with a related comment of referee B. In [Q11] referee A implies that my QSS2 inhomogeneity is not "very strong" (a "very strong" one, he says, cannot work). Referee B says "The explanation requires the blueshifted ray to traverse two extremely inhomogeneous regions, modelled using the Szekeres metric." So, the same QSS2 region is not a "very strong" inhomogeneity for referee A, and is "extremely inhomogeneous" for referee B. Neither of the referees defined what "very strong inhomogeneity" or being "extremely inhomogeneous" means. Neither of them proposed a measure of strength of the inhomogeneity, nor quoted any such measure from another source. They are just posturing: both want to be seen as insiders to the subject and try to increase their credibility by using strong, exaggerated language while neglecting to assign precise meanings to the terms they use.

[Q12] "and for sure it must work differently, if more similar inhomogeneous regions will be added"

[R12] Again, the "for sure" is an empty claim. With more intervening Szekeres regions the ray will be redirected more times along the way. In typical cases this will magnify the effect (the direction drift at the observer will become faster and the highest-frequency ray will disappear from observer's view even sooner) - unless the drift in the (n + 1)st region cancels the one acquired in the n-th region. Now, this would be a (super)fine-tuning! Not only the density profiles, but also the directions of the mass-dipoles along the ray would have to be connected by unique equations.

[Q13] "If fine-tuning is not hidden in the QSS2 density profile than it is hidden in the number of QSS regions."

[R13] This is one more empty claim. The first configuration I tried did the job, and of course more must exist. I repeat from [R10]: How many examples would be enough? By the way, the construction "if not ... then" suggests that the referee had the following in mind: "Here or there, the fine-tuning must be hidden somewhere, or else the author would be right, what is impossible."

[Q14] "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.'"

[R14] This is only the main result of the paper. There are several more, about which the referee has nothing to say.

Summarizing, the objections of referee A are all incorrect - based on false assertions about my paper. They follow either from his lack of understanding of relativity theory (Q2, Q6, Q10, Q11, Q12, Q13) or from his unfamiliarity with my paper (Q2 again, Q3, Q4, Q14), and one (Q1) is a rhetorical distortion of my intention. The remaining comments are irrelevant (Q5, Q7, Q8, Q9 and the long anecdote at the beginning). This tenacity in trying to discredit my paper points to hidden negative motives, so the report lacks objectivity.

Three comments of referee B must be answered separately:

[B1] "The explanation requires the blueshifted ray to traverse two extremely inhomogeneous regions, modelled using the Szekeres metric."

This is not a correct description of my paper. The ray does not "traverse" two inhomogeneous regions: it is *emitted* in the first of them and goes through the other. Re "extremely inhomogeneous" see [R11] above.

[B2] "There is no reason presented for the origin of such regions and there is no observational evidence for them."

The reasons are presented in my three previous papers, in particular in Ref. [1], and are briefly recapitulated in the introductory section. The observational evidence of them are the gamma-ray bursts.

[B3] "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.)"

In the paper, the light ray traverses the inhomogeneities at the early stage of their existence - soon after the last scattering. They might evolve into black holes later, which I did not verify. Even so, the "not strong enough" statement is not based on any calculation. Even mild inhomogeneities (like galaxy clusters) can act as gravitational lenses. It is lensing that shortens the duration of the observed high-frequency impulses.

In summary, the report of referee B is all beside the point - it fails to evaluate my paper for what was really done in it.