Dear Editor. You said that the current referee is a relativist, but he evaluated my paper so extremely one-sidedly from the point of view of modelling the gamma-ray bursts that I would count his report as a voice coming from the astrophysics community. Moreover, his final remark is a false statement about the contents of my paper (see my reply), which suggests that he did not really read it. There are in fact more pieces of the report where he misread and misrepresented my paper, see (R5) in my replies to him. So I think it would be fair to employ as the second referee a genuine relativist.

You expect a revision that meets the criticisms made, but the report does not call for any revisions: it is a total denigration. All I can do is to provide counterarguments and hope that the referee will change his mind.

Finally, I find it not right that in negating my results the referee argues by poetic hyperbolae that are not backed by any calculations. Examples: "strong deviations from a homogeneous and isotropic model" (what measure of "strength of deviation" does the referee use?), "energy output ... is enormous" (exactly how much is "enormous"?), "much farther away" (it is actually twice as far), "gigantic focussing effect" (again, exactly how much is "gigantic"?)

Reply to referee #1:

The letter from the editor says that the referee is "advising that you revise your manuscript". However, the report says that my paper is wrong or worthless in all its parts and contains only justifications for rejection. So all I can do is try to convince the referee that my paper is worth presenting to the readers in spite of all the shortcomings of the model presented in it.

The motivation for the work presented in my three previous papers and in the now-submitted one is this: the Lemaitre-Tolman (LT) and Szekeres (Sz) models, unlike Friedmann (F) models, predict that some of the light rays emitted shortly after the Big Bang (BB) *may* be reaching present observers with a blueshift instead of redshift. The LT and Sz models contain F as a limiting case, so are its exact perturbations, and for this reason were investigated in a large number of papers to explain various observed properties of the real Universe. If these models are to be taken seriously, it must be explained where (and if) the blueshifted rays might be found among the current observations. In the first attempt, I tried to relate them to the GRBs because the sources of the GRBs are believed to lie at large distances from us (several billion years to the past). My model puts them approximately twice as far. I hoped this would be less controversial than relating the blueshifted rays to lower-frequency radiation such as X-rays because sources of the latter are believed to be much better understood and not so distant. Still, all of my papers on this subject were concerned more with optics in the LT and Sz models than with explaining the observed GRBs. Before a serious astrophysical application of my models is considered, we must make sure that we understand their geometry and the physics implied by it.

Here are my replies to the specific points made by the referee. Quotations from the report are marked by (Q1), ..., (Q12), my corresponding replies are marked by (R1), ..., (R12).

(Q1) "Even apart from the connection to gamma-ray bursts I don't think that the cosmological model is viable: It is a dust model (no dark energy) and it assumes that there are strong deviations from a homogeneous and isotropic model."

(R1) Here the referee treats dark energy as a necessary component of every cosmological model, and implies that a model that does not include DE is to be immediately rejected. I do not wish to extend the discussion in that direction, but I remark that dark energy (in the form of the cosmological constant of appropriate value) can be included in my consideration. Generalizations of the LT and Sz models to nonzero Λ are known and were discussed in several papers. The reason I preferred to assume $\Lambda = 0$ is that then a part of the calculation can be done analytically, which is more reliable, while with $\Lambda \neq 0$ I would have to use numerical calculations from the very beginning, thus adding numerical uncertainties on top of conceptual and analytical difficulties. Thanks to my papers, someone willing to consider $\Lambda \neq 0$ (perhaps myself) will have an easier task. Re "strong deviations" see (R3) below.

(Q2) "It is already very difficult, although maybe not quite impossible, to explain the apparent accelerated expansion of the universe by inhomogeneities, rather than by assuming the existence of dark energy."

(R2) It looks as if the referee adheres to the philosophy that investigating models that do not agree with the current astrophysical paradigm should be prohibited. That would have led to prohibiting research in general relativity for most of the 100 years of its existence (and Peter Szekeres would not have even tried to find the models now named after him because in the 1970s everybody knew that the Universe is perfectly isotropic and homogeneous). What if the paradigm changes? Should all non-mainstream research wait in suspension until then?

(Q3) "I cannot believe that the model suggested here could be in agreement with the observed isotropy of the cosmic background radiation."

(R3) This point was explained in my paper [1]. The *observed* GRBs DO perturb the isotropy of the CMB: for the time when a gamma-flash is visible it blacks out the CMB rays within the image of the gamma-ray source. My model does just the same. However, in the present form my model implies a larger angular diameter of the radiation sources in the sky than is observed in the GRBs (nearly 2 degrees in the model vs. 1 degree resolution of the GRB detectors). So the model needs to be improved, and possible ways to improve it were pointed out in each of my papers. I intend to work toward decreasing the angular size of the blueshifted sources. I do have an idea how to begin - but see (R4) below for the explanation of difficulties. This is not a question of believing, but of calculating.

(Q4) "Studying Szekeres models, and other cosmological models with little or no symmetries, certainly has its merits: However, I firmly believe that the deviations from homogeneity and isotropy cannot be so big as assumed in this paper."

(R4) As explained in (R3), these deviations are not really big and can possibly be made still smaller if the model is improved. This will hopefully happen, but I wish to stress that each step of improvement requires a lot of conceptual and numerical work. The work involves guessing a modification of the shape of the BB hump and testing the consequences of each change by a long-lasting numerical run. Besides, what does "so big" mean quantitatively? And, referring to (R2) again, if the deviations are bigger than allowed by current observations, should it be prohibited to interpret such geometries?

(Q5) "I cannot see any reason why transitions in the electron shells of neutral hydrogen or helium atoms near the hypersurface of last scattering could be associated with such an enormous energy output. One would have to assume that the spacetime geometry produces not only a blue-shift in certain directions but also a gigantic focussing effect. I cannot see that the model suggested here does anything of the kind."

(R5) My papers do contain answers to these questions - see points I - III below. I emphasize that those answers are all given in one or another of my earlier papers - I have not invented them now. Plus, both the blueshift and the "gigantic focussing" result from calculations of null geodesics, they are not assumptions. The primary reason of their presence is non-constancy of the function $t_B(r)$.

- (I) The process that gives rise to the blueshifted rays is not "transitions in the electron shells of neutral hydrogen or helium atoms" but *recombination* - capturing electrons by hydrogen and helium nuclei at the end of the recombination epoch. Each of the rays calculated in my papers goes back to the last-scattering hypersurface, and the blueshift cumulates along its final segment up to the point of intersection with the LSH. (It would continue to cumulate if the ray were traced further back in time, but I consistently adopted the assumption that before last scattering a dust model is unrealistic, so there is no point in using a Szekeres model there.)

- (II) The blueshifted radiation is generated *during* last scattering, not "near the hypersurface of last scattering". It is *the same radiation* that becomes CMB in generic directions. The rays emitted in preferred directions of the Sz regions get blueshifted instead of redshifted, and all "energy output" comes from the Big Bang via recombination, just as for the CMB. The blueshifting arises because in an inhomogeneous spacetime the expansion velocities of constant-mass shells are uncorrelated with their radii. When $t_B(r)$ is not constant, the geometric distance between the surface of the smaller shell and the surface of the larger shell increases, for a limited period, slower than the radius of the larger shell, which looks as if the region between the shells were contracting relative to the cosmic background. So the energy gain connected with blueshifting is at the cost of the (non-uniform) cosmic expansion. I emphasize again that what I say here is an explanation in words of a (numerical) calculation, and not the basic reasoning used in the paper.

  Assuming that this mechanism works in the actual Universe (that our Universe is like a Sz spacetime with non-constant t_B rather than F), the observed characteristics of any given GRB could be used to infer about the geometry of the QSS region that emitted it. But first the models with nonconstant t_B have to be understood at sufficient detail. This is what my papers aim at.

- (III) Re focussing: As was shown in my papers [2,3], in an axially symmetric QSS model strong blueshifting may occur only along an axial direction. So, the maximally blueshifted rays stay "focussed" to a line (in spacetime - to a 2-dimensional sheet) all the way. Similar strong blueshifting accompanied by "focussing to a line" was demonstrated numerically in exemplary fully nonsymmetric QSS models in my Ref. [2].

(Q6) "It is my impression that the author suggests that ALL gamma-ray bursts, long and short, are of a cosmological origin."

(R6) For my purpose, described above (Q1), it would be sufficient if *some* of the observed GRBs, either long or short or both, are of cosmological origin. It would be a task for the observers to identify which ones, if any, really are. The observations may exclude this possibility altogether, but the observers will never face this task if my papers are censored out already at the refereeing stage.

(Q7) "Since the late 90s we have instruments that are sensitive enough so that we observe an afterglow for almost all long gamma-ray bursts. For many of them we have (spectroscopic, not just photometric) redshift measurements. The observed redshifts are all positive, with a maximum between 2 and 3, whereas in the model suggested in this paper they should be negative. I emphasize that in many cases the redshift is measured by indentifying absorption lines of heavy elements. How could these lines be explained in the model suggested here? We would have to assume that, by pure coincidence all these observed gamma-ray signals have been travelling through a cloud of matter that is totally unrelated to the place where they have come into existence. This is against all probability. Moreover, almost all long gamma-ray bursts could be associated with a galaxy and many of them were accompanied by a supernova. Again, in the model suggested here all this would have to be just a series of coincidences. Of course, the probability for such coincidences is absurdly low."

(R7) The referee says "for almost all" and "many of them", implying that what we know about selected GRBs applies to all of them. But actually, "almost all" and "many of" means *not for all*. I refer here to my (R6) - the cosmological GRBs may hide in the gap between "almost all" and "all". The point made here by the referee is a crucial test of any model, but the test should be applied with due care, and not by blind extension of what is known. If all (really *all*) observed GRBs are proved to be inconsistent with my type of models, then the implication will be: if blueshifted rays from last scattering exist at all in the present Universe, then they are not among the GRBs. The next thing to do will then be to verify whether they can be found among the X- or UV rays. If those are eliminated as well, it will mean that only those LT and Sz models are realistic in which the function $t_B(r)$ is constant (only then blueshifts are not generated). This would be a strong result. But we will not come to this conclusion until all consequences of nonconstant t_B are investigated - and the referee aims at derailing this activity.

(Q8) "For short gamma-ray bursts we do not usually observe an afterglow and we do not usually have spectroscopic redshift data. However, also in this case we have the above-mentioned problem of the observed energy fluxes."

(R8) See (R5).

(Q9) "That's why I come to the conclusion that the idea of this paper is not tenable, for NO type of gamma-ray bursts."

(R9) So far I think I disproved all the arguments in favour of (Q9). And I remind that my papers are concerned primarily with the geometry and optics of the Sz models, not just with modelling the GRBs.

(Q10) "(I mention in passing that there is no need for a new interpretation of short gamma-ray bursts: The kilo-nova model, which associates them with merging neutron stars, got wonderful support by the gravitational wave signal GW170817 and its counterparts.)"

(R10) Like in (Q7), the referee implies here that what we know about a selected short GRB (just one in this case) applies to all of them. How can we be sure about it?

(Q11) "I have thought for a while if the calculations on light propagation in QSS models could justify a publication in GRG, without the connection to gamma-ray bursts."

(R11) My papers [2] and [3] were accepted for publication under the condition that I remove references to real GRBs as far as possible (I could not remove all because paper [1] had already existed in print). I did. This time, I tried to do this before submitting the paper - and still the connection looks too explicit for the referee. I think I can obscure this connection still further, but I would appreciate hints on which paragraphs or sentences or words in the paper should go away in order to make it less outrageous.

(Q12) "However, I don't think that, as far as these aspects are concerned, the results go sufficiently far beyond the work in the author's earlier papers. The new aspect is a certain combination of QSS regions which is motivated only by the desired relation to gamma-ray bursts and hardly of any interest by itself."

(R12) This is simply not true. The previous papers seemed to imply that once an observer sees a stream of gamma rays coming from a certain direction, she would continue to see it for hundreds of thousands of years, with the frequency of radiation going down slowly (the speed of frequency change being related to the expansion rate of the Universe). In the present paper I have demonstrated that there exists a mechanism that shortens this visibility period to less than 10 minutes. This feature of the QSS geometry was not considered in previous papers, is not even implicitly contained in their results, and is independent of the relation of the model to observed GRBs. The reason of the radical reduction of the visibility period is the angular drift (time-dependence of the deflection angle) of the rays in the second QSS region. The drift occurs because the rays traverse the second QSS region non-axially. The drift phenomenon was so far explicitly calculated only in LT models (in my Refs. [10] and [11]), my present paper does this in a Sz model for the first time. This calculation required a large amount of numerical work, and along the way I found out (numerically) a new fact: the drift vanishes not only for axial directions, but also for rays that enter the QSS2 region orthogonally to the symmetry axis, even though the deflection angle is then nonzero. In view of what I said in this paragraph, the "hardly of any interest" verdict is unjustified and unfair.