In the second report the referee ignored the most important explanation of my previous reply: "what I propose in my paper is not a fully worked out single model of a GRB source, as the referee seems to assume, but a third step in a sequence of refinements of the model that I proposed in my Ref. [1]". This is not the last step - I am aware, and I stated it in the paper, that further improvements are needed. The improvement that is most needed is finding a way to decrease the angular diameters of my model-GRB-sources below their current minimum while preserving the value of the blueshift they generate. Since the ultimate improvement promises to be difficult (see below), in the present paper I concentrated on showing that a quasi-spherical Szekeres (QSS) deformation superposed on a Lemaitre-Tolman (LT) background works in the desired direction - the angular diameter did become smaller, although not yet sufficiently.

The referee still treats the present model as the ultimate one and keeps repeating the adjective "large" (large QSS regions, large holes in the CMB distribution, etc. - 6 times in the whole report), which has no precise meaning, but is meant to imply that my whole class of models has already failed all possible tests. This is my request to the referee: please take into account what I had said, in particular please do not ask questions that had already been answered (example: [Q26] - [A26] below).

A few deficiencies of the current model do need to be worked on, but the referee claims that he/she knows already now that the work will result in nothing. It is not the task of referees to thwart further research in this way.

Here are my answers to the points raised in the referee's second report. Quotations from the report begin with [Q], my answers begin with [A]. Asterisks are used for emphasis. In the quotations I have omitted the parts that I believe do not need answers; they are marked with ....

[Q1] "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."

[A1] The first sentence of my paper says "This paper is a continuation of Refs. [1] and [2]", then brief abstracts of the two previous papers follow. I thought this would be a sufficient introduction, but if the referee thinks otherwise, I may easily add some extra information. I do not think this omission could be a justification for rejecting my paper.

[Q2] "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."

[A2] (In the penultimate sentence above I suppose "larger than 1 arcmin" should actually read "smaller than 1 arcmin".) Lensing and blueshifting are two different phenomena. The essence of lensing is deflecting rays, and in most cases this occurs without any blueshifting. Conversely, the essence of blueshifting is increasing the frequency of emitted radiation (it is the inverse of redshifting), and the maximally blueshifted rays do not undergo any deflection. The terms "blueshift" and "blueshifting" were not invented by me, but have been in use for more than 30 years now. Moreover, in my model the QSS regions that generate the blueshifts are not "between us and the last scattering surface" - the emission points of the blueshifted rays are in the part of the LSS that lies *within* a QSS region.

[Q3, continuation of Q2] "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.)"

[A3] I agree that the angular size of my model-GRB-source is now too large and needs to be made smaller. But if the GRB sources lie at the last scattering surface (LSS), as my model assumes, then the disturbance in the CMB temperature should be present only as long as the QSS region remains in view of the observer. In my Ref. [1] I tried to explain the brief duration of the GRBs by the flow of observing time - after a certain time-interval the observer would be registering rays that have bypassed the main body of the extremum redshift hypersurface. This implied too long a duration of the GRBs and their afterglows. At present, I have nearly finished the next paper in which I show that the short duration of a GRB may be explained by the blueshifted ray being deflected by another QSS region between the GRB source and the observer. The angle of deflection is time-dependent in consequence of the cosmic drift (Refs. [13] and [14] in the paper). Consequently, any blueshifted ray would remain visible for the observer only for a brief period. This is supposed to be a proof of existence of the effect, not yet a model of any concrete GRB. The durations of the model-afterglows continue to be too long, and I intend to look into possible solutions of this problem in a future paper.

[A3, part 2] But the GRBs do exist - and something must happen to those CMB rays that are nearly collinear with the central gamma ray while a GRB is in view of the (real, present, earthly) observer. Do they display the pattern of redshift dependence on angle that my model predicts? Answering this question is not possible with the current resolution of the GRB detectors (1 degree angular diameter), but I assume it will become possible in the future.

[Q4] "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."

[A4] The crucial part of this statement is "unless the regions can be made extremely small" (I would replace the emotionally loaded "extremely" with "sufficiently"). Yes, the regions do need to be made smaller, and I do not know yet how to make them *sufficiently* small. But I intend to keep working on it, and if my present paper is published, other authors will have a chance to join me in this quest. It promises to be very difficult - see below for more on this problem. I do not know where the referee's certainty comes from where he/she says that the result ”is unlikely to match observations”, with no research done yet.

[Q5] ”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.”

[A5] See [A3, part 2] for the answer. I know of no attempt to observe CMB anisotropies in a vicinity of the gamma ray while the GR flash is on. See also [A4] re ”quite large”. And why should a guess (”probably”) be sufficient to reject my paper without any solid research?

[Q6] ”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).”

[A6] The difference between ”predicting” and ”believing” is subtle. And ”orders of magnitude” is an exaggeration. At present, the angular diameter of my QSS region (2 degrees) is just two times the current resolution of the GRB detectors (1 degree). I anxiously await the moment when the observers will be able to resolve the 1-degree disk and tell us what happens within it, i.e. how small the GRB disk in the sky really is. But even going down from 2 degrees to 1 arc minute, as the referee says would be necessary, is just two orders of magnitude. See below for more on this.

......

[Q7] ”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.”

[A7] This temperature anisotropy is seen from the tables, and a plot would be superfluous. The problem is not in the presence of the anisotropy, but in the angular diameter of the perturbed region. On this, I have already commented above. And, since the report repeats itself, I have to repeat my answer: the anisotropies, on a suitably smaller angular scale, are to be expected only during the brief period when a GRB is in view of the observer.

......

[Q8] ”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.”

[A8] This remark is cynical. If this were true, peer reviewed journals would not need to exist. A paper published in the arXiv is noticed only by a handful of readers, and is quickly forgotten by all except perhaps the few who are directly interested in the subject; unless somebody happens to cite it. If not followed by a proper publication, the paper will be treated less seriously, possibly with suspicion.

....

[Q9] "* "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."

[A9] The energy of the gamma radiation in my model depends, via the blueshift, on the point on the LSS where the blueshifted ray was emitted. The time-dependence of this energy is dictated by the profile of the hump on the Big Bang set, which is adjustable - in the LT and QSS models the BB function $t_B(r)$ is arbitrary and should be adapted to data from observations. So, if my model is to be applied to any specific observed GRB, the shape of the BB hump should be suitably chosen. But I note that in the literature on the GRBs (see, for example, Refs. [1] and [2] below), the term "spectrum" is used with a different meaning: it is the number of separate bursts observed per given energy interval. This is easier to model, as each GRB would be coming from a different BB hump, so it is enough to adjust the height and width of each hump.

....

[Q10] "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."

[A10] See [A9] and [A11] below.

....

[Q11] "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."

[A11] The discrete emission lines are blueshifted to different wavelengths, depending on the position of the emission point on the LSS. They may also be spread when the ray, on its long journey through the Universe, passes through various objects. Indeed, I have not discussed this problem. I believe such a discussion would be premature at the present stage of development of the model. (BTW, looking at what is called "spectra" in Refs. [1] and [2] below, they are far from continuous.)

[A11, part 2: re CMB photons] A necessary condition for a strong blueshift is that the ray was emitted radially (in an LT model) or along the preferred direction (in a QSS model). For LT, this is an immediate conclusion from the equations of null geodesics (Sec. V in paper [1]). Another necessary condition for strong blueshifting is that the ray is emitted at such a point of the BB, where the function $t_B(r)$ has nonzero derivative. Both these conditions must be fulfilled simultaneously. A great majority of rays fail to obey one or both of these conditions, and they evolve into the CMB. The CMB thus consists of those photons that were emitted in non-preferred directions or in the unperturbed Friedmann region where t_B is constant, and this is how they avoid being blueshifted. I see that the referee treats all my statements with utmost suspicion, so I suppose I have to add the following: what I said in this paragraph is not my invention in response to the current report, but the most basic piece of knowledge about blueshifts. For the LT models this knowledge begins with Refs. [3] and [4] below; Ref. [2] of the paper extended it to QSS models.

Perhaps the following technical remark has to be added here (which in fact is contained in my paper, but maybe not explicitly enough): the equations of light rays in the paper are integrated backward in time, from the present instant. Those rays that aim at the special directions in a QSS region, and at points where t_B is not constant, *would* have infinite blueshift if they were followed to the intersection with the BB. But the QSS models, where the source in Einstein's equations is dust, cannot be applied to arbitrarily early epochs in the real Universe because at a certain time the assumption of zero pressure becomes unrealistic. So, the integration of the rays is terminated when the mass density along them becomes equal to the recombination density calculated from the ΛCDM model (i.e. at the LSS). Still, the rays somehow "feel" that, if they were extended further back, they would hit a special point of BB and along a special direction, and begin to build up strong blueshift before reaching the LSS.

....

[Q12] "Please provide some estimate of how small the regions can be made while still preserving the strong blueshifts."

[A12] What is the minimal size of a QSS region that would produce sufficient blueshift is a difficult question that I have left for the future. As I explained in Sec. XII, I achieved the current blueshift range by guessing the shapes of the BB humps. (The hump must be such that the blueshifted ray remains below the extremum redshift surface as long as possible after emerging from the LSS.) The progress achieved between the first attempt and the present paper was quite significant (see first paragraph of Sec. XII), but it is improbable that I could have arrived at the very optimal shape in this way. Further experiments with the shape will need to be computerized. They will have to include shapes of other classes than those investigated so far. I cannot make any estimate based on the data I have now. If it is proven that one cannot go below the current 2 degree angular diameter, then this would rule out my models.

[Q13] "Based on the current paper, it seems that only slight changes in size can be made,"

[A13] This statement is not correct. The correct summary of my results is: *so far* I was able to make only small changes. How large are the changes that ultimately *can* be made is a separate problem - see [A12].

[Q14] "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)."

[A14] This claim is unjustified. See above. Moreover, a factor of 60 is not "orders of magnitude" - see [Q6].

[Q15] "Can the regions be made this small?"

[A15] See [A12]. If I knew how to make them this small, I would have done it already.

[Q16] "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)."

[A16] The aim of the calculation in Section XI was to estimate the upper limit on the number of GRB sources of a given angular diameter that could be fitted in the observer's sky. For this purpose, they were imagined to be placed as close together as possible without overlapping, and as uniformly in the whole sky as possible. The estimate is obtained by dividing the whole area of a unit sphere by the area of a curvilinear rectangle surrounding each circle of a given angular diameter (see paper). This is where the number in eq. (11.2) comes from. I was curious how many QSS regions of 2-degree diameter could be placed side by side in the sky, how many 1-degree observed GRB spots would fill the whole sky, and how different the two numbers are. The consideration in Sec. XI is thus not intended to model the real distribution of the GRB sources in the sky.

[Q17] "[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."

[A17] My two sentences quoted above are contained in the second part of a reasoning. That part applies to a "hole" that would be created by a *real* GRB if its source lied closer to us than the last scattering surface (LSS). The first part of that reasoning refers to the situation when the *real* GRB source lies *at* the LSS, which is the case in my model. The aim of the whole reasoning is to point out a possible observational test of my model. The referee took out the two sentences out of context, misinterpreted them, and made them up to look like a defect of my model.

The question asked by the referee should be phrased differently: "If I observe a patch of the CMB at 200 GHz with a bundle of gamma-rays coming from within it, will I see a T 2.75 K blackbody in that direction, or won't I?" This has not so far been answered by observers, and the answer would be interesting irrespectively if my model is right or not.

[Q18] "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."

[A18] I assume that the resolution of the GRB detectors will improve with time, and each improvement will pose a new challenge for the models of my class. Why should "probably" rule out those models already now?

[Q19] "[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."

[A19] I have already answered this comment in [A11] above.

[Q20] "[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."

[A20] The referee's declaration in [Q20] is not consistent with the main body of the second report. I am faced with a barrage of objections, even though I did provide "broad plausibility arguments". Where my model is off the observed value by two orders of magnitude (and maybe less - like in the angular size of the CMB perturbation), the referee objects by saying that "orders of magnitude" (implying *many* orders of magnitude) of improvements are still needed. Where I postpone an aspect of the model to be investigated in the future, the referee demands an answer already now. I am sorry, I feel that my complaint quoted by the referee in [Q20] still applies.

[Q21] "[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."

[A21] It was the referee, not me, who said that there is a problem with the preferred axes of the objects being pointed at the observer. My answer was given in the last sentence of my point [3a]: "So, if the referee is not worried by the multitude of the observed GRBs, then this particular objection against my model does not apply."

....

[Q22] "[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?"

[A22] Yes, lower maximum blueshifts are theoretically possible, and yes, their sources would be much easier to model. I began with attempts to model GRBs just because they seemed to be the most difficult to handle, and because the GRBs stand out by being at the extreme end of the electromagnetic spectrum. Yes, it should be verified whether there are observed candidate-phenomena in lower-frequency ranges, and I hope somebody (perhaps myself) will investigate this in the future. One cannot overload a single paper with information - see the pair [Q20] - [A20] above.

[Q23] "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."

[A23] I believe I have answered this in [A22]. I do not think this problem can be solved in a short comment inserted somewhere in my present paper. There are two more problems involved here:

(i) Each UV, X-ray, etc, phenomenon, *if it is generated by blueshifting the hydrogen emission radiation close to the BB*, will be modelled by a separate BB hump, so my present model of a GRB cannot answer the referee's question.

(ii) One should not take a given model and then ask how it describes UV, X-ray or other phenomena. One should think in reverse: given an observed phenomenon, what model would best describe it. If modelling such phenomena via blueshifting is sufficiently perfected, then it will become possible to use models of this kind to infer about the timetable of the non-constant Big Bang from observations.

[Q24] "[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."

[A24] What should this comment imply? When a theory, which is otherwise well-tested, predicts a phenomenon, the prediction must be put to experimental/observational tests. This is how physics has progressed since the times of Newton. This is what I am doing in my current papers: I am trying to find a place for blueshifted radiation among the observed phenomena.

....

[Q25] "[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."

[A25] My papers provided a proof of existence of the effect. I do not claim that the models I presented are unique - they are only first examples. Other examples should emerge in further research. So the "specific properties" is only a temporary situation. The last part of the second sentence in [Q25] questions the whole logic of theoretical physics. A model fitted to an observed effect being special within a larger class of models is a typical situation in accounting for observations and experiments.

[Q26] "What causes only those QSS regions with appropriate properties to be realized in nature?"

[A26] The supposition made in this question is incorrect. Other QSS models were shown to describe formation of structures and the influence of matter inhomogeneities on the propagation of light rays. Names of authors of the relevant papers were mentioned in my first reply. It is definitely not true that "only those QSS regions" are "realized in nature".

[Q27] "Or, alternatively, why are the implications of models with less extreme blueshifts not discussed?"

[A27] See [A20] and [A26].

[Q28] "[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."

[A28] The subject of inflation is altogether marginal for my paper, and I am embarrassed that the referee is forcing me to engage in such a long discussion of it. But, not to leave this point unanswered, I add this:

I have followed the topic of inflation rather casually, so I may have overlooked something. But all the inflationary models that I have seen in the literature used solutions of Einstein's equations that were homogeneous already at the beginning. At best, they were anisotropic Bianchi-type models that isotropised during inflation, but usually they were in the Robertson-Walker class all the way. The intended message of the papers presenting those models was: "look how fast the expansion is during inflation. It is so fast that it *might* homogenize matter in the Universe within this short period". I have not seen a paper that would begin with an inhomogeneous model and show how it becomes nearly homogeneous in consequence of inflation. Thus, the claim that inflationary models can transform "an arbitrarily inhomogeneous initial state to a smoother almost-homogeneous and isotropic state" is an extreme exaggeration, not supported by any explicit example. Nevertheless, the claim that inflationary models can do this has been the basis of relentless advertising for the inflation idea. This is what I called "inflationary propaganda".

Having said this, I ask the referee not to continue this distractive digression. I still offer to remove the offending sentence from my paper, and then the subject of inflation will disappear from our discussion.

[Q29] "[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."

[A29] This is also only a side-topic and I would prefer not to extend the discussion of it. But being forced to do it, here is my answer: the LT and QSS models are the most general currently known exact solutions of Einstein's equations that can be used as FLRW perturbations. They help us not only to explain various cosmological phenomena, but also to understand the relativity theory more deeply. They use exact mathematical methods, which enables us to establish clear connections between assumptions and results. Perturbative calculations would be unable to discover subtle phenomena like blueshifting or cosmic drift. (They would miss blueshifting because their practitioners never thought of the possibility that the Big Bang function might be nonconstant, while this possibility naturally emerged from exact calculations.) Research into generalizations other than LT and QSS is progressing vigorously; see the account in Ref. [5] below. At present, we work with what we have, and I see no reason to censor out this research by claiming that perturbed FLRW models can do things better.

Re "quite restrictive", I suggest that the referee looks up the recent Ref. [6] below, where quite a sophisticated set of structures was obtained using QSS models. This research area is still young and developing, and any general claims about its weaknesses are premature.

[Q30] ”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).”

[A30] This statement is plainly false. Conditions under which shell crossings do not appear are well-known. Those for the LT models were found by Hellaby and Lake in 1985 (Ref. [7] below), those for QSS were found by Hellaby and myself in 2002 (Ref. [8] below). The arbitrary functions in my model were chosen so that these conditions are fulfilled, see eqs. (2.7) - (2.9). Just in case the referee would like to say again that these conditions are highly fine-tuned: this is not the case. They are simple inequalities to be obeyed by the derivatives of the arbitrary functions. Being inequalities rather than equations they delineate continuous sub-families, labelled by arbitrary functions, of all LT and QSS models.

....

I conclude again that the referee’s objections stem from what he/she *believes* will or will not be possible to do with my results in the future. Those possibilities and impossibilities do require serious investigation, which will not happen if my paper is rejected right now on the basis of intuitive expectations of the referee.

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