We followed the referee's recommendations wherever we found them acceptable. However, the referee is basically wrong in a few points, and in those places we had to object (see below).

In the modified text of the paper we have clearly marked the changes made. If the referee approves the modifications, we will send a clean text with all the comments removed.

The referee's main misunderstanding is in his/her paragraphs 3, 4 and 5, where he/she discusses what is or is not "gauge dependent".

First a terminological remark. The term "gauge" in the meaning "a definite choice of coordinates" has been borrowed from the vocabulary of the theory of linearised perturbations of cosmological models. However, in describing exact solutions of Einstein's equations we prefer to speak about coordinates in spacetimes and foliations of spacetimes, and we shall use this terminology in our reply.

Where the referee writes that the quantities we discuss are "highly gauge dependent", he/she means that, for example, the distribution of mass in a hypersurface of constant time t depends on how we define the time coordinate t. This is true, but when the velocity field u of matter in the spacetime is rotation-free (like it is in the Lemaitre - Tolman, LT, model), there exists a *unique* preferred foliation, the one in which the hypersurfaces $t =$ constant are orthogonal to u. With any other choice of foliation either the metric would contain time-space components or the velocity field would have more than one component (i.e. the coordinates would not be comoving), or both. Such untypical coordinates for the LT metric had been occasionally used for some specific geometrical considerations (see for example Refs. 1 and 2 below), but this made the simple algebraic relations among the LT functions complicated and non-transparent. Virtually all other papers on the LT model used the same comoving-synchronous coordinates that we use in our paper. Thus, there can be no discrepancies between different papers resulting from "gauge dependence" when everyone uses the same foliation ("gauge"), which, we recall, is invariantly and uniquely defined. The only difference between different papers is the choice of the radial coordinate. However, features of density profile at a given instant do not depend on the choice of r. As seen from our eq. (3), at a fixed t, $\rho = 2(dM/dR)/R^2$. Hence the change of r cannot change a void into an overdense region.

This misunderstanding about the "gauge" led the referee to another error in his/her paragraph 5. There, he/she says that there is some ambiguity in defining the hypersurface $t =$ now (there is none – see above) and that it is possible to choose such a "gauge" in which $t_B =$ constant so that each particle in the Universe has the same age. This is not correct. The function t_B in general depends on r, and when it does, no transformation of coordinates can make it constant; t_B being constant or not is an invariant property of the model. The referee probably wanted to say that it would be preferable to choose a new time coordinate τ such that the singular set $t = t_B$ is transformed into $\tau =$ constant. This is possible even with non-constant t_B, but then the hypersurfaces $\tau =$ constant would not be orthogonal to the velocity field u, with the undesirable consequences described above. *Nobody* among the numerous authors who investigated the LT model has used such coordinates.

The quantity $(t - t_B)$, the "age of the Universe", *is* (not "appears") different at different constant-r spheres when t_B is not constant – this is one of the most basic features of the LT model, and one of the most basic differences between LT and FLRW. What we said in the paper is that $(t - t_B)$, a feature of the model, should not be identified with the physical age of our real Universe. The latter is also position-dependent (it must be if the Universe is truly inhomogeneous), but has a different value from $(t - t_B)$ because the LT model fails to accurately describe the actual Universe at very early times.

What we wrote above is common knowledge among relativists, and I think we can expect that much of our readers. Therefore we omitted all these explanations in our paper, and we hope the referee will forgive us for insisting that they remain omitted (their proper place is in a textbook).

The referee said the issues discussed above constituted his/her principal problem with our paper. Since the explanations we gave above solve this problem, we hope that the main objection is thereby removed.

In discussing the remaining part of the report we will have to quote its large parts in extenso. The quotations will be in small font italic.

In the introduction the authors say "We do not claim we are living at or near the centre of any spherically symmetric universe." The rest of the paper then goes on to describe a global L-T space-time. Such models appear to put the observer in a very special place, contrary to the statements made in this paragraph.

This is not a correct description of our paper. We *are not* discussing a "global L-T space-time". We describe a structure of the size of the giant void, which is a *local* (albeit large) structure within a spacetime whose geometry beyond the distance of a few Gpc we do not describe at all. To have a global model one could, for example, match our LT model to a Friedmann model describing what the astronomers believe to be the average background of our Universe. The matching would be done at a certain $r = r_m$. To make the mass density continuous at r_m we would have to arrange for the LT density to go up to the cosmic average ρ_0 at r_m, which is quite easy to do (we did this in a few of our earlier papers, see for example [3]). The results we obtained are independent of the value of r_m, which is again a well-known fact.

Later in the introduction the authors write "Why, then, has this information been overlooked and several researchers have been led astray by the frequent claims of a giant void being implied by an L-T model? We suspect the reason might be twofold." They then write about half a page on the alleged existence of the Hubble bubble and suggest this is why previous authors have found a "giant void" when considering L-T models. I think this is quite unlikely. Most of the papers that describe giant voids are the results of fitting to data (often with a priori constraints on the initial data, as the authors rightly point out). However, I very much doubt these initial constraints were constructed with the intention of making a giant void. They appear to be done for simplicity. If the data had suggested a giant hump after these initial simplifications I expect it would have been reported by most (if not all) of these studies. These comments therefore seem inappropriate to me.

When we said the alleged existence of the "Hubble bubble" inspired previous authors who constructed giant void models, this was no invention on our part. We give below examples extracted from some among the most famous articles published on the issue.

- Alnes, H., Amarzguioui, M. & Grøn, Ø. 2006, Phys. Rev. D73, 083519, write in their Introduction (Section I): "The basic idea ... is that we live in an underdense region of the universe ... An analysis of early supernova data by Zehavi et al. gave the first indications that there might indeed exist such an underdense bubble centered near us (and they cite here Zehavi, I., Ries, A. G., Kirshner, R. P. *et al.* 1998, ApJ 503, 483)".

Then, in their Section II, they write "Since we are interested in studying a universe with an underdensity at the center, we choose ..." and there, they describe their model constructed a priori with an underdensity at the center.

- García-Bellido, J. & Haugbølle, T. 2008, JCAP 0804, 003,

  write in their Introduction (Section 1): "we do live in an (locally) inhomogeneous universe, some say within a large underdense void" (and here they cite as [6] Zehavi et al. 1998, as [7] Tomita 2000, as [8] Tomita 2001a, [9] Tomita 2001b, as [10] Frith et al. 2003 and as [11] Busswell et al. 2004, all these papers being cited in the discussion of our own Introduction).

  Then, in their Section 2.3, they write that their GBH model is governed by six parameters among which "r_0 characterizes the size of the void". Here again, the void is assumed by construction of the model.

- Yoo, C. M., Kai, T. & Nakao, K-i. 2008, Prog. Theor. Phys. 120, 937

  write in their Introduction (Section 1): "The basic idea is that we are in a large underdense region, i.e., a large void; ... Pioneering works include those of Zehavi et al. in 1998 and Tomita in 2000 and 2001. Zehavi et al. analyse early SNe data and suggested that such a large void might exist around us without dark energy. Tomita proposed the void universe model ..."

- Even, Enqvist, K. 2008, Gen. Rel. Grav. 40, 451

  whose model was not a void, writes in his Introduction (Section 1): "one may use the LTB metric to describe a local underdense bubble in FRW universe, for which there is some evidence both from supernova [26] and galaxy data [27]". His reference [26] is Zehavi et al. 1998 and his reference [27] is Frith et al. 2003.

We could add a number of other citations to this list but we hope the referee will be sufficiently convinced by the ones given above.

I do not see the need for Fig.1. The L-T model in the figure is not described anywhere that I can see, and showing that an anonymous model gives different results to LambdaCDM seems unnecessary (it doesn't seem to add anything beyond the statements made in the text).

Fig. 1 was meant to demonstrate that the giant void models are at variance with the "standard" ΛCDM model as concerns the mass density in the redshift space and the $H(z)$ function. The reason for this figure is to show that future observations will distinguish between giant void models and dark energy models. This figure depicts it very clearly. Since no other paper which studied giant void models focused on galaxy number counts we find it important to present these results to our readers. However, following the referee's concerns about the anonymity of the model we provided a description of the giant void model whose galaxy number counts are presented in Fig. 1.

The authors make clear they are trying to reproduce ΛCDM observables in an L-T model with no Lambda. This is not the same thing as reconstructing the L-T functions from observable data, however, even if ΛCDM is consistent with all observations. This is hinted at by the authors in various places, but not made very clear. This seems especially important as the observables the authors consider have not yet been observed out to the redshifts they consider. Number counts, for example, as far as I am aware are only generally considered reliable out to redshifts considerably less than 1. Looking at Fig. 10 out to such distances gives a very different picture to considering much larger (unobserved) distances: In fact, it would appear to suggest a local void rather than a hump.

To satisfy the referee, we added one more sentence at the end of the 3rd paragraph from the end of Sec. 1. As concerns Fig. 10, it should be noted that the density at $t =$ now plotted there has almost exactly the background value at the centre, and goes up to about $1.06\times$ the background value before it starts to drop. Thus, this is definitely not a void, which should have the density of about 10% of the background throughout most of its volume.

In the results sections the authors implement a special procedure to deal with the apparent horizon. I have no problems with this procedure, and see why it is necessary. However, it breaks up the flow of the paper a little and I would suggest to the authors that they consider putting it in an appendix instead (this is very much an issue of style only, and I only mention it as a suggestion to improve the flow of the text).

We made a serious effort to follow this advice. Experiment showed that it would not be enough to move these short segments to the Appendix. Large bits of later text depend on them, so they would have to be moved to the Appendix as well, along with several figures. The flow of the resulting text would in consequence be less transparent to the readers, not counting the danger of us making several mistakes in such an extensive re-edit. Moreover, the description of the changes in the text would be so complicated that the referee him/herself would probably regret giving this advice to us. In the end, we decided to restore the original version here – and we apologise to the referee for disobeying him/her.

Figures 2 and 3 seem unnecessary. The same quantities, with different arguments, seem to appear later in Figures 5 and 6. I don't see why they need to be displayed twice. There are also some oddities with these figures. The x-axis is labelled with a coordinate distance in units of proper distance. The "scale factor" is now a function of r, so shouldn't the proper distance be a non-linear function of r? Also, the quantities being plotted could be more usefully displayed as $-2E/r^2$ and M/r^3, as they would then have straightforward interpretations in terms of FRW quantities (which is what many readers will be interested in). Removing the r^2 and r^3 factors would probably make the plots more useful also, as at present they look very much like simply r^2 and r^3 only. (I, for one, would also be interested to see if the spatial curvature at the centre of symmetry is higher or lower than the asymptotic value. Such a rescaling of variables may make this more apparent). These comments can be applied to relevant later plots as well.

We think Figs. 2 and 3 are useful in addition to 5 and 6. Figs. 2 and 3 present the numerical problem in the way in which it first appears in the calculation, while Figs. 5 and 6 present an approximate solution to the problem and show how good the approximation is. Moreover, the two sets of figures use different independent variables. Figs. 5 and 6 present the functions M and E as functions of $R(t_0, r)$, where $R(t_0, r)$ is the areal distance. The areal distance has always length dimension. We do not understand why the referee calls 'oddity' the fact that $R(t_0, r)$ as well as r are expressed in giga-parsecs – as seen from our (12), r and R must be of the same units because $1 + 2E$ is dimensionless. However, we followed the referee's suggestion regarding the plots of E/r^2 and M/r^3 - with one exception though. Since the radial coordinate is defined by our eq. (12) thus plots of E/r^2 and M/r^3 are not constant even in the FLRW limit. What is constant in the FLRW limit is E/R^2 and M/R^3. We therefore plot them as insets in Figs. 5, 6, 14, and 15.

The authors write just before the start of Section 3.2 the following: "Finally, as seen from Fig. 10, the current density profile does not exhibit a giant void shape. Instead, it suggests that the universe smoothed out around us with respect to directions is overdense in our vicinity up to Gpc-scales." It seems worth pointing out that at present observables do not extend as far as the authors have plotted these quantities. In fact, if the graph were restricted to the region covered by current observation it would show a void rather than a hump. The existence of the giant hump that the authors describe does not, therefore, seem to be a consequence of current observations, but rather of their expectation that future observations out to larger redshifts will follow the ΛCDM prediction. This seems worth making explicit.

Even with the observables covering only a part of our plot, it definitely shows an overdensity – i.e. a density *above* the cosmic average, while a void would have a density about 90% *below* the average, as already stated above. Still, as we claim in the paper, and as already pointed out by the referee, we do not use the real observations but fit a model to expectations of the ΛCDM model. The point is (as already mentioned in several places in the text) that an inhomogeneous model representing the features of the dark energy model exhibits a giant hump rather than a giant void.

In the Discussion section the authors write: "As we said earlier in this paper, the belief that an L-T model fitted to supernova Ia observations necessarily implies the existence of a giant void with us at the centre was created by needlessly, arbitrarily and artificially limiting the generality of the model." I do not consider these choices to be needless or arbitrary, although they do limit generality. It seems to me that the previous work that is being described by these words often has a considerably different aim to the present paper: They are attempting something closer to hypothesis testing (in a Bayesian sense, often). To try and compare a model with arbitrarily many free parameters (i.e. free function) to a model with one constant would results in the L-T models being dismissed as highly improbable. By parameterizing the functions in some way the number of constants to be fitted is then reduced to a workable number, making the proposed (less general) model much more favorable. In this sense, limiting generality is a very sensible, if restrictive, thing to do. I understand the ambiguity in this kind of reasoning, but certainly do not consider it needless (for the goals mentioned above). These comments could be applied to earlier discussion, too.

We removed the words "needlessly and artificially" from the paper because, as the referee explained, there was a purpose in such actions. However, the limitations on generality were certainly arbitrary – there was no physical justification to the special forms of the functions chosen by those authors. We also wish to point out that the results of those papers were misleading – they seemingly implied the existence of the giant void, and, in consequence, caused that a research paradigm came into being, aimed at detecting the void. As we show in our paper, it is not at all certain that the theory really implies the existence of this void.

Figure 20 shows a giant void model and the authors giant hump model. I can't see what model the giant void is referring to though. It would be helpful to mention this in the caption and the text (if it is not already, and I can't see it). It's probably also worth mentioning that (I expect) the giant void model was fitted to data at low z, and not out to $z = 4$. This would make the (unobserved) difference with the giant hump model more understandable.

We included description of the giant void model (page 4, and see also a footnote on page 8). We also modified the caption of Fig. 20.

The authors go on to write: "Thus, while dealing with an L-T (or any inhomogeneous) model, one must forget all Robertson-Walker inspired prejudices and expectations." This seems a bit too much, as FRW cosmology is very useful for understanding lots of aspects of more general cosmologies. I would suggest "be cautious when applying", rather than "forget all".

The referee is very right here, and we made the change.

Shortly afterwards the authors write: "This putative opposition can then give rise to the expectation that more, and more detailed, observations will be able to tell us which one to reject. In truth, there is no opposition." This may not be strictly true. Observations of the kSZ effect, and the growth of linear structure may give effects in L-T models that cannot be easily reproduced by FRW. For the kSZ effect see

arXiv:0807.1326 Title: Looking the void in the eyes - the kSZ effect in LTB models Authors: Juan Garcia-Bellido, Troels Haugboelle

And for linear structure see

arXiv:0903.5040 Title: Perturbation Theory in Lemaitre-Tolman-Bondi Cosmology Authors: Chris Clarkson, Timothy Clifton, Sean February.

These effects seem worth mentioning.

It seems that our message was misinterpreted by the referee. We are speaking about two different things. We, in our paper, give a statement that if homogeneous models are good in cosmology then inhomogeneous models can be even better. On the other hand, the referee has in mind particular inhomogeneous configurations (for example a giant void model) without dark energy. Therefore, the referee says that future observations should distinguish between these two alternatives. While this is true (obviously if there is no dark energy future observations will tell us this) this was not what that paragraph was about.

Therefore, in order to avoid further misinterpretation, we modified our statement. We hope that our message will be transparent for our readers now.

However, even if this is beyond the scope of our article as we stressed above, we want to take advantage of this interesting discussion with the referee to give our opinion about the use of the kSZ effect and perturbation theory in our case.

The kSZ effect of the arXiv:0807.1326 paper is considered assuming that the universe is a quasi-pure LT model around the observer and not, as we claim, that this model is an approximation which takes only the radial inhomogeneities into account. If the LT models were to be ruled out by near future kSZ surveys, this would not be a probe of LT against FLRW but rather of LT against more detailed and complicated inhomogeneous models, e.g., Swiss-cheeses. In a Swiss-cheese, an observer in a distant galaxy cluster would observe a CMB dipole not very different from ours and a small kSZ effect would be likely.

Similar criticisms apply to the perturbation theorem.

Later the authors write: "Thus, if the Friedmann models, CDM among them, are considered good enough for cosmology, then the L-T models can only be better". This is true in terms of reproducing some observations, but for hypothesis testing they could be disfavoured (see comments above on hypothesis testing).

We think the "hypothesis testing" that requires an artificial limitation of the generality of the models being tested is not the right approach to inhomogeneous models. If some sub-model of the LT class passes such a test, then this is a useful result (provided the sub-model does not lead us astray on other topics, as was the case with the hypothetical giant void). But if it does not pass the test, then either the LT class is bad in general, or the tester had made a bad choice of the sub-model. Thus, such testing can go on for ever without any conclusion being ever reached. The astronomers had better invent a method to deal with models that contain arbitrary functions, since the Bayesian or χ^2 analyses so much in favour among them to compare the fits of different models are not at all adapted to such cases. So far, the only exact models we have contain arbitrary functions of one variable, but this is still a great oversimplification. Sooner or later exact solutions containing arbitrary functions of two or three variables will be discovered, reflecting the true flexibility of the initial data for the Einstein equations, and then this kind of "hypothesis testing" will become miserably inadequate. (See also comment above).

Shortly afterwards the authors then write: "the question that should be asked is "what limitations on the arbitrary functions in the model do our observations impose?" rather than "which model better describes a given situation: a homogeneous one of the FLRW family, or an inhomogeneous one?"". In terms of trying to reproduce ΛCDM observations with an L-T model this may be true (although see comments above). But in terms of trying to fit the two models to observations it seems perfectly plausible to ask which model better describes them: The L-T model will probably usually fit better, but it is perfectly possible that ΛCDM could in the future be shown to be inconsistent with observations, or more favorable, in a Bayesian sense.

We are sorry, but here we stand by our argument. We do not speak about the comparison between the ΛCDM model and LT, but between any model of the FLRW family and LT. Arranging for a contest between FLRW and LT is not very productive. At a suitable level of averaging, an FLRW model will still be considered an appropriate approximation to the observed Universe. So we are not aiming at a decision of the kind "FLRW out, LT in", but rather "when FLRW not precise enough, use LT with such and such functions and parameters".

Finally, the last paragraph seems contrary to the rest of the paper. The paper up until this point has been about reconstructing the L-T functions by matching observables to their expected *values in ΛCDM. If this is the programme, then there is only going to be one result – the model the authors have found. In this case there seems no point comparing to any other model, as it will not reproduce ΛCDM as well. Presuming that future observations are in keeping with FRW, the giant hump model will fit better. Of course, future observations may turn out to be in conflict with ΛCDM, but this does not seem to be what this paragraph is considering.*

We agree with the referee and we have removed this paragraph.

In the paragraph below, numbering added by us, for later reference

The list of references also seems to be missing a few recent publications that should probably be included for completeness. These are:

1. *arXiv:0807.1326 Title: Looking the void in the eyes - the kSZ effect in LTB models Authors: Juan Garcia-Bellido, Troels Haugboelle*
2. *arXiv:0903.5040 Title: Perturbation Theory in Lemaitre-Tolman-Bondi Cosmology Authors: Chris Clarkson, Timothy Clifton, Sean February.*
3. *arXiv:0810.4939 Title: The radial BAO scale and Cosmic Shear, a new observable for Inhomogeneous Cosmologies Authors: Juan Garcia-Bellido, Troels Haugboelle*
4. *arXiv:0909.1479 Title: Rendering Dark Energy Void Authors: Sean February, Julien Larena, Mathew Smith, Chris Clarkson*
5. *arXiv:0807.1443 Title: Living in a Void: Testing the Copernican Principle with Distant Supernovae Authors: Timothy Clifton, Pedro G. Ferreira, Kate Land*
6. *arXiv:0809.3761 Title: Can we avoid dark energy? Authors: J. P. Zibin, A. Moss, D. Scott*
7. *arXiv:0902.1313 Title: What the small angle CMB really tells us about the curvature of the Universe Authors: Timothy Clifton, Pedro G. Ferreira, Joe Zuntz*

The last of these seems particularly relevant, as it also finds that an inhomogeneous bang time is necessary to fit to cosmological observables.

All the papers proposed by the referee are actually very interesting but some can be accepted as more or less relevant to our purpose and others not so. We comment the referee's list below:

We added papers no 1, 2, 3, 6 and 7 to our references. We comment on the others below.

Paper 4 considers an nth void class of models with $t_B = 0$ and the improvements provided are not essential. Moreover, since we have the choice among a large set of papers of this kind, we prefer to cite the already published ones.

Paper 5 proposes means to discriminate between a "realistic smooth void model, and ΛCDM". We do not see any connection with our work.

If the authors were to cut back on some of the Figures, and reduce the discussion in the introduction, I expect the paper could be reduced in size by as much as 25% without losing any scientific content.

We wish our paper to be not only a certain scientific statement, but also a pedagogical and historical overview of earlier results. If we follow the advice of the referee, the pedagogical/historical aspect will be all lost. Moreover, our paper is of average length. Shortening it would save some space, but we believe it would become more difficult to read. For example, we considered saving space by combining the following pairs of figures into single graphs containing more curves:

Fig. 5 with Fig. 14,
Fig. 6 with Fig. 15,
Fig. 7 with Fig. 16,
Fig. 8 with Fig. 18,
Fig. 9 with Fig. 17,
Fig. 10 with Fig. 19.

If the referee insists, we can do it, but we believe our paper would thereby become more difficult to read.

References

[1] Gautreau, R. (1984). Imbedding a Schwarzschild mass into cosmology, *Phys. Rev.* **D29**, 198.

[2] Stoeger, R. W., Ellis, G. F. R. and Nel, S. D. (1992). Observational cosmology: III. Exact spherically symmetric dust solutions, *Class. Quant. Grav.* **9**, 509.

[3] Krasiński, A. and Hellaby, C. (2004). More examples of structure formation in the Lemaître–Tolman model, *Phys. Rev.* **D69**, 023502.