Dear Dr. Celerier,

The above manuscript has been reviewed again by two of our referees.

The resulting reports include criticisms which are sufficiently adverse that we cannot accept your paper on the basis of material now at hand. We enclose pertinent comments.

If you feel that you can overcome or refute the criticism, you may resubmit to Physical Review Letters. Although it is our usual policy to conclude the review process after two rounds of review, we are willing to consider the paper for one more, final round of review. Please accompany any resubmittal by a summary of the changes made, and a brief response to all recommendations and criticisms.

Yours sincerely,
Stanley G. Brown
Editor
Physical Review Letters

Report of Referee B – LF11967/Celerier

The authors consider the spherically symmetric Lemaître-Tolman-Bondi (LTB) solution as a cosmological model which could explain supernova distance observations without dark energy. They argue that a LTB model which reproduces the distance and number count observations of the ΛCDM model does not describe a giant void, in contrast to what is usually thought.

Dark energy is a central topic in cosmology, and it is important to understand possible alternatives. The view that a giant void is required in order to fit the supernova data with a LTB model is widely held, and from the point of view of rigorously testing this alternative to dark energy, it is important to understand the different possibilities in detail.

The manuscript is interesting, but also has some shortcomings. The first referee report criticized the manuscript on five different points. Some of the criticism is not valid, while other points are correct and relevant.

- The first referee considered the manuscript to be flawed because there is a non-constant bang time function t_B, which implies a large decaying mode. However, the authors make it clear in their manuscript that the LTB model cannot be extended to arbitrarily early times (because of the assumption of zero pressure). While a large non-constant t_B can be expected to lead to observational or theoretical problems, one would need to how the spherically symmetric region is formed in order to fully assess the issue. Skepticism about the assumption of a non-constant t_B is warranted, but a model should not be rejected out of hand because of it.

- The first referee criticized the manuscript for lack of rigor. Of the issues mentioned by the referee, the treatment of the critical point has some validity. It may be that the authors' analysis is correct, but this is not entirely transparent from the manuscript. For clarity, it would be better to start with the model where the free functions are taken to be polynomials.

- The first referee criticized the authors for assuming that any functions $D_A(z)$ and $m(z)n(z)$ can be reproduced by a LTB model. This criticism is not valid, as this assumption is not made in the manuscript, as the authors point out in their reply.

- The first referee expressed doubts about the model on the grounds that the solution would be a void model in some respects, that the model might be pathological at large redshifts. Regarding the first point, the term void is usually used to refer to a region where the density is significantly lower than outside, and typically the density is lowest at the centre. In the situation in the manuscript, while the density does at first grow away from the centre, the increase is modest, and the central density is in fact much larger than the density at large radii. It is justified not to call this density distribution a void. Regarding the second point, while the model probably has trouble fitting the CMB and other observations, the authors are only discussing the supernova data, which is a valid (if incomplete) approach. (Note that a non-constant t_B contributes to the CMB anisotropies via the Integrated Sachs-Wolfe effect even if the observer is at the centre, contrary to the authors' assertion that it cannot affect the CMB.) These criticisms therefore do not appear to be justified. However, the authors should have have emphasized that the model is not to be taken seriously at high redshifts.

- The first referee's criticism of the third paragraph of the concluding section is correct. The authors acknowledge this in their reply, and their suggestion of removing the paragraph would solve the issue.

The title of the manuscript is somewhat obscure, and I would suggest changing it to something more suitable for a general audience. The authors should also specify which parameter values they have taken for the ΛCDM model.

In summary, the manuscript addresses an interesting topic in cosmology, and in contrast to the first referee, I do not find it to be fundamentally flawed. If the authors (a) clear up the presentation of the treatment of the critical point, (b) emphasize that the model is not meant to be taken seriously at large redshifts (or discuss briefly how the model might be expected to fare with other cosmological data other than the Snae), (c) remove the third paragraph of the concluding section and (d) mention which parameter values they have used for the ΛCDM model, the manuscript should be suitable for publishing in Physical Review Letters.

[Comment from AK: below is the report of referee C copied from a different file, which was attached to the editor's email]

A central aim of the paper is to correct possible misconceptions about the spatial distribution of dark matter for a particular spacelike slicing of the L-T metric. The rebuttal to some of the issues raised by the previous referee are unsatisfactory:

1. To dispel any worries of pathological mass distribution at $z = 1000$, the authors could have plotted on the same plot as $\rho(t_0; r)/\rho_0$ the mass function $\rho(t_1; r)/ < \rho(t_1) >$ where t_1 corresponds to the time on the past light cone of the photon last scattering surface.

2. The authors in their rebuttal wrote "And, by the way, we never intended to claim that this overdensity is a necessary consequence of using an L-T model to fit supernovae.

This is most probably a peculiarity of the model we ended up with. Because of space limitations we had no chance to explain it properly, but we hinted at it in the first sentence of the last section, where we said that such an L-T model "can contain" a hump rather than a void." Since this point is central to the theme of the paper, perhaps the authors should take a hint and try to fit in a reemphasis of this point.

New criticisms:

1. Although having a large radial inhomogeneity at large geodesic distances does not lead to irreparable problems with primary CMB anisotropies (unless of the matter distribution becomes singular such that the redshift becomes anomalously large), it does lead to a stronger fine tuning for the observer being at exactly at the center of the L-T model. This needs to be addressed.

2. No discussion is made of possible ISW problems which probably is more acute for the matter distribution presented in this paper than in large void models.

3. No discussion is made of the baryon acoustic oscillations which may also pose a significant problem. If the paper did not focus on comparing with observations as much, this would not be an issue. However, if the authors would like to argue as they do in the rebuttal "In the redshift range in which we have observations our model predicts results that differ very little from those of the ΛCDM model," they have to do better.

4. I personally do not find the result particularly interesting nor surprising to warrant publication in PRL, but I do sense that some popular astrophysicists have a rather myopic view of the possibilities within the L-T setup. Nonetheless, as far as my vote is concerned regarding the subjective criteria of wide interest and importance, it is somewhat negative. With some clarification, I believe this paper can be published in something like PLB.

Incidentally, the authors cite a previously published PRL article as a reason to publish this. On the other hand, they do not even cite that paper in this paper. The editors should decide if every "correction" to a previously published PRL paper deserves another PRL publication particularly if the "correction" itself may not be the state of the art in terms of physicality and the original paper itself (which probably did not deserve publication in PRL) may not be particularly significant. In summary, I do not recommend this article's publication in PRL.