Reply to the referee

> Although I find this subject matter to be very interesting, I
> question its interest to a general physics audience.

This statement of the referee does not seem to reflect the Phys. Rev. Lett. policy. This journal published the following paper:

Clifton, T., Ferreira, P. G. and Land, K. (2008). Living in a void: testing the Copernican principle with distant supernovae, Phys. Rev. Lett 101, 131302.

In it, the authors take the existence of the giant void for a proven necessary consequence of adapting the L-T model to the SNIa observations. They even discuss ways to confirm or deny its existence on the basis of observations. If, however, a void is not a necessary consequence of an inhomogeneous model of SNIa dimming, then such tests will not be sufficient.

> 1. The basic premise of this work is flawed. I agree that choosing a
> functional form for the energy function can be bad, but the authors
> also appear to be (very forcefully) claiming that there is absolutely
> no reason why the bang time function should be held constant.
> On the contrary, there's actually good reason to hold the bang
> time function constant – so that the early universe makes sense.
> It has been shown numerous times that the energy function of an L-T
> model corresponds to the growing mode of linear perturbation theory,
> while the bang time function corresponds to the shrinking mode.
> See, for example, gr-qc/0510093 (which is actually written by two
> of these authors).

We wish to point out that in that paper we have treated a non-constant t_B as an obviously admissible choice.

> Thus, if the bang time function is non-constant in any non-negligible
> way, then there would be a non-negligible shrinking mode today. Late
> time inhomogeneities from the shrinking mode would need to have
> been huge in the past and thus would strongly impact early universe
> processes. The early universe appears inflationary, and therefore
> choosing a constant bang time function seems sensible.

This argument is based in an FLRW mode of thinking. At best, assuming statistical homogeneity, it suggests that the bang time variation between widely separated regions should not be too large. But do observations constrain how much t_B may vary within an inhomogeneity? How will we know if we don't try it? As we pointed out in the paper, t_B should be treated as a function describing a degree of inhomogeneity rather than as the actual age of the Universe. As to inflation, even if it occurred (for which so far we do not have any direct evidence), one can imagine a Universe in which the inflationary phase ended at different instants leading to large t_B variations. Thus, this point of the referee is flawed. How does the referee know? All current models of inflation are based on FLRW models, where t_B is constant from the very beginning. Without an explicit calculation the claim that inflation does not allow any variations of t_B is merely a speculation.

> 2. There is a general lack of scientific rigor.

From the point of view of general relativity and mathematics we have been absolutely rigorous and we are very surprised to see this criticism. Below we show that it is incorrect.

> For instance, it
> is well-known how a void model physically reproduces the desired
> supernova dimming, but they have not made it clear physically why
> an overdensity should be able to have the same effect.

This is a misunderstanding. A void by itself is does not necessarily produce supernova dimming. It is the specific combination of $E(M)$ and $t_B(M)$ that produces both the dimming down the past null cone and the present-day void. Similarly in our paper, the supernova dimming is an effect of the full model, manifested on the the past null cone. The overdensity is another effect that appears when a present-day constant time section is considered. The overdensity in our model cannot be considered the cause of the dimming because the supernovae, at the moment when they emit their light, lie to the past of the overdensity in the spacetime.

> Simply doing
> a numerical calculation and calling it a day isn't sufficient –
> the authors need to explain their findings.

Phys. Rev. Lett. has a very strict length limit of 4 printed pages, so it is not possible to fully explain everything. We had to trim quite a bit to squeeze our message into this limit, and cannot add anything more, which is why we announce in the last paragraph that a more extended version will be published separately.

> This is especially
> true since there is some skepticism to be expected regarding how
> they solved their equations to find this model. They mention below
> Eq. (9) how there is a critical point in their equations of motion
> that they essentially ignore (and just smooth over after the fact),

This is not a correct description of what we do. Having found that the E and t_B functions of the L–T model cannot be numerically calculated through the "critical point" from the data we took initially, we approximate these functions by polynomials. They give a very close approximation where the initial calculation was possible and go through the "critical point" smoothly. This means we simply take a different L–T model than the one we began with, and we do not hide or ignore anything. We then completely recalculate the observational relations for the LT model defined by these E and t_B polynomials, and compare them with ΛCDM and observations. This is 100% legitimate: propose an LT model, calculate the expected observations, compare with the data.

> and then they refer to other papers "for a rigorous approach."

We admit this seems to imply our current method wasn't rigorous, but this is not the case. We refer to other papers for another method of dealing with the same difficulty, where the problem is solved without approximating the L–T functions with polynomials. What we meant to say is that the cited papers don't need the re-calculation step.

> I think it's a problem that they do not solve their equations in a
> numerically rigorous manner, and then in doing so they arrive at a
> seemingly contradictory model that they do not make any attempts
> to explain in any physically intuitive way.

We hope the above explanation has clarified the validity of our numerical method. Also, we do explain our result in the penultimate paragraph of our paper, where we say:

"What is the cause of this difference between the density distribution on our past light cone and in the t = now space? It is the oft-forgotten basic feature of the L–T model (and in fact of all inhomogeneous models, also those not yet known explicitly as solutions of Einstein's equations): on any initial data hypersurface, whether it is a light cone or a t = constant space, *the density and velocity distributions are two algebraically independent sets of data* (their rigid connection in the Robertson–Walker models is a singular property of the latter). Whatever initial density distribution we observe can be completely transformed by the velocity distribution. For example, as predicted by Mustapha and Hellaby [21] and explicitly demonstrated by Krasiński and Hellaby [22], any initial condensation can evolve into a void and vice versa."

For our referee we add the part that we omitted in the paper because we thought it would be obvious for the readers: It is well-known that the whole secret in imitating the dimming of supernovae with mass inhomogeneities is in replacing the acceleration, i.e. the variation of expansion with time, implied in FLRW models, with spatial variation in an L–T model. In our model we have exactly the same mechanism, except that we have two free functions instead of one. So, after imitating the supernova dimming (which puts one algebraic relation on the two functions), we are still left with one more function to model one more set of observations. This is possible thanks to the function t_B being non-constant.

> They also claim that the overdensity that they derive is not
> observable (which I highly doubt), but without any rigorous justification.

We say the following about the overdensity, already in the abstract: "which, being in a spacelike relation to us, is not observable". For relativists and cosmologists, the fact that observations are on the past null cone does not need repeating. What "rigorous justification" is needed for the fact that an object in a spacelike relation to us is not directly observable at present? To observe it we would have to use faster than light signals. This is a basic conclusion of special relativity, known since 1905 and we did not think any reader of a relativity paper would need a justification here.

> 3. It appears as though the authors are working under the assumption
> that any reasonable sets of data for $D_A(z)$ and for $m(z)n(z)$ can be
> simultaneously fit by a single sensible L-T model by utilizing the
> two free functions. As far as I know, this has never been shown.

This is a strange comment. Our paper is an example of achieving such a result to better than observational accuracy. Furthermore, the algorithm of the Mustapha et al paper was explicitly validated by Lu and Hellaby.

> They do refer to a 1997 paper of the fourth author (Ref. [15]), which
> claims to "prove" this, but it does not. (...)

We referred to the Mustapha et al paper just to be fair to our predecessors, but we did not use any of their results. We did not need this. We only borrowed their numerical procedure that worked OK also for us. What MHE predicted in general, we showed on an example. Moreover, Araujo and Stoeger, PRD 60, 104020 (1999) gave a general demonstration of the possibility to match a dust spherically symmetric solution to both the area distance and the number counts on our past light cone.

> 4. I have serious doubts about their resulting model, as shown in
> Fig. 1. First of all, it looks like their solution is, in fact,
> a void model in some respects.

We ourselves said this - see the last sentence of the second paragraph in section 4: "we are in a shallow and wide funnel at the top of a giant hump in density."

> If we inspect the density plot, it
> looks like the density rises from the center going out to about
> 2 Gpc, and then it plummets for larger radii, going down to 40%
> of the critical density all the way at 10 Gpc out. They don't
> plot past R=10 Gpc, but it looks as though their model is severely
> pathological at large distances.

We don't see any divergencies or anything that might be termed "severely pathological", except that the result contradicts certain expectations. But we must repeat: this hump exists on a SPACELIKE HYPERSURFACE SIMULTANEOUS WITH OUR PRESENT TIME. Whatever happens on it, we will not be able to see for at least hundreds of millions of years to come. This figure should not be taken as something to be tested by current observations, but as an illustrative example of unexpected things that can happen in models more general than FLRW. 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.

> Hints to this can also be seen in
> their H(z) plot, where they show that their solution starts diverging
> significantly from the ΛCDM one at around z=3. They should
> show what happens at higher redshifts, say out to the surface of
> last scattering (z=1000). If this trend continues, as I suspect it
> does, then the age of the Universe (and other various things) will
> be very wrong.

The function $H(z)$ is not one of those we used as input data in our model. We calculated it from our model after it was defined by $D_A(z)$ and $mn(z)$. A discrepancy between ΛCDM and our model should not be counted as our failure. The ΛCDM model itself is a subject of observational tests, and using it as the ultimate standard of correctness looks like dogmatism. What should count is the agreement or otherwise of each model with observations. Our figure shows observations as well, and they cannot discriminate between the two.

Moreover, in LT models, one cannot relate directly $H(z)$ to THE age of the Universe as in FLRW models. There is not ONE age of the Universe, but a different one for each r value. This is due to the fact that we did not constrain t_B to be constant. Therefore, tests based on the evaluation of the age of different objects observed on our past light cone and giving lower limits for the age of the Universe at their location are more involved in LT than in FLRW models. A mere look at the H(z) plot can give no insight about this issue which would need more calculations. However, this was not our point here.

See also our next reply.

> In general, it appears as though the authors are only
> concerned with matching the low-redshift $D_A(z)$ and m(z)n(z) data,
> and seem completely unconcerned with all of the other cosmological
> data that we currently have at our disposal.

The reason why the results are not presented beyond 10 Gpc is that, as we stated several times, we intend to show that a giant void is not mandatory to fit supernova data. Supernova data extends up to z 1.75 which is roughly 5 Gpc (at the current instant). We have few cosmological observations beyond $z = 1.7$ thus we were not worried by the fact that for $R_0 = 10$ Gpc our model deviates from the ΛCDM. The important point is whether it deviates from observations!

> As a result, it appears
> as though their model might suffer from some severe pathologies at
> higher redshifts.

See above - these "pathologies" have no relation to what is currently observable. The referee keeps forgetting that the density distribution he/she refers to is on a constant-time hypersurface, not on a light cone. If the referee is referring to the $H(z)$ and $mn(z)$ data then the word "pathologies" is just not applicable. In the redshift range in which we have observations our model predicts results that differ very little from those of the ΛCDM model. Does the referee make an extrapolation to higher redshifts "by eye" and conclude that it deviates from the ΛCDM model? If so, then how does the referee know that ΛCDM will agree with observations at those redshifts? Besides, once we have more data for higher redshifts, we can easily modify our model to fit them – but not for this publication, since acquiring more data by the observers may take a few years.

> I also don't understand why they don't show the
> resulting $t_B(R)$ in addition to the energy function in these plots.

Because we have already used up the 4-page limit, with much difficulty actually to squeeze our message into it. We showed the graphs that we found most interesting.

> I suspect that their t_B is highly inhomogeneous, in conflict with
> CMB data, as discussed in point #1 above.

Again, this is an unproved assumption of the referee. Our t_B is RADIALLY inhomogeneous, but perfectly isotropic around the central observer, so cannot be in conflict with any CMB data.

> 5. The third paragraph of their concluding section does not make
> any sense. They claim that there exists in the literature the
> "erroneous impression" that the homogeneous FLRW model is not a
> special subcase of the spherically-symmetric L-T models and that
> only one or the other can correspond to truth. As someone who has
> been interested in L-T models for years, I have never come across
> this, and I am perplexed as to why the authors believe such a
> misunderstanding exists.

This is the first comment of the referee with which we can agree. What is done in the literature are attempts to distinguish observationally between an L–T model with zero cosmological constant and a ΛCDM model (FLRW with a non vanishing cosmological constant). The latter is not a subclass of the former and we acknowledge some confusion in the way we put it in our paper. The simplest way to correct this is to delete this whole paragraph, and we offer to do it. But we do not think this secondary little detail is a sufficient justification to reject the whole paper.