This manuscript is on the well-studied problem of how one can fit large scale cosmological data with inhomogeneous (yet still spherically-symmetric) Lemaitre-Tolman (L-T) cosmological models. This has been shown to be a way to avoid the use of dark energy to reproduce the observed Type Ia supernova dimming that we see at larger distances, because the matter inhomogeneities can be tuned in such a way as to have the same effect. In general, people have found that the reproduction of the supernova data requires that we live in the center of a large underdense region (a "void"). However, the authors here argue that this requirement is merely "legend" and was derived while placing unnecessary restrictions on the two free functions available in L-T models. They claim that, by relaxing these restrictions, they can simultaneously fit the supernova and the galaxy number counts data with an L-T model that actually has us living in an overdensity, and not a void.

Although I find this subject matter to be very interesting, I question its interest to a general physics audience. Unfortunately, I also find that this work is fundamentally flawed. For the reasons that I outline below, I do not feel that this is suitable for publication in PRL or Physical Review.

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).

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. I know that at least some of the papers that the authors cite mention this as the reasoning behind choosing this restriction. The authors even state in their introduction that "t_B should merely be regarded as a function that describes a degree of inhomogeneity of the initial or early time conditions." We know the very early universe was highly homogeneous, and hence the choice for a homogeneous t_B.

2. There is a general lack of scientific rigor. 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. Simply doing a numerical calculation and calling it a day isn't sufficient – the authors need to explain their findings. 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), and then they refer to other papers "for a rigorous approach." 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. They also claim that the overdensity that they derive is not observable (which I highly doubt), but without any rigorous justification.

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. They do refer to a 1997 paper of the fourth author (Ref. [15]), which claims to "prove" this, but it does not. On p. 6 of that paper they state the theorem, but then they seem to "prove by algorithm," i.e. it looks like they just state that since there exists a system of equations to solve, that it must always be possible. In their "proof", they say, "By determining the 3 arbitrary functions, we have specified the [L-T] model that fits the given observations and evolution functions. This result simply asserts we can construct a (generally inhomogeneous) spherically symmetric exact solution of the field equations that will fit any given source observations combined with any chosen source evolution functions.

We assert, without proof, that if the given observations and source evolution functions are reasonable, then the [L-T] arbitrary functions will generate a reasonable [L-T] model." Asserting without proof does not constitute a proof.

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. 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. 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. 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. As a result, it appears as though their model might suffer from some severe pathologies at higher redshifts. I also don't understand why they don't show the resulting $t_B(R)$ in addition to the energy function in these plots. I suspect that their t_B is highly inhomogeneous, in conflict with CMB data, as discussed in point #1 above.

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.