THIRD REPLY TO THE REFEREE

1. On "gauge dependence"

We will deal with this question at the end of our reply because this is the most serious disagreement between us and the referee.

2. On our special position in the Universe

The point we wish to make is that our observer is put at the centre – actually a special position – OF OUR MODEL which is only a first exploratory model and is not meant to represent all details of our real Universe. We do not wish to imply that WE (the actual observers on the Earth) are living at the centre OF A SPHERICALLY SYMMETRIC UNIVERSE, but only that the L–T solutions we use must be considered as a first step in the process of modeling cosmological inhomogeneities. They represent the Universe with the inhomogeneities smoothed out over angles, analogous to the smoothing out over the whole space in homogeneous models. The use of such models must be regarded as a first approach which will be followed by more precise ways of dealing with the observed inhomogeneities. We think this is a very important point to be stressed when using L–T models in cosmology. This implies no violation of the 'cosmological principle', especially when L–T metrics are used as models of local structures embedded in a Friedmann background that may contain many local L–T patches. We stress that when only one L–T region with a central observer is considered, this has nothing to do with a global picture of our Universe. Putting the observer off the centre, to compute the results and to compare to real data, is a refinement of any such model, but this cannot be demanded already in a first discussion that is meant to clarify the concepts at a minimal level of mathematical complexity.

Several authors did put the observer off-centre, see, e.g., Schneider and Celerier 1999, Alnes and Amarzguioui, 2007. This increased the mathematical complexity considerably. In addition, stringent limits were put by the CMB data on the distance of the observer from the centre and/or on the amplitude of the inhomogeneities. However, these considerations are not based on the smoothing out over angles, they take the L-T model rather literally, which we prefer to avoid.

This is the important point we want to make and we have thus reworded the paragraph on page 2, para 2, beginning with "The spherical symmetry..." and ending with "...the cosmological principle" and added a footnote in the following paragraph.

3. On the "Hubble bubble"

The referee seems offended by the sentence in our third para in the right column of page 2, beginning with "Why, then, has this information been overlooked..." and ending with "...implied by an L–T model?" We have now reworded this sentence and the end of this paragraph. Even more, if the referee is still unhappy with the "Hubble bubble" part of our Section 1, we are ready to remove it completely.

4. On whether the "giant hump" is apparent.

The referee has deflected the discussion of this matter from the main point of our paper. The structure that we called "giant hump" should not be compared with observations (these were so far unable to decide about the existence of the smaller "giant void"), but with the predictions of other papers. When we compare these two sets of predictions, our model predicts something different than the earlier papers. The referee insists that our structure may still be called a void because observations do not reach far enough to detect even the maximum of our "hump". This is not correct. What is called "void" in astronomy, true to the meaning of the word, is a region where the density of matter is very low compared to the large-scale average, call it ρ_0. Our structure has the density at the centre equal to ρ_0 and goes above ρ_0 with a maximum reached around more than 4 Gpc (see the new Figs. 10 and 19) from the centre. To this, the referee replied (we quote only the key statement): "...I therefore expect the central observer would reconstruct an energy density that is increasing as a function of r, not decreasing. The authors cannot be sure of the contrary unless they actually do the analysis with real data. It therefore seems at least a possibility (and in my opinion likely) that a giant hump is a result of extrapolating observables in the expectation they will follow LCDM, rather than the result of any real data."

Yes, we agree with the last sentence. Our paper is only one possible example of what can happen when the L-T model is put to full use. Constraining the model with real data is to be done in the future, but first we have to agree on the principle we advocate. Precise adjusting the model to real data is worthy of at least one separate paper. But the current question is: what should we change in our paper to make it acceptable to the referee? We believe the whole of our paper is consistent with the message summarised in the last sentence quoted above, but how often do we have to repeat it? And what name are we allowed to use for our structure to avoid protests, and at the same time to emphasise that it is a different structure than in the earlier papers by other authors? We are willing to settle this dispute, but please give us a hint how to do it.

We also wish to comment on another sentence of the referee:

"With only supernova observations and number counts I do not see how an observer would be able to tell if their local environment is above or below the cosmic average density (whatever that means in an inhomogeneous model)."

The cosmic average density that we referred to is a quantity taken from observations, not a parameter of the model. All astronomers believe they know the value of this quantity (although perhaps not all believe in the same value). So, a configuration in which the density is equal to that average at the centre and increases outwards would not be called a void by any observer.

5. On "hypothesis testing"

Our and the referee's arguments about this issue became entangled in such a complicated way that we now have difficulty to find a way out of the dispute. So, let us go back to our original text. The passage in it that gave rise to the discussion was:

"When considering models that go beyond the FLRW approximation, 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 order to reasonably answer this for the L–T model, a general framework for interpreting observations in the L–T geometrical background should be created (and in the future it should be transformed into a framework for interpreting the observations in a still more general, or *the most general* geometrical background). Such a program is still in its infancy, but is being actually developed by C. Hellaby and coworkers under the name 'Metric of the Cosmos' (Lu and Hellaby, 2007, McClure and Hellaby, 2008, Hellaby and Alfedeel, 2009)."

Let us say this in different words. If the L-T model is tested against observations in its full generality, and not in an a priori simplified form, then the observations will limit its generality down to a small (exact) perturbation of the Friedmann model. On the other hand, the "hypothesis testing" requires that the generality of the L-T model is a priori limited in such a way that instead of its two arbitrary functions of r it contains two handpicked explicit functions of r containing only a certain number of constant parameters. Then, such a restricted model is compared with the Friedmann model that never contained anything more general than 3 arbitrary constants. After putting the L-T model in opposition to Friedmann in this way, the "hypothesis testing" is reduced to deciding which model to retain for further research and which one to reject. We say that such comparison is unfair, and necessitated only by the unavailability of better methods that should be created in the future. It is unfair because the replacement of arbitrary functions by explicit functions with arbitrary parameters is always done by a human being, who has no objective clues which function to choose and usually follows his/her expectations and prejudices.

The referee seems to understand what we said as an attack on the astronomers doing the "hypothesis testing" and has put him/herself in the position of their defender. But we did not want to attack anyone or deny the value of anybody's work. We only wished to say that the science of astronomy, in this particular respect, is not prepared to make a full use of the flexibility of the L-T models and to give them a fair chance in the competition. It is even worse prepared to observationally constrain still more general models, containing arbitrary functions of 2 or 3 variables that, we believe, will appear in the future. We wish this situation to change for the better, but we have no constructive idea how to do it. So we just try to make the astronomical community aware that such a problem exists.

Now we ask the referee: what should we change in the paragraph we quoted to avoid offending anyone and make it acceptable to the referee? If we are given a clear hint, we are willing to follow it.

6. Back to "gauge dependence".

In the points discussed above, even if we feel that the referee is pushing us around to suppress some of our thoughts, or to say things we do not agree with, we are willing to give in because saving the main message is most important to us.

However, on the subject of "gauge dependence" the referee is saying things that are in sharp contradiction to basic principles of the relativity theory. Thus, even if this might cost us rejecting the paper, we cannot agree on signing our names under statements that are basically incorrect.

The question of coordinate-dependence may arise if different authors use different coordinates, and the disagreement between their results may be attributed to a coordinate transformation. This is not the case when everybody (and we stress - this is really EVERYBODY, no exceptions) is using the same uniquely defined coordinates, and with good reason (see below). This is the situation with the Lemaitre - Tolman model. In our third attempt to clarify the problem, we refer to chosen segments of the most recent referee report. This will make our reply long, for which we apologise. The quotations begin with $< Q >$.

$< Q >$ ”I assume that the authors recognise that general relativity is diffeomorphism covariant, and that in this sense there is no such thing as a preferred foliation.”

This is true at the level of formulating general laws of the theory (like the general form of the Einstein equations or of the geodesic equations), but definitely not true at the level of deriving and investigating explicit solutions of Einstein's equations. Explicitly defined classes of geometries DO define geometrically preferred foliations. For example, whenever there is matter in spacetime, there is also its vector field of four-velocity and scalar field of mass density, and each of them defines additional structures that are often used to associate coordinates with, as shown below. These coordinates ARE preferred because they are connected to geometrical structures that can be identified in any other coordinate system.

It happens rather rarely that the coordinates in which the Einstein equations are analytically tractable have such a clear geometric interpretation as the CS coordinates for the L-T metric, and are strictly unique. The flow-lines of matter in the L-T spacetime are identifiable in any coordinate system - they are tangent to the timelike field of eigenvectors of the Einstein tensor. Since the congruence of the flow-lines is non-rotating, it defines a UNIQUE family of hypersurfaces (call them H_3) orthogonal to it, and these are the spaces in our eq. (1). They are also identifiable in any coordinate system. Then, the space coordinates (r, θ, ϕ) are defined by the spherical symmetry of the H_3 spaces. The time coordinate in eq. (1) is the proper time of observers sitting on the particles of matter. The H_3 hypersurfaces then provide a natural synchronisation of clocks between different observers: the value of the coordinate time is the same for all observers throughout H_3.

The H_3 are geometrically preferred in yet another way: the synchronisation provided by them is consistent with the special relativistic synchronisation. Namely, each observer can, in her local rest space, determine the (flat) 3-space of events simultaneous with a given event on her world line. (This is done in a standard way, by sending a light signal to a nearby observer, receiving the reflected signal, recording the instants of emission and reception, and comparing the readings of clocks between the two observers.) This locally determined flat space of simultaneous events is tangent to H_3 at every point. The H_3 are the ONLY hypersurfaces with this property; the hypersurfaces of equal age advocated by the referee do not have this property, see below.

$< Q >$ ”The hypersurface they take to record the energy density on must therefore be considered a choice.”

This choice is uniquely defined by the geometry; almost forced upon us as the obvious one. This is the referee's ”no such thing”, a geometrically preferred foliation.

$< Q >$ ”My complaint has been that they are presenting their result of finding a ”giant hump” as if it were not foliation dependent, when it most certainly is (see above.)”

We did not say anywhere in the paper, nor did we indicate, that our results are foliation independent. We ask the referee to point out, exactly which place in our paper gave him/her this impression. Such a claim would have been a nonsense. The important point is that all authors of other papers used the same foliation. So, even though their results were foliation dependent just as ours, the question of "gauge dependence" was never asked.

$< Q >$ "We can argue about whether a CS foliation is the best way to record the energy density, but they must surely agree that if they chose a non-CS foliation (as they are completely at liberty to do) then their results are going to change. It is necessary to state that this is the case (that they work in CS gauge, and that the interpretation of a "giant hump", or otherwise, depends on this). I hope this is not controversial. The authors may consider it obvious, but it needs to be said."

We did define the CS foliation above eq. (1) and never used any other throughout the paper. (Strange that the referee did not notice this. How could anyone write a paper on properties of a certain metric without first defining the coordinates?) The fact that a density profile depends on a hypersurface in which it is calculated is patently obvious. It was just as obvious to all the other authors - all of them said on which hypersurface they calculated their density profiles, and none found it necessary to say that the profile would change if the hypersurface were changed. No confusion resulted because all papers used the same foliation. Thus we see no need to say what the referee pushes us to say. When everyone uses the same, uniquely defined foliation, the results can be compared and the differences between results cannot be blamed on "gauge dependence".

$< Q >$ "What appears to be the more controversial suggestion I made was that the authors should consider a different gauge to see if their results hold more generally, or if it depends on a CS foliation. For this purpose I suggested a gauge were each space-like region has the same age. As I've tried to explain, this seems very reasonable to me, and easy to achieve. All that needs to be done is to use their existing best fit solution and plot $\rho(t_1 + t_b(r), r)$, where t_1 is a constant."

We followed this suggestion and produced new versions of figures 10 and 19. We did even more than the referee asked for - we added plots of the density profiles for times before present in both foliations. But the question is whether this was a useful and sensible exercise. There is an infinity of possible foliations, so how can transforming the result to ONE of them help against being foliation dependent?

A sensible foliation is one that can be used for calculating various results. It should thus be noted that the equal-age foliation (EAF) is not even defined at the start because the function $t_B(r)$ is undetermined at the beginning of our consideration. The functions $t_B(r)$ and $E(r)$ become numerically defined after equating the functions $(H(z), D_L(z))$ calculated from our L-T model to the corresponding functions from the LCDM model. Being defined only numerically, the EAF cannot be used for analytical considerations. Moreover, the simultaneity defined by EAF does not agree with the special-relativistic synchronisation (see above): with non-constant t_B, the locally determined flat spaces of simultaneity are not tangent to the equal-age hypersurfaces. Then the question arises how any observer can determine the EAF hypersurfaces in her local neighbourhood. She would have to know the shape of the t_B function, i.e. the time-schedule of the Big Bang, for observers in her neighbourhood. This is a sensible problem to investigate, but if a solution to this problem should be a pre-condition for determining the coordinates, then the whole undertaking becomes impossible to even begin.

It does not make any sense to use foliations that are not defined in an invariant way, for which there is no chance that any observer might reconstruct the 3-spaces from observations. Apart from the CS foliation the only sensible one is by past light cones of the central observer. This one was tried by Stoeger, Ellis and Nel (Class. Quant. Grav. 9, 509 (1992)), but turned out to lead to mathematically intractable equations and was abandoned.

$< Q >$ "I do not doubt that most other studies of L-T use a CS gauge, and only a CS gauge, but most other studies do not make as their central claim a strongly gauge dependent result (in the sense of the first paragraph above.)"

This statement is very strange. The "other studies" with which our paper can be compared are those that found a giant void - i.e. a density distribution in the hypersurface of constant time coordinate in the CS foliation. If our result is "strongly gauge dependent", then so are theirs, automatically, because we compare different results for THE SAME PHYSICAL FUNCTION (mass density) in THE SAME FOLIATION and in THE SAME HYPERSURFACE. If their results are not "gauge dependent", then neither are ours. We cannot figure out what the referee had in mind when saying this. And one more remark: it is not true that "most other studies of L-T use a CS gauge". ALL other studies do so, we challenge the referee to point out to us just one paper in which any other "gauge" was used. The book by one of us - A. Krasinski, Inhomogeneous cosmological models, Cambridge Univ. Press 1997 - may be helpful in this. (We know of two papers that experimented with other coordinates just to explore their usefulness - the Stoeger et al paper cited above and R. Gautreau, Phys. Rev. D29, 198 (1984).)

$< Q >$ "This is easy to do, and will, I believe, make what is going on clearer. (I suspect that the appearance of a "giant hump" may well be due to the fact that they are comparing the energy density of the central region when it is considerably younger than the outer regions, but cannot tell given their current analysis.)"

Now we have the figures the referee asked for, and they raise more questions than they answer. The text below is meant to convince the referee that adding the new graphs will only cause confusion and that it would be better not to add them to the paper.

These are the points to note:

1. The new graphs show that in the equal-age foliation (EAF) the hump is still there, also at earlier times. It has different geometric parameters, but a qualitatively similar shape. So, the new graphs do not support the referee's claim about "gauge dependence".

2. There is a confusing ambiguity about the choice of measure of distance. All the graphs, for both foliations, use a comoving radial coordinate, which means that points with the same horizontal coordinate in the graph correspond to the same matter particle. But each EAF hypersurface lies to the past of its corresponding CS hypersurface (as seen from Figs. 3 and 12, because the functions t_B decrease with distance). This means that the geometrical distance of a given particle from the centre in an EAF leaf is smaller than in the corresponding CS leaf - because the Universe expands.

Will the referee press us to add this explanation in the paper? Moreover, once EAF is chosen as the basis of coordinates, the geometrical radius $R(t, r)$ is no longer a natural measure of distance because each EAF leaf cuts through different values of t. In EAF, the measure of spatial distance would be

$$\int \sqrt{dt^2 - (dR/dr)^2 dr^2/(1+2E)},$$

the integral along a radial direction in a chosen EAF leaf. This quantity is difficult to calculate and difficult to imagine - it is very counter-intuitive. This is not really a spatial distance because the integration path does not run along local spaces of special-relativistic simultaneity, as explained above.

3. The choice of units of density may also cause some confusion. For each graph, the unit of density is ρ_0 - the density at the centre in the chosen hypersurface. (NB we were not clear about one thing in our earlier explanations - see the newly added sentence at the end of sec. 3.1.4). The physical value of ρ_0 is different at each t, but such units are better for visualising the degree of condensation of the hump with respect to the background. However, this is a good unit to compare the CS profiles between themselves; it is not good for comparing CS profiles with EAF profiles, and not good even for comparing the EAF profiles between themselves. The reason is that each EAF profile lies to the past of its corresponding CS profile, so the greater degree of condensation seen in the graphs is in large part due to the fact that the Universe was denser in the past. To visualise the proper degree of condensation for EAF graphs we would have to take a different unit of density for every instant and FOR EVERY MATTER PARTICLE, namely the background density at the CS instant corresponding to the EAF spatial position in question. Plus, we would have to add this long explanation of what the graph actually shows. Is this a reasonable undertaking?

If the referee's answer to the above question is "yes", then how does this agree with his/her postulate from the first report to make the paper shorter?