Dear Editor,

We are sorry for the delay in our answer - we had to consult the reply to the referee among the four of us, and it took some time.

The referee who evaluated our paper seems to have missed our points. We reply to him/her below. However, he/she seems to have some personal stake in defending VFW and, in at least one point, leads the discussion astray. Therefore, we have to ask you to appoint an independent referee to settle the dispute. Here are our suggestions:

[names deleted from this copy – the persons we listed might not wish to be involved in this affair]

These are our replies to the referee report (we quote only excerpts from it):

"The VFW paper had two major components: first, they resolved the apparent contradiction between models of apparent acceleration with only normal matter and general theorems that such models are impossible;..."

As we show in our paper, there has never been such a contradiction, and so there was nothing to resolve. See next answer below for explanation.

"1. The present authors appear to have misinterpreted the Hirata & Seljak 2005 paper. HS05 proved that acceleration is impossible with only normal matter, for two definitions of acceleration. There is an HS05 theorem for q_1 (denoted q_{HS} in this paper), but also for q_4 (which in LTB or other spherically symmetric geometry is the same as these authors' q_0). The latter theorem is what VFW were referring to and there is indeed an apparent contradiction with some results in the literature."

As we showed in the last paragraph of our Sec. II, our q_0 (HS's q_4) does not measure acceleration/deceleration in a general cosmological model. With HS's definition (13), it coincides with the deceleration parameter used by observers, and it actually measures deceleration only in the Robertson - Walker limit. In more general models, HS's q_4 is unrelated to deceleration, and so it makes no sense to consider it in that context.

The q_1 that we considered is the actual measure of deceleration of expansion in any model. As we said in the paper, there is no "contradiction with some results in the literature", but simply a confusion spread by a few authors who failed to understand the geometry and physics of the LT model. Under the conditions stated by HS in the second paragraph of their Sec. IIB, q_1 is non-negative, implying deceleration. A spurious, or simulated, acceleration appears only if some results calculated in an LT model are read against a Robertson-Walker geometry. The referee makes here the same mistake that we pointed out and criticised in our paper. The quantity that is the cosmological constant parameter in the RW reading (and would imply accelerated expansion in an RW geometry) has no geometrical meaning in the LT model, it is just an arbitrary constant coefficient in a function. As several authors (INN among them) argued, in an inhomogeneous model the acceleration of expansion is mimicked by a spatial variation of expansion, caused either by non-simultaneity of the Big Bang or by the inhomogeneity of the initial velocity. The spatial variation along the past light cone of the observer is interpreted by him/her as temporal variation - i.e. a perturbation of deceleration - if he/she uses an RW model. But this has nothing to do with the physical acceleration or deceleration in the inhomogeneous model.

Note that the RW model in question here IS NOT the homogeneous limit of the LT model, but an unrelated model in which a certain function happens to have the same algebraic form - but not the same geometric interpretation.

"2. These authors claim that HS05 never assumed the absence of a central singularity. However, the q_4 theorem in HS05 depends explicitly on the extrinsic curvature being smooth: in Eq.31 of HS05 one uses its derivative $K_{ij|k}$, and then HS05 proceed to assume that the average of $K_{ij|k}n^i n^j n^k$ over all directions n^i is zero (as appropriate via symmetry if K_{ij} is differentiable)."

Since q_4 is not a measure of deceleration, the above remark is besides the point. But we admit that the title of our Sec. III is not well-chosen, it should actually read "The sign of acceleration does not depend on the presence of a singularity". We will change this title when the dispute is settled.

"If the extrinsic curvature is not differentiable at the origin then this argument breaks down. VFW correctly identify this non-differentiability as the source of the discrepancy between the HS05 theorem and some constructions in the literature."

The referee leads the discussion astray here. The "singularity" referred to in the VFW paper was an infinite value of $\Box R$. The possible singularity in $K_{ij|k}$ is another, independent one, never mentioned by VFW. We stand by our argument: $\Box R$ being infinite has no meaning for the conclusion that $q_1 > 0$ under HS's conditions.

Moreover, in the LT model, the only component of $K_{ij|k}$ that is not zero at the origin is $K_{rr|r}$. This component is regular at the origin provided dT/dr and (rd^2T/dr^2) are finite at r = 0 (where T(r) is the bang-time function, and the coordinate r is chosen so that M/r^3 = constant). Thus, the conditions to avoid a singularity in $K_{ij|k}$ are clearly not the same as those to avoid $\Box R$ being infinite. Consequently, the last remark of the referee is unrelated to the subject of our discussion, and incorrect in addition.

"The present authors take issue with the nomenclature "weak singularity" in VFW. While it is true that VFW's weak singularity is not a spacetime singularity (the universe is not smooth but there is no breakdown of GR analogous to the singularity in a black hole), the disagreement here appears to be primarily an issue of semantics."

There are two issues here:

1. An incorrect use of an established mathematical term.
2. A completely erroneous citation of a paper.

To 1: The term "weak singularity" has a well-defined meaning in relativity. What "weak" means we do not wish to discuss here, to avoid making the discussion too long. However, a "weak singularity" must first be a singularity, which basically means an infinite value of one or more algebraic scalars built from the components of the curvature tensor. This is not the case here. $\Box R$ being infinite has NEVER been a criterion for any kind of singularity. This is not a question of semantics, but of misleading the readers. A real curvature singularity is a bad thing that sometimes disqualifies a metric as a model of a physical situation. Some authors of later papers have taken VFW's "weak singularity" seriously and believed that models containing it should be dismissed.

To 2: The paper by Tipler, cited by VFW as a source of the definition of their "weak singularity", never mentions any "weak singularity", and, of course, never refers to the criterion $\Box R \to \infty$. The VFW authors made Tipler responsible for introducing a false criterion of singularity that in fact they invented themselves.

"3. Section V: ... after several readings I was unable to find anywhere where the present authors' results actually contradict those of VFW. This is in contrast to the authors' claim in the abstract that VFW is "seriously misleading". However, a modified version of the section on transcritical solutions might make sense as a stand-alone paper."

The main point we made here is that what happens at z close to 1 is not any breakdown of a model or any "pathology", while VFW used these expressions to describe the situation there. The same kind of a problem would occur if one tried to invert the function $y = x^2$. Since the mapping $x - - > y$ defined by this function is not one-to-one and has zero derivative at x = 0, the inverse function is not unique and must be defined separately for each of the ranges $x < 0$ and $x > 0$. The "pathology" discussed in the VFW paper is really as simple as this - the LT function R(z) along the past light-cone is not one-to-one, so trying to invert it to z(R) one encounters an infinite derivative dz/dR and cannot proceed beyond the point where dR/dz = 0. This is a property of a certain function in the model, which is a direct consequence of the existence of an apparent horizon, and it does not imply any problem with the LT geometry. A stand-alone paper on this would not be new because several papers have already been published, and we cited them.

"4. Section VI.A claims that Flanagan's Eq. (5) combined with HS05 results contradicts the INN construction."

This is not a correct summary of this subsection. The contradiction is not between Flanagan and INN, but between Flanagan and Raychaudhuri (a much more serious problem), and the INN metrics are explicit examples of that contradiction. What we stated there was this: whether Flanagan's (5) is correct or not, the Raychaudhuri equation must also hold. When combined with the latter, Flanagan's (5) leads to our (6.2), which should thus be generally correct, but does not hold in the INN cases. Since we have not verified Flanagan's (5), we cannot pinpoint the error. However, a contradiction is really there, and Raychaudhuri's equation is not to be questioned. This is a point that requires explanation, but we would prefer not to engage in it. We believe Professor Flanagan might investigate it without much difficulty.

The above is a description of the situation from our (authors') point of view.

Note however that the referee's arguments reveal a self-inconsistency in the reasoning of VFW and of the referee. If Flanagan's (5) were correct, then our (6.2) would be correct, and this would imply that in geodesic spherically symmetric models (LT among them) HS's q_1 and q_4 would coincide at z = 0. At the same time, both VFW and the referee make a big issue of these two quantities being different. This presumably means different at z = 0 because this is the only point at which q_4 and q_0 are ever calculated. Is this not contradicting oneself?

"In summary, I cannot recommend the present paper for publication due to several scientific shortcomings."

As we argued above, we do not see any scientific shortcomings in our paper, while we still see the same shortcomings in the VFW paper that motivated us to write ours.

"Additionally many parts of the paper read as attacks on VFW, with the scientific criticisms as ancillary material; the authors would do well to focus on scientific issues instead."

We believe we have strictly focussed on scientific issues. We had no intention to attack VFW (what reasons would we have for this?). In fact, we sent a copy of our paper to VFW a week before submitting it to PRD, to give them time to reply (which they did not do). Our main goal was to counter the spreading of a few false messages that will poison the literature if left unanswered.