Dear Dr. Krasinski:

The manuscript “Imitating accelerated expansion of the Universe by matter inhomogeneities: Corrections of some misunderstandings” (DC10766) by Andrzej Krasinski et al. has been reviewed by Professor Misao Sasaki in his capacity as a member of our Editorial Board in accord with our standard procedure for a formal author appeal. A copy of his report is enclosed.

Sincerely,

Dennis Nordstrom
Editor
Physical Review D

Report of Editorial Board Member

I read the paper and the correspondence between the authors and the referees. Since the second referee’s report doesn’t contain any new information, I focus on the first referee’s report and the reply from the authors. (There is a preprint posted by Vanderveld et al, arXiv:0904.4319, in response to the present paper, but I will not take it into consideration.)

> ”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.”

I have checked both the Vanderveld et al. (VFW) paper and the Hirata-Seljak (HS) paper. I think this comment by the referee is correct.

> 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.

I’m not quite sure what the authors are trying to claim here. The point of the VFW paper is whether we can be fooled by a (flat) LTB model to conclude that the expansion is accelerating. So, I think there is no problem to consider an apparent q_0 that would be obtained from observation that assumes an FLRW universe.

> 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.

I agree that geometrical quantities of an LTB model are nothing to do with 'apparent' quantities obtained in observation by assuming an FLRW universe. But, it seems to me that this is indeed the point which motivated FVW to write their papers.

> "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 $K_{ij|k}n^in^jn^k$ 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.

About this point I think both sides are correct in the following sense. HS did assume smoothness of the extrinsic curvature to prove the positivity of q_4. And VFW did correctly identity it 'mathematically'; see VFW's (2.23) and discussions around it. But at the same time VFW did make a misleading statement that a singularity in $\Box R$ is the cause of the discrepancy.

> 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.

I agree with the comments by the authors. However, if this is what they want to claim in their paper, perhaps they should do so with some politeness. The wording used by the authors is a bit too harsh and it sounds like they are attacking VFW.

> "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. >

I agree with the authors that the word 'pathology' used by FVW is inadequate. However, since VFW clearly states that they considered the inverse problem only with flat LTB models, there is no logical inconsistency in their conclusion. Hence I think that the authors' claim that "VFW is seriously misleading" is too strong.

Again, if the authors intention is to correct some mistakes or inadequate terminologies in the VFW paper, I suggest them to do it in a more polite and/or objective way.

> ”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.

I suspect that this point was the source of all the controversy. And apparently it was enhanced by VFW’s misleading discussion on ”the weak singularity”.

As the referee pointed out in #2, the discrepancy between Flanagan and INN is due to the non-smoothness of the extrinsic curvature at the origin. If my understanding is correct, an important point to bear in mind is that the cusp in the density is not the direct cause of the discrepancy, although it is related to the non-smoothness of the extrinsic curvature through the Einstein equations.

> 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?

Again, since HS’s q_4 is equivalent to Flanagan’s (5), and since (6.1) as well as (6.2) assumes a smooth extrinsic curvature, there seems to be no contradiction.

> ”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.

Regrettably I cannot but help agreeing with the referee that the present paper cannot be accepted for publication in its present form. The reason for this decision is mainly due to the very aggressive wording used by the authors against VFW, giving the readers an impression that the VFW paper is completely wrong. But some of their claims are apparently based on their misunderstandings of the VFW paper.

Besides there are many expressions that sound quite subjective or emotional. For example, the abstract starts with a very strong sentence against the VFW paper. But instead I would suggest the authors to start with a more objective sentence like 'We re-analyze and clarify several issues associated with modeling the apparent accelerated expansion of the universe by an LTB model...'

Another example is the last two lines of page 2. There the authors state that one of their motives for writing the present paper is because 'VFW's paper is quite often cited'. But even if this was what the authors actually felt, I'm afraid but such a statement shouldn't be included in a scientific paper, simply because it is unnecessary and it doesn't sound scientific at all.

In conclusion, if the authors are willing to revise their paper in such a way that it is written more objectively, by focusing more on their scientific achievements, by removing the harsh words and correcting their misunderstandings, then the paper may be reconsidered for publication.