Answers to the 2nd Review of Reviewer 1

We would like to thank the reviewer again for his thorough review of our article. His hints, in fact, led to an improvement of the text and the figures. Below, please find our point-by-point answers (in our answers, we refer to the line numbers of the 'revision 1 with marked changes', too).

L. 150: Ok, so you store perturbations from a hydrostatically balanced state. But then how do you use this state within the HEVI method. Because there, now you will seek a new solution $q^{n+1} = qp^{n+1} + qr$ *but linearized about* $q^n = qp^n + qr$ *– how does this work?*

Answer: Right, we store the perturbations, denoted with a tilde, from a hydrostatically balanced reference state, denoted by '0'; these are the prognostic variables. The beginning of Sec. 2.2 is reformulated to clarify this. One could write down the equations for these tilde-variables (and this has already been done frequently in the literature), but they do not look very different from the equations given in Section 2, mainly because the reference state is time-independent and $\tilde{M} = M$ since it is at rest, too.

These equations are temporarily integrated by an IMEX-RK scheme, where one has to decide how to define the implicit and the explicit operator, O_{im} and O_{ex}, respectively. Of course, this splitting must be consistent as you have proposed, e.g., in Giraldo et al. (2013):

$$O = O_{im} + (O - O_{im}) \text{ and } O_{ex} := O - O_{im}.$$

In the paper this is expressed by the fluxes and source terms in Eq. (22).

It has been shown in Giraldo et al. (2024) that defining O_{im} as a linear operator is generally sufficient. Additionally, O_{im} may be time-dependent; the linearisation coefficients are occasionally updated using a recent atmospheric state.

Since O_{im} itself is not derived in time, this time-dependency can be quite arbitrary as long as the above-mentioned impl.-expl. consistency is fulfillled (this O_{im} does not change during an RK-step, therefore eventually different time levels in the explicit and implicit stages of an IMEX-RK are not a problem). Thus, the linearisation state stays close to the actual atmospheric state. Thus, what is done in BRIDGE should be quite similar to the 'LHEVI-PS' approach in Giraldo et al. (2024), and it was also used successfully in Baldauf (2021).

The linearisation procedure can be a bit delicate, because it does not need to be exact in a mathematical sense. A certain hydrostatic balancing in the implicit operator part may even be more important than an exact linearisation, and this results partly in some simplifications we have described. We have extended this part slightly: please see our additional answer below to one of your questions in the first review. We also slightly revised the related sentence at the end of the introduction for better clarification: instead of 'Our linearisation approach is slightly different ...' we now write 'Our determination of the linearisation coefficients is slightly different ...'. Additionally, to avoid confusion, we rewrote the equations for the linearisation of the BCs in l. 564 and 565 without the reference state index '0', but mentioned that for the BCs a linearisation around the reference state is indeed sufficient (in contrast to the flux and source term linearisations).

L. 253: what do you mean by p'?

Answer: Thank you for spotting this! It should be a $\tilde{p}$. The prime ' is more often used for this in the literature, so even we have overseen this here. However, the prime serves us to distinguish between the unit coordinate and the terrain-following coordinate indices. Therefore, we decided to denote deviations from the reference state by a tilde instead of a prime. A footnote has been added in Sec. 2.3 to clarify the prime notation.

L. 273: Related to my previous comment: might be worth defining these since it looks like you are simply linearizing wrt the constant reference state.

Answer: Good idea; formulas now show the full dependency on $q = q_0 + \tilde{q}$ instead of only on q_0.

L. 352: "stable for itself" should be "stable by itself".

Answer: Thanks, done.

Figure 6 : gray lines are difficult to discern. Can you use different colors or additional markers? Why are there multiply gray lines in each plot?

Answer: Thank you for this hint. The grey lines should just indicate the true 4th or 5th order convergence rate. Therefore, we originally plotted multiple parallel lines. However, you are right; this is rather confusing, so we now plot only one such line in each plot. Additionally, we increased the thickness of these grey lines so that they are more visible in the legend box.

L. 786: I would not characterize a mass 10^{-10} as "immeasurably small". The point is that it is not double precision zero. I'm not sure why this happens since a Galerkin method should conserve provided that the numerics satisfies a discrete integration-by-parts (so called summation-by-parts). I know this is satisfied for both CG and DG with Lobatto and will be conserved for any nodal method with exact integration. I guess on line 788 you show that this is due to the filter? This is odd but I would recommend rewording lines 786-788 to capture your main points. This is another reason why I'm not a fan of filtering even though we have used them in the past :-(

Answer: Agree, this term is misleading without further specification. By "immeasurably small" we meant that it would be hard to detect such a mass loss, e.g., by pressure observations in a real atmosphere. Of course, the mass loss is visible numerically. It could be an issue for single precision, which we cannot asses at the moment (although formally available, BRIDGE is not yet practically ready for single precision). We have reformulated these sentences now.

We agree that summation-by-parts, under special circumstances, can even deliver local conservation at the subcell level. Due to our Gauss quadrature, we assume that we do not have an SBP property. However, we rely on the usual antisymmetry property of the interface numerical flux, which provides local conservation on a cell-wise level. We have now added a remark after Eq. (27).

Figure 11: "The isolines and partly the colors..." doesn't make sense to me.

Answer: Yes, this sounds a bit odd. Ullrich et al. (2014) obviously used the same color palette than Jablonowski, Williamson (2006), so we have been tempted to refactor a similar color palette and use it for both tests. But Ullrich et al. used different intervals than JW2006, resulting in a certain shift in the colors. Now we have designed our own color palette for the surface-pressure plot that better matches Fig. 4 and 5 of Ullrich et al.

L. 838: maybe I missed it but where do you talk about the local conservation property? You could just show earlier that as long as you have a discrete integration by parts then this will guarantee local conservation. This will also extend to global conservation if the surface flux is continuous across internal edges. I know the proof for exact integration and for inexact integration with Lobatto points but not sure how this is done with the collocated interpolation/integration points you are using.

Answer: Here, by 'local conservation' we mean that what flows out of a grid cell is exactly what flows into the neighboring grid cell, sharing the same edge. Therefore, in this sense, this property should be a direct consequence of the finite-volume heritage of DG schemes (also see our answer above and the additional remark we have added after eq. (27)).

However, the flux transformations we have to perform at the edges in the horizontal directions are a bit more delicate, by the way, doing these flux transformations correctly was one of the main

reasons for the strict use of the covariant formalism. For this, a proof of local conservation is given in Baldauf (2020) JCP, Appendix E (also mentioned now in Sec. 3.2).

L. 857: Giraldo 2024 uses CG not DG. However, Giraldo-Restelli JCP 2008 (in 2D) and Kelly-Giraldo JCP 2012 (in 3D) do describe diffusion for DG (among other works).

Answer: Thank you for this clarification! We have replaced the term "DG method" with "element-based Galerkin scheme" now, because we believe this work generalizes in many aspects beyond the pure CG approach. We use the Bassi-Rebay (BR1) scheme for diffusion, as was done in Giraldo-Restelli JCP 2008 and in Kelly-Giraldo JCP 2012.

L. 858: Why do you say that adding diffusion will violate energy conservation? The global integral of the divergence of a flux should vanish for a finite element method – be it continuous or discontinuous Galerkin methods.

Answer: You are right: Hyper-diffusion (i.e., higher powers of the Laplacian) itself does not violate conservation properties because it is the divergence of a (diffusive) flux. However, in a numerical scheme it depends on the type of implementation. In a DG scheme, a proper implementation would be to extend the local DG or the Bassi-Rebay method to higher derivatives. This approach is very expensive and may not be justified, given that hyper-diffusion is essentially a non-physical term in the equations. A much cheaper alternative is to apply it as a source term for each cell separately, i.e., neglecting the flux between neighboring cells. This is how we interpreted Section 3.4 in Giraldo et al. (2024). However, the price of such a simplification is the loss of conservation (in particular of energy). We have replaced the sentence in L. 858 with this explanation.

Additional changes in the text

We now have an extension to our answer to the following question of your first review of the original version:
L. 210: Section 3.1 is confusing to me. E.g., how is the nonlinear part treated linearly? E.g. in line 248 it is mentioned that "practical simulations reveal" that you only need half of the KE contributions. What do you mean by "practical" and how did you come up with this factor? What happens if you use the complete factor and what savings are you getting by omitting them?

Additional answer: In the meantime we can better motivate the simplifications we made in the linearizations - that is, neglecting some terms and using the factor 1/2: We now show that the resulting implicit momentum flux is simply the pressure perturbation - see the additional equation around line 280. This improves the hydrostatic well-balancing of the implicit state.

- We fixed a few tiny typos in the text (e.g. size of brackets in formulas, bug fix in equation numbering, ...).
- In l. 297, we have replaced the term gz by the more general geopotential Φ. However, in the frame of this article, both are equivalent.
- We have added a missing underbracket on the RHS of the equation in line 266.
- We have added two missing factors ρ in the equation in line 529.
- The two expressions in line 533 have been corrected (because the vanishing vertical derivative of velocity (l. 531) does not mean a vanishing derivative of momentum). Nevertheless, this was merely a copy-paste typo; the uploaded BRIDGE code v0.9 is correct.