Response to Reviewer 1, Manuscript: egusphere-2023-376

Owen Hughes and Christiane Jablonowski

1 Author Comments

We thank Reviewer 1 for their greatly appreciated constructive feedback, which improved the quality of the manuscript. When referring to line number edits made by the reviewers, we refer to the revised version of the manuscript. The key changes in the revised version of the manuscript are as follows:

- We have used the sentence-level edits to improve the flow of the language in the paper.
- We have added additional references to Sect. 5.3, which place the physics-dynamics coupling difficulties observed in SE in the context of the existing literature.
- We rephrased the conclusion to delineate better that modifications to the deep-atmosphere steady state were suggested in Skamarock et al. (2021).

2 Response to Reviewer 1

We thank the reviewer for their kind words and detailed review.

Comment 1:

In 2.3 you describe the method of initialising w for non-hydrostatic models which, you say, doesn't have much impact. I would imagine that, if you did need to get rid of initial condition shocks then you would need to apply a projection to get discretely divergence free initial conditions.

Response: This is an interesting idea, but the implementation would be largely model-dependent. This may be especially useful for the further work we are doing on the Lamb wave, which results from the initial imbalance (although this also appears in hydrostatic models). We thank the reviewer for their potentially useful insights. However, the imbalances are small enough that we have elected not to address this point in the current publication.

Comment 2:

On lines 399-403 you talk about comparing area averages of precipitation. However it is not clear at this stage that this is what you are going to do. The relevance of the paragraph is not clear.

Response: The reviewer is correct that this paragraph is more relevant to subsection 5.2, Precipitation and diabatic forcing. The reference to Chen and Knutson (2008) provides some theoretical justification for why a summary statistic such as area-integrated precipitation is one way of assessing whether a model (in this case, FV) falls outside of the distribution of models at a particular nominal resolution when pointwise convergence cannot be expected.

Comment 3:

On line 480 I would say "compares" rather than "intercompares".

Response: This wording has been improved in the manuscript in line with this comment.

Comment 4:

On line 485, "the" rather than "to".

Response: The language has been adjusted following the comment.

Comment 5:

On line 502, "have" rather than "has"

Response: The language has been adjusted following the comment.

Comment 6:

On line 503, "compact" rather than "concise".

Response: The language has been adjusted following the comment.

Comment 7:

The paragraph including line 530 sounds important, describing how you can remove spurious oscillations. I would think that you should show evidence for this.

Response: While this is an important solution, the problem with SE's coupling strategy and the resultant fix have already been demonstrated in slightly more complex test cases. Language has been added on Line 537 to direct readers to direct comparisons in these other references, which have highly similar qualitative characteristics to the comparison for our test case.

Comment 8:

Line 548, why is damping out a Lamb wave considered to be more desirable than maintaining a Lamb wave.

Response: This paragraph does not explicitly state that it is more desirable. Historically, authors have conducted work to remove horizontal acoustic modes at model initialization (see, e.g., Washington and Baumhefner (1975)). However, the wave detected in our test case is a physical response to a numerical imbalance at initialization. Therefore, once the wave is triggered, it should be preserved. The further work we refer to in this manuscript will be concerned with why this wave is triggered and an explanation of why it is preserved in one out of four dynamical cores studied.

Comment 9:

Line 579 you say that you have "discussed the impact of diffusion on the flow and precipitation". You haven't discussed this much.

Response: These impacts of diffusion are mentioned in the context of model intercomparisons. Lines 435-440 identify FV's diffusive mechanisms as a cause of slower wave amplification. Lines 475-480 discuss the impact of these mechanisms on precipitation structure, especially over the mountain. We have changed the language to clarify that we discuss this topic briefly in the paper.

Comment 10:

Line 589, this is not clear. Say from the start of this topic that you are referring to work does by Skamarock et al 2021. Otherwise you seemed to be saying things without evidence.

Response: We have rearranged the paragraph beginning at line 591 to clarify which information is drawn from Skamarock et al. (2021).

References

Chen, C.-T. and Knutson, T.: On the Verification and Comparison of Extreme Rainfall Indices from Climate Models, J. Climate, 21, 1605–1621, https://doi.org/10.1175/2007JCLI1494.1, 2008.

Skamarock, W. C., Ong, H., and Klemp, J. B.: A Fully Compressible Nonhydrostatic Deep-Atmosphere Equations Solver for MPAS, Mon. Wea. Rev., 149, 571–583, https://doi.org/10.1175/MWR-D-20-0286.1, 2021.

Washington, W. M. and Baumhefner, D. P.: A method of removing Lamb waves from initial data for primitive equation models, J. Appl. Meteor., 14, 114–119, 1975.