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Geoscientific Model Development Discussions
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Interactive comment on "WAVETRISK-1.0: an adaptive wavelet hydrostatic dynamical core" *by* Nicholas K.-R. Kevlahan and Thomas Dubos

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Thank you for your helpful suggestions for improving the clarity of the paper. Please find our detailed responses below.

Significant changes to the manuscript (other than typo corrections) appear in BLUE in the revised version.

COMMENT: 1) Abstract: Most journals do not allow references in the abstract. Check the GMD guidelines.

RESPONSE: Thanks for pointing this out. The guidelines state "Reference citations should not be included in this section, unless urgently required," I guess these references are not really "urgently required", and so we have removed them.

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COMMENT: 2) Reformulate page 2, line 25. It is true that adaptive 3D atmospheric flows, especially in spherical geometry, have not been extensively studied in the past. There are, however, at least two Ph.D. theses in the literature (Jablonowski (2004) and Ferguson (2018)) and have some discussion of 3D dynamical cores on the sphere with dynamic grid refinements. There are more examples for 3D atmospheric flows in limited-area AMR models, like e.g. Skamarock and Klemp (Mon. Wea. Rev., 1993).

RESPONSE: On page 2 we now cite this previous work developing 3D atmospheric AMR codes. However, no other global 3D AMR method has appeared in a peer-reviewed publication. This is an important distinction since it is difficult to evaluate the robustness, accuracy and state of development of codes that have been reported only in unpublished conference presentations or theses. For example, Ferguson's thesis notes explicitly that "Several stability problems were observed in both test cases that need to be better understood and corrected." and that the grid refinement criteria require more investigation (his code is based on the general purpose CHOMBO AMR library).

Similarly, the results reported in the conference publication by Popinet (cited in another comment) have obvious errors in the variance of the potential temperature for the Held-Suarez test case. It is possible that these issues can be easily resolved, but there is still some development work left to do. (We have discovered that with adaptive codes there is a big gap between "almost works" and "definitely works"...)

COMMENT: 3) Page 9, line 12, also page 27, line 3: Typo, either state 'by Dubos et al. (2015)' or use citation format '(Dubos et al., 2015)

RESPONSE: I think you may have mis-read the sentence on p 9: the citation starts a new sentence:

Dubos and Kevlahan (2013)} found that good weak parallel efficiency is possible with as few as 1300 computational elements per core in adaptive runs. The three-dimensional code has better parallel efficiency because the column structure of the data produces a higher computational load for each active grid element.

We have corrected the citation form on p 27.

COMMENT: 4) Page 12, line 13 and 25: The AMR criterion needs to be clarified. Are the grid adaptations invoked if all three indicators eμ, eq, eu exceed the threshold or is it enough that one of them exceeds the tolerance? It is not clear how the maximum norm is computed. Please provide some insight into its evaluation in this wavelet-based method.

RESPONSE: We have clarified on p 12 that a node is labelled active if any associated scalar wavelet (i.e. mass or mass-weighted potential temperature wavelets) is active at any vertical layer, and an edge is labelled active if its associated vector wavelet (i.e. velocity wavelet) is active in any vertical layer. The maximum norm for each vertical layer is calculated in the usual way: the maximum absolute value of the relevant variable over all active grid points and all scales.

COMMENT: 5) Page ‘5, line 28, define the acronym DCMIP

RESPONSE: Done.

COMMENT: 6) Figures 5, 6, 10, 12: Provide more explanation for the selected map projection. Are these stereographic projections? Which point are they centered on (e.g. north-polar stereographic projections)? The blue-red color scheme for all flow figures is very hard to read. Please improve the quality of the figures and e.g. use the examples by Aechtner et al. (2015), Figs. 17 and 18.

RESPONSE: All plane projections are simple equidistant cylindrical map projections on a rectangular latitude-longitude grid, which is standard for many test cases (e.g. Galewesky et al (Tellus 2004) and DCMIP 2008) to avoid complications of different map projections. This is now mentioned in the text on p 18 line 8.

We have re-done all spherical figures using a clearer paraview colour palette similar to that used in the previous paper.

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Discussion paper

COMMENT: 7) Figs. 9 and 10: Is it intentional that Fig. 9 documents the parallel characteristics with e=2 (when adapting on the trend) and e=0.06 in the actual visualization of the flow? The tolerances also differ when the adaptations on the solution are documented. This these two figures both document the baroclinic wave test case, why is the reader not shown the flow fields (Fig. 10) that correspond to Fig. 9? Explain or modify.

RESPONSE: We meant to show the results with the same thresholds. We have corrected this inconsistency and have taken this opportunity to re-run the simulations with the more accurate quadratic PPR remapping. (Because PPR is less diffusive than the original piecewise constant scheme, we used slightly larger thresholds to have a similar number of active grid points at 9 days.) Note that the results are qualitatively similar to the previous version.

COMMENT: 8) Page 24, line 19-20: Add the physical units for the diffusion coefficients

RESPONSE: Done.

COMMENT: 9) Page 25, line 2: typo, should read '. . . we find for the . . .'

RESPONSE: Done.

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