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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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1 Comment on the Speed-Performance characterization

I have read the present manuscript with great interest. I agree with the authors that adaptive methods are likely to become the most effective option for future weather and climate models. As such, the potential for significant scientific impact of this manuscript is large. Unfortunately, the present manuscript does not address the most prominent question that naturally arises when assessing an implementation of adaptive mesh refinement (AMR). I hope to convince the authors to extend the analysis in this work via this comment.

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Discussion paper

The reason to introduce AMR is related to its speed performance, and the authors make various unsupported claims regarding this aspect. I realize that a complete characterization is impractical, but the authors do not present *any* data that quantify this important detail:

1: The claim in line 19 Page 4 is pivotal and should be proven for `WAVETRISK-1.0`. I like to be shown wrong that τ_n is a function of the actual grid structure and will vary with the compression ratio for typical atmospheric flows. If there exist a characterizing numbers for τ_n, they should be listed.

2: It does not help that the authors made an effort to non-dimensionalize the vertical axis in fig. 4, instead of listing absolute performance (e.g. τ_n). Updating the figure would allow for simple comparisons between various AMR strategies, and thereby help to destinguish the good from the inefficient approaches for AMR-code development (with the relevant disclaimers). My first suspicion when a code displays near-perfect parallel scaling, is that it is just very slow, such that the overhead of MPI communications is relatively small. I think it is important to show that this is not the case for `WAVETISK-1.0`.

3: The paper does not address the memory requirements for running with high resolutions/compression ratios. Is there adaptive memory management as well?

2 Furthermore,

1) The authors make contradicting claims regarding their choices for the refinement indicator. In line 32-33 Page 2, they claim that their formulation for the refinement criterion is clearly defined and objective. Whereas in line 5-6 page 29, they admit to operate in a new field and therefore provide more than one option. I have learned form the authors (et al.), that the multi-resolution analysis is indeed a powerful tool to locally quantify non-trivial polynomial content in the discretized field data. But how it should

be linked to mesh-element-size selection is still an open topic, and it should be clearly presented as such. Further, I think the work of Naddei et al. (2019) and the references therein form a good starting point for the interested reader on this topic.

Naddei, F., de la Llave Plata, M., Couaillier, V., Coquel, F. (2019). *A comparison of refinement indicators for p-adaptive simulations of steady and unsteady flows using discontinuous Galerkin methods.* Journal of Computational Physics, 376, 508-533.

2) The Held-Suarez model has been studied before using an adaptive tree grid by Popinet (2012);
S. Popinet - *Quadtree-adaptive global atmospheric modelling on parallel systems.* Weather and Climate Prediction on Next Generation Supercomputers, Exeter, UK, 22-25 October, 2012.

https://www.newton.ac.uk/files/seminar/20121024100510409-153402.pdf

It makes sense to attribute this work proper.

3 Finally,

I did enjoy reading the manuscript and I am convinced this work does represent an important step for the future of atmospheric modeling.

Your sincerely,

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