# Review of 'Configuration and Evaluation of a Global Unstructured Mesh Model based on the Variable-Resolution Approach'

In this manuscript, the variable-resolution (V-R) version of the GRIST model, based on Voronoi tessellations is described. The authors define the mesh generation process and explore multiple refinement approaches with a dry dynamical core test case (the Jablonowski-Williamson baroclinic wave) and a moist test case with simplified physics (the Reed-Jablonowski tropical cyclone). They subjectively (visually) and objectively ($l_2$ errors, etc.) compare V-R simulations against quasi-uniform (Q-U) reference simulations and verify the V-R simulations perform generally as one would expect, particularly based on previous findings with V-R models using similar test cases.

V-R models have indeed been shown to be useful tools and multiple modeling centers are currently pursuing their development. Therefore, further evaluation and validation of such configurations is warranted, especially as V-R models become more commonly used for scientific research and application.

In general, the results here are a confirmation of robust performance rather than any overtly new physical insight. This makes GMD a suitable venue for such work. I do find the manuscript fairly underdeveloped, however. The model description is lacking, particular describing options specific to V-R dynamical cores such as scale-specific diffusion and model timestep. The simulations evaluating the ability of the tropical cyclone to move between resolutions are interesting but feel almost tacked on, with weak expansive discussion, particularly with regard to diffusion behavior. Some other useful and commonly-reported information is also omitted, such as computational scaling numbers.

While the manuscript wasn't illegible by any means, it did contain numerous grammatical errors that detract at times from the science.

I suggest **major revisions**. Again, this is more of an application of existing test cases to an existing model to essentially demonstrate that a V-R configuration is not performing poorly. For this to be a useful reference to other users of GRIST in the future, as well as a comparison benchmark for other modeling centers, some additional evaluation and breadth of discussion is warranted.

## Major comments

- The model description in Section 2 is lacking.
  - For example, it is unclear exactly what numerics are being applied. Finite volume, I assume? What is the vertical discretization? How close are the numerics to the Model for Prediction Across Scales (MPAS)?
  - It seems reasonable that the timestep of a global V-R simulation scales with the finest grid spacing to satisfy the CFL constraint, although this isn't explicitly stated. It would be helpful to note this, however, as some ill-posed V-R configurations can actually be more restrictive from a stability perspective than their equivalent Q-U counterparts.
  - What is the vertical resolution of the model? Is this constant across all configurations, or correspondingly increased in either/both the V-R and higher-resolution Q-U runs? How does this compare to other models with published baroclinic wave and tropical cyclone test results?
  - Appealing aspects of V-R modeling are the computational savings when solving a regional problem. Do the authors have scaling numbers that could provide a more objective quantification of this? Should they expect the simulations to scale linearly with the number of degrees of freedom in the mesh? Is there additional overhead associated with refinement that causes this scaling to be sub-linear?
- Along this line, it is unclear what (if any) modifications are made for the V-R configurations relative to the Q-U. A Smagorinsky diffusion is applied in the horizontal. Is there any additional scale-selective

explicit diffusion such as hyperdiffusion, or does the flow-dependent Smagorinsky handle everything? The latter would imply a fairly diffusive scheme in an implicit sense.

- The moist tropical cyclone test section is underdeveloped.
  - A couple sentences of additional description are warranted. What is the surface configuration, what does the idealized moist physics consist of? Convection? Boundary layer parameterization? Surface fluxes? How else is the model initialized?
  - The cyclone moves through the mesh – how is this done? Is there a background flow or does the configuration relay on beta drift associated with gradients of Coriolis across the cyclone?
  - I would postulate that relative vorticity would be a better quantity to evaluate when assessing potential distortion or wave reflection in a numerical accuracy sense (e.g., Figs. 7-8). Are there artifacts in this field during the TC transit?
  - Other relevant citations which could help contextualize the TC results with respect to dynamical core and diffusion are Zhao et al. [2012] and Reed et al. [2015].
- It is quite unclear exactly what the authors are showing in Fig. 12. Is the goal of this figure to show that V-R simulations are more sensitive to diffusion coefficient than a Q-U grid with the same setting(s)? In some ways, it is a natural finding that a cyclone transiting multiple grid spacing will 'feel' multiple diffusion scales, although as noted above, it isn't stated whether this diffusion explicitly scales with resolution or this is an implicit response. Further, in the abstract, the authors note that this 'suggest[s] the importance of parameter tuning,' although there is not enough description of the configuration to support this statement. Is this tuning just one 'number' for the whole mesh? I would recommend spending another paragraph or two explaining the importance of this finding in the context of the V-R validation exercise and how it pertains to the evaluated version of GRIST.
- Is is unclear from the KE spectra how well the V-R runs are doing. For example, they could be accumulating spurious energy near the grid cell. It doesn't appear that they are from the spatial plots, however, the interpolation to the T106 Gaussian grid means nothing definitive can be said about the refined regions within the nests since those are below the truncation scale. There are a few ways to evaluate KE spectra within a regional model or regional patch, such as those proposed by Errico [1985] and Skamarock [2004]. I would recommend their exploration.

## Minor comments

- Lines 49-53. Wave reflection can be strongly influenced by other parameters than transition zone width, such as numerical method and grid staggering. See Ullrich and Jablonowski [2011].
- It is unclear why both hydrostatic and non-hydrostatic cores are exercised here. Both test cases do not emphasize non-hydrostatic dynamics (being of relatively 'coarse' resolution compared to regional weather models), so it should be expected that both solutions look similar in the absence of some sort of erroneous formulation. There is nothing inherently wrong with testing both cores, although it is mentioned more frequently than probably necessary.
- Section 2.2.2. is quite long, specific, and doesn't add a ton of 'added value' to the manuscript. I would recommend shortening this slightly; keeping the description of important parameters (e.g., $\gamma$, $\lambda$, etc.) and removing extraneous text.
- Lines 192-193. Why is the iteration number different for these grid methods? Is there a quantitative reason, or was this a subjective design choice during mesh generation?
- I am not sure what this sentence means in the code and data availability section: 'GRIST is available at https://github.com/grist-dev, in private repositories. A way is provided for the editor and reviewers to access the code, which does not compromise their anonymity (to our best effort).' I would double-check that this all conforms to GMD's policies.

- I believe both the baroclinic wave and tropical cyclone test case were part of the Dynamical Core Model Intercomparison Project (DCMIP) test suite. It may be worth reviewing multi-center reviews (such as Ullrich et al. [2017]) or references from other labs to see if there is any benefit in comparing results to those previously published using the same test cases.
- Fig. 2. It is not 100% clear which (X4) mesh is being shown. Are all the V-R meshes so similar they functionally look like this? If they are not, the three different generator meshes should be plotted.
- Figs. 5, 8, and 9. Why do the black refinement isolines look 'jagged?' I assume the plotting software is struggling with cell areas right at a given threshold, would recommend smoothing for visualization.
- Fig. 6. Recommend moving the reference slopes above the spectra so that they do not intersect the raw data.

### Typographical errors and grammar

As noted above, there are numerous – albeit generally minor – grammatical errors. This list is not meant to be exhaustive, but rather, a few obvious catches I noted while reading. I recommend a thorough proofread for grammar before resubmission.

- Line 37. ... while permitting...
- Line 43. ... while retaining or minimally degrading...
- Line 64. ... maintains tropical cyclones...
- Line 83. 'three difference initial point sets' is awkward phrasing.
- Line 95. ... developmental ... (?)
- Line 216. ... model level nearest to...
- Lines 232-233. 'Nevertheless...' sentence is awkward.
- Lines 252. Perhaps something like 'sign of the relative vorticity is flipped to account for hemispheric differences' or thereabouts.

## References


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M. Zhao, I. M. Held, and S.-J. Lin. Some counterintuitive dependencies of tropical cyclone frequency on parameters in a GCM. *Journal of the Atmospheric Sciences*, 69(7):2272–2283, 2012. doi: 10.1175/JAS-D-11-0238.1.