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Geoscientific Model Development Discussions

Interactive comment on "DYNAMICO, an icosahedral hydrostatic dynamical core designed for consistency and versatility" *by* T. Dubos et al.

Anonymous Referee #1

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The main portion of this paper outlines the Hamiltonian formulation of DYNAMICO atmospheric model. The Hamiltonian formulation for the hydrostatic equations can be considered an extension and generalization of that outlined in Gassmann (2013). The authors provide a concise overview of other models using icosahedral meshes, discuss motivations for their approach, and provide a limited number of tests for the new model. It is well written and accessible, sufficiently complete as a reference to their model, and it provides a brief introduction into finite-volume modeling on icosahedral grids. The paper should be published subject to the minor corrections and clarifications suggested below.

page 1753, line 20: "Hamiltonian theory for compressible hydrostatic flows and for non-Eulerian vertical coordinates was incomplete until recently" followed by references to

the Authors' previous work - are their references to the "incomplete" work preceding them?

page 1753, line 24: Lauritzen et al (2014a) gives test results for a large variety of schemes. Two points - which one of the schemes in that paper are the authors referring to - SLFV-SL or SLFV-ML (it appears to be SLFV-ML)? Is there not a primary reference for this scheme?

page 1756, 19-22: mass fluxes -> mass fluxes per unit area

page 1757, eqn (9): A and B are also used in eqn (4), for the non-expert, please define them in both places, noting that these are not the same A and B.

page 1758, eqn (9) This general formulation is standard in hydrostatic models - should be noted or perhaps reference(s) added.

page 1758 - 1759: consistent splitting of tracer transport equations should reference Easter, R. C., 1993: Two modified versions of Bott's positive definite numerical advection scheme. Mon. Wea. Rev., 121, 297–304.

page 1757: The advection scheme isn't described in any detail in Gassmann (2013), rather she gives the primary reference - Skamarock and Gassmann (2012)

page 1759, line 17: The only scheme noted in Lauritzen et al (2012) is CSLAM, do the authors mean to reference Lauritzen et al (2014a) or perhaps a primary reference?

Section 2.3: Is this detail necessary, i.e. is there a primary reference(s) describing the reconstruction and limiters?

Section 3.1 - 3.3: Are there any fundamental differences in the Hamiltonian approach presented here and that in Gassmann (2013) (outside of the equation set)? I expected to see some references to Gassmann's work in these sections.

Section 3.4: The time integration method is a non-standard 4th-order Runge-Kutta (only 2nd order accurate for nonlinear equations). For what reasons is this scheme

chosen over RK4 variants that are 4th-order accurate for nonlinear equations? Low storage and ease of implementation are noted (page 1769, lines 18-21), but not as drivers of the decision to use this scheme.

Section 3.5 Filters, page 1771, lines 3-4: What is meant by "applied every other N_diff timesteps"? What is meant by "other" in that phrase? For example, if N_diff is 10, every other N_diff timesteps would imply every 20 timesteps.

Section 3.5 Filters, page 1771, line 13: The operators defined on lines 10-12 are positive-definite for p=1, but I do not believe they are positive definite for p>=2.

Section 3.5 Filters: Are L_theta, etc computed so that the stability limit (in terms of N_diff tau) is known?

Section 5, Results: Generally speaking, it is difficult to know what is the mean cell spacing (i.e. nominal "resolution") given M. It would help readers if the authors gave both in the text and figures.

Section 5.1: Figure 4, change "nbp" in figure to M. Perhaps also give M in terms of mean cell-center spacing on an earth-radius sphere? Also, are there results in the literature with which to compare (Kent et al 2014)?

Section 5.2: How do these results compare with others in the literature, for example with those in the Jablonowski and Williamson (1996) paper, or perhaps in Lauritzen, P. H., Jablonowski, C., Taylor, M., & Nair, R. D. (2010). Rotated versions of the Jablonowski steady-state and baroclinic wave test cases: A dynamical core intercomparison. Journal of Advances in Modeling Earth Systems, 2, 15. doi:10.3894/JAMES.2010.2.15

Section 5.2: In Lauritzen et al (2010), grid imprinting is examined by looking at the ability of the model to maintain the geostrophically balanced but unstable flow in the absence of an initial perturbation. Results from this test would be of equal or perhaps more interest to readers than the qualitative results given in this section, and the results

would provide a quantitative comparison with other models.

Appendix B, page 1785, line 1: "A solution of Eq. (B2) . . ." should be "A solution of Eq. (B1) . . ."

Figure 1: Perhaps a purely horizontal view of the mesh would be sufficient for the left-hand-side panel of Figure 1 (the 3D figure takes a while to interpret), especially given that the vertical staggering is given in the right-hand figure.

Figure 3: The edges of the neighboring polygons are difficult to see.

Figure 5: Why are the results at day 12 presented - most publications show the surface pressure field at day 9. Additionally, the pressure and temperature contour intervals should be given so comparisons can be made with other published results.

Figures 6 and 7: Please give contour intervals for all plots - the color bars show a continuous gradation and the intervals cannot be determined.

Figure 8: What specifically is "grid size"? Is it the side length of the triangle normalized by the side length of the original icosahedral triangular mesh?

Interactive comment on Geosci. Model Dev. Discuss., 8, 1749, 2015.