DYNAMICO, an icosahedral hydrostatic dynamical core designed for consistency and versatility

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"Final response" to interactive comments (Anonymous referees #1 and #2, Exec Editor Dan Lunt)

Both referees are positive and make a number of remarks to improve the clarity of the paper and the presentation of the numerical results. We have taken into account most of them and provide detailed answers below. The main changes to the manuscript are as follows :

- subsection 2.3 giving details about the horizontal transport scheme has been removed since a primary source for this information is now available
- a new subsection 3.5 discusses in more detail the main differences between the present approach and that of Gassmann (2013).

All changes to the text have been highlighted in red for the convenience of referees.

Referee #1

1. *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?*
The references are to extensions of the Hamiltonian theory to compressible hydrostatic flows and to general vertical coordinates - this has been clarified.

2. *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?*
The scheme used here is SLFV-SL. This is now specified. There is now a primary reference (not available at the time of submission) which is now referred to : Dubey et al. (2015) DOI: 10.1016/j.apm.2015.04.015

3. *page 1756, 19-22: mass fluxes -> mass fluxes per unit area*
The term "mass flux" sometimes refer to a vector field (density times velocity) and sometimes to its integral through some surface. In order to avoid this ambiguity, we now refer to "mass flux vector" for the vector field and to "mass fluxes" for the integral.

4. *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.*
Agreed. In addition we have renamed a, b the coefficients A, B involved in the mass-based coordinate in order to avoid a conflict with areas A_i and Bernoulli function B.

5. *page 1758, eqn (9) This general formulation is standard in hydrostatic models - should be noted or perhaps reference(s) added.*
A mass-based coordinate is definitely standard in hydrostatic models, but also commonplace in non-hydrostatic models. We deliberately refer to the coordinate as mass-based rather than pressure-based because the link between mass and pressure is specific to hydrostatic models (more precisely hydrostatic, traditional, shallow-atmosphere models with a pressure top boundary condition) while the concept of a mass-based coordinate applies to arbitrary equations of motion and boundary conditions. This point has emphasized in 5.4.

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

7. *page 1757: The advection scheme isn't described in any detail in Gassmann (2013), rather she gives the primary reference - Skamarock and Gassmann (2012)*
Here we do not refer to a specific finite-volume scheme but to the fact that more elaborate reconstructions are possible within the Hamiltonian framework, an idea first put forward by Gassmann (2013). This has been clarified.

8. *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?*
Thanks, should have been Lauritzen et al (2014a) indeed - another mistaken reference. We now also include to Dubey et al. (2015)

9. *Section 2.3: Is this detail necessary, i.e. is there a primary reference(s) describing the reconstruction and limiters?*
Indeed since details are now available in Dubey et al. (2015) section 2.3 has been replaced by a 4-line paragraph at the end of section 2.2.

10. *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.*
Indeed we should have highlighted the most significant differences wrt to Gassmann (2013). They are the use of a non-Eulerian vertical coordinate and the use of covariant rather than contravariant momentum components as prognostic variables, which simplifies considerably the computation of vorticity and the discretization of the Poisson bracket. A discussion paragraph (4.5) has been added.

11. *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.*
This is a good remark and referee #2 makes a similar point. The main reason (pointed out in the text) is that, given the second-order (at most) accuracy of the spatial discretization, fourth-order temporal accuracy will likely not materialize in practice. Therefore the main design goal is stability, and this simpler scheme is as stable as any 4th-order, 4-stage RK scheme. However there exist second-order RK schemes with more stages that have a better efficiency (maximum imaginary Courant number $2N$ for $2N+1$ stages) (Kinnmark & Gray, 1984). Such schemes have now been implemented, and this section has been updated accordingly.

12. *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.*
"other" has been removed.

13. *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.*
Indeed hyperviscosity is known to produce oscillatory solutions (e.g. Jimenez JFM 1996). This point has been made in order not to give the false impression that the filters are positive.

14. *Section 3.5 Filters: Are L_ theta, etc computed so that the stability limit (in terms of N_ diff tau) is known?*
We compute L_θ etc. just after mesh generation. Strictly speaking stability depends only on the dissipation time scales, time step τ and N_{diff}. The latter is computed to ensure stability. Disspipation time scales are combined with L_θ etc. to give the hyperviscosities in (29-30).

15. *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.*
Indeed this information was given only in Table 1 and not for all values of M. All values are now given at the beginning of section 5 and in the text and captions.

16. *Section 5.1: Figure 4, change "nbp" in figure to M.*
Thanks for spotting this - it has been fixed.

17. *Perhaps also give M in terms of mean cell-center spacing on an earth-radius sphere?*
Yes (see point 15)

18. *Also, are there results in the literature with which to compare (Kent et al 2014)?*
We now briefly compare our results to the sample results presented in Kent et al (2014).

19. *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*
Admittedly, the discussion of the numerical results is rather superficial but we feel that an in-depth comparison is out of the scope of this paper which focuses on the method. We now provide a brief statement pointing to figures of Jablonowski and Williamson (1996) to which interested readers can compare our results.

20. *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.*
Although the setup of Lauritzen et al. (2010) is cleaner, what happens in the Southern hemisphere (SH) is essentially the same as what would happen without the initial perturbation. Indeed the gravity wave triggered in the northern hemisphere by the initial perturbation, although it does propagate to the SH, is not the seed of the instabilities that develop there. We have now computed l_2 errors of surface pressure in the SH and compared to values provided in Lauritzen et al. (2010). (see also point 24).

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

22. *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.*
We have made the left-hand-side 2D.

23. *Figure 3: The edges of the neighboring polygons are difficult to see.*
This diagram has disappeared with section 2.3 (see point 9).

24. *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.*
We present pressure at day 12 to emphasize grid imprinting, which is not visible at earlier days at the higher resolution $M = 128$. Notice that temperature is presented at day 9 which enables comparison with other published results (see point 19).

25. *Figures 6 and 7: Please give contour intervals for all plots - the color bars show a continuous gradation and the intervals cannot be determined.*
Intervals are now given in figure captions.

26. *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?*
Here we have not provided the exact definition since the focus is on the dependence of error on grid size (power law) rather than their absolute values. We now provide this definition in the caption.

Referee #2

1. *page 1760 line 24: Is SCVT defined anywhere? On a SCVT grid, do the C_i and G_i coincide?*
Thanks for spotting this missing definition. This part of the text has been replaced by a reference to Dubey et al. (2015), see comment by referee #1.

2. *page 1767: Since the authors discuss efficiency of RK methods, but end up using a RK4 method with the usual efficiency, I'll note that there are low storage 3rd order accurate RK methods which have more stages, but are more efficient in terms of CFL per stage.*

(see similar point 11 made by referee #1). We are aware of second-order RK schemes with $p = 2N + 1$ stages and maximum courant number $2N$ for imaginary eigenvalues (Kinnmark and Gray, 1984a). We have implemented the $p = 5$ method. We have updated this part of the text accordingly. Kinnmark and Gray (1984b) have also proposed third- and fourth-order methods with a slightly worse stability limit. However they consider only linear equations. We would be highly interested in references to RK schemes with better properties than ours.

3. *page 1771: The authors mention hyperviscosity for control of grid scale noise, but dont mention its more common usage of controlling the physical energy or enstrophy cascade. What is DYNAMICO would use for that very important component? Although later I see this was addressed somewhat in the conclusions.*
   As is now stated at the beginning of 3.5, hyperviscosity is indeed used both as a noise-control device (filter) and to handle nonlinear cascades (subgrid turbulence model).

4. *page 1774 line 10. Typo in reference - missing year?*
   Thanks, fixed.

D. Lunt, executive Editor

- *The paper must be accompanied by the code, or means of accessing the code, for the purpose of peer-review. If the code is normally distributed in a way which could compromise the anonymity of the referees, then the code must be made available to the editor. The referee/editor is not required to review the code in any way, but they may do so if they so wish.*
  The submitted manuscript points to the webpage `http://forge.ipsl.jussieu.fr/dynamico/wiki` where detailed instructions can be found to obtain, compile and run the code and analyze results.

- *All papers must include a section at the end of the paper entitled "Code availability". In this section, instructions for obtaining the code (e.g. from a supplement, or from a website) should be included; alternatively, contact information should be given where the code can be obtained on request, or the reasons why the code is not available should be clearly stated.*
  The information mentioned above now constitutes a short (unnumbered) section.

- *All papers must include a model name and version number (or other unique identi- fier) in the title.*
  We now give a version number (1.0) in the title.