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Interactive comment on "Verification of a non-hydrostatic dynamical core using horizontally spectral element vertically finite difference method: 2-D aspects" *by* S.-J. Choi et al.

T. Benacchio (Referee)

tommaso.benacchio@metoffice.gov.uk

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Review of the manuscript "Verification of a non-hydrostatic dynamical core using horizontally spectral element vertically finite difference method: 2-D aspects" by S.-J. Choi, F. X. Giraldo, J. Kim, and S. Shin.

General comments

The manuscript "Verification of a non-hydrostatic dynamical core using horizontally spectral element vertically finite difference method: 2-D aspects" by S.-J. Choi, F. X. Giraldo, J. Kim, and S. Shin describes the formulation and verification of a numerical model for the nonhydrostatic compressible equations in a two-dimensional vertical-slice framework.

The model uses a direction-split approach in space with a spectral element method in the horizontal and a finite difference method in the vertical direction. The feasibility of the model for the simulation of atmospheric flows is tested on mountain wave, rising and falling thermals and inertia-gravity wave benchmarks.

The article is well-structured and the employed numerical methods are outlined in a thorough and concise way. The results are in line with published work and look promising with a view to a full three-dimensional implementation. However, especially in the results section, there is a lack of precision due to missing information in the comparison with the literature. Moreover, throughout the paper the phrasing is at times unclear.

Once the points listed below are addressed and the language revised, I suggest the manuscript should be published in GMD.

Specific comments

1. Motivation: what is a typical situation in which the direction splitting would be of advantage over unsplit SEM or finite-difference approaches? At least a reference would be useful to better assess the scope of the paper here.

2. Time discretization: which order of accuracy is the employed discretization? According to Skamarock and Klemp (2008) the method should be second-order for nonlinear equations, but no discussion is present in Section 3.2 of the manuscript about this point. Furthermore, split-explicit models usually employ divergence damping for stability reasons. Is that the case for the discretization proposed in this paper as well?

3. Some pointwise comparison with WRF model on the same tests at a given resolution would be helpful to better assess the results, see e.g. figures 4 and 5 of Skamarock and Klemp (2008). If an accuracy gain is achieved, this should be explained and documented. Otherwise it is difficult for the reader to understand the advantage given by the proposed discretization over the existing ones.

4. The employed time steps are not reported in the description of the numerical tests. For the sake of reproducibility the employed time step sizes / Courant numbers should be reported for each simulation.

5. The model is effectively shown to compare well with published solutions. It would be interesting to assess the model behaviour in a convergence test. For instance, in the case of the density current, a self-convergence test like the one documented in Figure 6.7 of Restelli and Giraldo (2009) could be performed.

6. As already noted in a previous comment in the discussion, it would be interesting to evaluate the maximum vertical velocities generated by the model in a long-time simulation of a resting atmosphere above orography.

7. The language needs to be revised as sometimes the phrasing is unclear and the reading flow is lost. This is especially true for, but not limited to, the Test cases Section 4, and subsections 4.3 and 4.4 in particular.

Technical corrections

- Page 3718, line 9: what does “quadrature” refer to in a finite difference context?
- Page 3718, lines 9-12: “The Euler equation ... in this model”. The reader is left guessing which kind of vertical coordinate is used in which model. Please rephrase in a clearer way.
- Page 3718, line 12: “verified”-> validated
- Page 3718, line 26: Graldo -> Giraldo.
- Page 3719, line 18: “ an attractive alternatively” -> alternative
- Page 3719, line 22: “conservative flux-form finite-difference method” -> is it a finite volume or finite difference method? Please clarify.
- Page 3720, line 18-20: the sentence is not clear, what does “in which” refer to?
- Page 3720, line 26: “reported by PK13”, is the coordinate introduced in PK13? Otherwise please include a reference to the work where the hybrid coordinate is first used.
- Page 3721, line 16: it is not immediately obvious that equation (4) is in flux form, given that μ_d is variable. Moreover, in the last term of the first line of equation (4) it is not clear whether ∇_η is the gradient only of ϕ or includes the bracket as well.

- Page 3722, line 7: are the overbars needed above z as well?
- Page 3722, lines 9-16. The sentence is too long and includes two formulas. Please rephrase.
- Page 3723, line 11: "(X-Z) slice framework", is there a reason why x and z are capitalized here?
- Page 3723, line 19: the text in the bracket is somehow confusing. Surely the basis functions cannot be constant?
- Page 3724, line 10, "basis function": please refer to ψ_k here as well.
- Page 3724, line 16, "The right-hand sides is evaluated" -> are evaluated.
- Page 3725, line 13: the index k is used in Section 3.1.1 for the formulation of the horizontal discretization and in Section 3.1.2. in the vertical. The authors may consider using a different index to improve clarity.
- Page 3725, lines 16 and 17: are the brackets encompassing the derivative terms needed in the inline formulas?
- Page 3725, lines 22-24: which kind of quadrature rule is actually used in the vertical?
- Page 3726, lines 1-8. The sentence is too long and hard to follow.
- Page 3727, line 22: "center of the profile". You can add "x_c" afterwards to define it.
- Page 3728, line 14: "The extrema ... is" -> are.

- Section 4.1. Please report the information about the use of 5th order polynomials for this test case as detailed in the caption of Fig. 2 in the text as well. Moreover, it would be helpful to report at which resolution the referred studies are running this test case.
- Section 4.2. In the original study of Straka et al. (1993) , the 15 K perturbation is actually on the temperature, not on the potential temperature, see also Müller et al. (2013) for corroboration. This results in an initial potential temperature perturbation of -16.63 K in the center of the cold bubble (see the caption of Figure 1 page 4 of Straka et al. (1993)).
- Page 3729, line 7: how is the viscosity term discretized? I appreciate the authors have replied to another comment about the bubble section regarding the discretization of the diffusion term. If the same discretization for the diffusion term is used both in Section 4.2 and in Section 4.4, it might be a good idea to anticipate the description to the first time it is mentioned, i.e. in Section 4.2.
- Page 3730, lines 8-13: the sentence is long and hard to read, "of which" at line 12 appears to refer to the Table but should be clarified. Same for "relieved" at lines 15-17.
- Page 3730, line 23: the phrasing "the perturbation diverges" is prone to misunderstanding. Please reformulate.
- Equation (25): shouldn't the bracket in the denominator be squared as in Skamarock and Klemp (1994) eq. (16)?
- Page 3732, lines 1-5: The first sentence of the page is hard to follow, please rephrase.
- Page 3732, line 26 to page 3733, line 5. To facilitate readability it would be a good idea to split the long sentence into two sentences.

- Page 3732, lines 9 and 12. What do you mean by “perfectly simmetric” and “concaving contours”?
- Figure 1: is there a reason why p_s is indicated but p_t is not?
- Please beware that in a printed version of the article some of the text in the figures appears so bold that it becomes unreadable, notably in the axis labels in Figures 2 to 7, 9 and in the contour information in figures 2, 3 and 9.
- It would be a good idea to give the contour interval values in the captions of Figures 4, 5 and 7.
- Figures 4 and 5: in most references reporting the right branch of the density current (e.g., Figure 4 of Skamarock and Klemp (2008) and Figure 7 of Giraldo and Restelli (2008)), the range in the x axis is limited to $x = 19200\ m$.

References

Giraldo, F. X. and Restelli, M. A study of spectral element and discontinuous Galerkin methods for the Navier–Stokes equations in nonhydrostatic mesoscale atmospheric modeling: Equation sets and test cases. Journal of Computational Physics 227, 3849–3877, 2008.

Müller, A., Behrens, J., Giraldo, F. X., and Wirth, V. Comparison between adaptive and uniform discontinuous Galerkin simulations in dry 2D bubble experiments. Journal of Computational Physics 235, 371–393, 2013.

Restelli, M. and Giraldo, F.X. A conservative discontinuous Galerkin semi-implicit formulation for the Navier-Stokes equations in nonhydrostatic mesoscale modeling. SIAM Journal on Scientific Computing 31, 2231–2257, 2009.

Skamarock, W.C. and Klemp, J.B. Efficiency and accuracy of the Klemp-Wilhelmson time-splitting technique. Monthly Weather Review 122, 2623–2630, 1994.

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