Review of the revised GMDD Manuscript: Adaptive Cartesian Meshes for Atmospheric Single-Column Models, a study using Basilisk 18-02-16

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Comments from Reviewer 3:
The revised manuscript has taken most of my previous comments and questions into consideration. However, the new manuscript has now raised many new questions and concerns, as there are several sign, math and physics errors in the new equations, inadequate descriptions of the test cases and forcing mechanisms, and missing parameter values and undefined symbols. The authors need to provide enough explanations to enable others to repeat the test setups.
In case the sign and math errors were present in the computations, all results need to be repeated and reevaluated. This might require major revisions.

Detailed comments:
1) Eqs. 1a-1d: Using references like Holtslag and Boville (1993), Liu et al. (Mon. Wea. Rev., Feb. 2013) or Andreas and Murphy (J. Physical Oceanography, Nov. 1986), all four surface flux equations 1a-1d have the wrong signs. Explain the sign discrepancy to the aforementioned papers and the sign convention used in this manuscript. In addition, the definition of q_0 needs to be 'saturation specific humidity at the surface'. There is a possibility that the surface fluxes have been incorrectly applied in this manuscript, which would necessitate a repetition of all simulations. It is also noted that the cited reference Louis (1982) for the surface fluxes does not exist. It is likely that the authors mean the paper Louis et al. (1982):
Louis, J.-F., Tiedtke, M, and Geleyn, J.-F.: A short history of the operational PBL parameterization at ECMWF. Proceedings of the Workshop on Planetary Boundary Layer Parameterization, 25-27 November 1981, ECMWF, Reading, U.K., 59-79, 1982
https://www.ecmwf.int/en/elibrary/10845-short-history-pbl-parameterization-ecmwf
However, this paper does contain any discussion of the surface fluxes (only some exchange coefficients) and is therefore an inadequate reference for the surface fluxes on page 3 line 14. The other provided reference Beljaars et al. (1989) is gray literature (is this an internal technical report, there is insufficient information), it is not available online, and has limited value here. Provide a better reference.

2) Eq. 3: There is again a sign error in this equation. The current formulation wrongly leads to a negative surface-layer bulk Richardson number for stable conditions with $\theta_{v,1} > \theta_{v,0}$. Such a stable stratification needs to have a positive Ri_b number (see also Holtslag and Boville (1993), their Eq. (2.8), for the correct definition). Since Ri_b is used in Eq. (5), there is the potential that most of the simulations in this manuscript are wrong. This needs to be clarified.

3) Page 4, lines 4 & 5, and Eq, (3): The definition of $\theta_{v,ref}$ is vague. What do you mean by 'reference value'? Provide the exact definition. Obviously, this reference value of $\theta_{v,ref}$ in

the surface layer must be different than the $\theta_{v,ref}$ values used later in the equation for the planetary boundary layer (Eq. (11). However, the same symbol is used, and no further explanations are offered. Correct this.

4) Page 4, line 8-9: the capital $Z_{0,M}$ symbol is undefined, needs to be $z_{0,M}$. Provide the value of the roughness length to make the results reproducible.

5) Eq. (9): Incorrect definition of the vertical wind shear magnitude. It needs to read

$$S = \left|\frac{d\vec{v}}{dz}\right| = \left\| \begin{pmatrix} \frac{du}{dz} \\ \frac{dv}{dz} \end{pmatrix} \right\| = \sqrt{\left(\frac{du}{dz}\right)^2 + \left(\frac{dv}{dz}\right)^2}$$

instead of the currently used definition

$$S = \left\|\frac{dU}{dz}\right\| = \left\| \frac{d\left[\left(u^2 + v^2\right)^{1/2}\right]}{dz} \right\|$$

with undefined symbol $\| \ \|$. If the incorrect formulation has been used in the computations, they will need to be repeated.

6) Eqs. 14, 15, 16: You converted the former vector equation to a scalar equation (as requested) but left the scalar product operator in the formulation. This is mathematically incorrect for the scalar formulation. The dot product needs to be removed. At which time level is the forcing 'r' evaluated? Explain (page 6, line 24) that 'n' denotes the time level.

7) section 3.1: The description of the Ekman spiral test is insufficient and needs thorough revisions. It is furthermore unclear how it is correctly implemented.
I disagree with the author's reply to my first review that it is not necessary to know the values of the parameters. Without the given values of

U_{geo}, f (and thereby the latitude angle ϕ), Ω, ν, ρ

the test case is irreproducible. These values need to be provided. In addition, it needs to be clarified that
(a) ν is constant (hidden information via the words 'without any closures', is this correct?) and serves the role of K(=ν) in Eqs. 14-16
(b) Eqs. (1)-(11) are irrelevant for the discussion
(c) the exact definition for the forcing terms r needs to be provided for u and v.
In order to arrive at the analytical solutions (17) and (18) of the Ekman spiral, it must be assumed that the motion vanishes at z = 0 and tends to the zonal geostrophic value $\vec{v} = \begin{pmatrix} U_{geo} \\ 0 \end{pmatrix}$ in the free atmosphere. In addition, the Ekman solution (17) and (18) is based on the equation set

$$0 = K\frac{\partial^2 u}{\partial z^2} + fv - \frac{1}{\rho}\frac{\partial p}{\partial x}$$

$$0 = K\frac{\partial^2 v}{\partial z^2} - fu - \frac{1}{\rho}\frac{\partial p}{\partial y}$$

When comparing this formulation to Eq. (14) identify exactly how the forcing term r represents the forcing from the Coriolis and pressure gradient term in the u and v equations (provide the equations for r_u and r_v).
Note that your definition of

$$\frac{dP}{dy} = U_{geo} f \rho$$ (page 7, line 14)

seems to have a sign error and might need to read $\frac{dP}{dy} = -U_{geo} f \rho$ if you imply a geostrophic balance.

8) The other problem with section 3.1 is the incorrect definition of γ (line 20) which needs to be

$$\gamma = \sqrt{\frac{f}{2K}} = \sqrt{\frac{2\Omega \sin\phi}{2K}} = \sqrt{\frac{\Omega \sin\phi}{\upsilon}}\,.$$

Only in the very special case of ϕ=π/2 (North Pole) is this equation identical to the definition of γ in the manuscript. However, this is not specified, and γ might be used in an incorrect way. In addition, the authors call the quantity γ 'Ekman depth'. Since the physical units of γ are m^{-1} this is inadequate (it is an inverse). The γ definition is then wrongly used in the definitions of z_{top} (line 22), t_{end} and dt. The physical units do not work out. Divisions by γ are needed instead of multiplications. The wrong use of γ also affects Figs. (1) and (2). The x-axis label Δ/γ must have units of m^2 in the current version (not dimensionless). Figs. 1 and 2 furthermore suggest that U_{geo} = 1 m/s was selected in practice. This is the necessary value to represent the upper error limit of 0.25 m/s along the scaled ε/U_{geo} y-axis in Figs. 1 and 2. Is this assumption correct? All these aspects need to be clarified.

9) Page 8, line 26: how do the 1000 time steps compare to the setting of t_{end} and dt?

10) Section 3.2: Point out that this is a dry test case. It looks as if the GABLS1 case only forces the *zonal* momentum (line 19). Also add the information about the constant Coriolis parameter f and the density ρ. Does the density vary with height and if yes, how? As in section 3.1, provide the exact forcing functions r_u, r_v and r_θ.
It seems clear how Eqs. (6)-(11) connect to Eqs. (14)-(16) (via the computation of K), but it is unclear how the surface flux equations (1)-(5) enter Eqs. (14)-(16). Provide this information.
Cuxart et al. (2006) presented their results after 9 hours (averaged over the 9th hour). You average the results over the 8th hour and compare to Cuxart et al. (2006). What is the reason for the discrepancy? Are the results converged enough to a

steady-state solution that the 8th and 9th hour time frames become comparable? Provide an explanation.

11) Page 10, line 25 and Figs. 6a,b: which time snapshot is shown? Add this information to the text and the figure caption.
The domain is 4000 m high, but only 1300 m are shown in Fig. 6? Why? How do the solutions compare in the upper domain?

Correction of typos and style:
Page 1, line 15: '… an SCM …'
Page 2, line 27: ' … built-in …'
Page 4, line 14: '.. description …'
Page 5, line 24 and page 7, line 25: '… its ….'
Page 6, line 17: '… spent …'
Page 7, line 10: should read ' … clean setup quantifies numerical errors explicitly and tests the …'
Page 8, line 2: Bring footnote into the main text
Page 8, line 3: '…shows the results of the errors at all levels and …'
Page 8, line 15: '… though…'
Page 8, line 28: '… arise in the solution …'
Page 9, line 1: '…and the computational performance …'
Page 10, line 1: '… parameterize …'
Page 10, line 6: … on the order of …'
Page 11, line 2: 'Fig. 5' needs to read Fig. 8
Page 11, line 8: '… presented a one-dimensional …'
Caption Fig. 1 and 2: Add the information that the errors are shown at t_{end} (the end of the simulation). Also add: the inset shows the errors for all time steps.
Fig. 3: symbol 'L' is undefined
Caption, Fig. 4: '… eighth hour …'. Do u and v stay constant above 275 m?