Review of the MWR manuscript MWR-D-15-0226

Title: Comparison of Terrain Following and Cut Cell Grids using a Non-Hydrostatic Model
Authors: James Shaw and Hilary Weller

Recommendation: Major revisions

Summary:
The paper uses a non-hydrostatic 2D (x-z) slice model to compare two terrain-following (TF) vertical coordinates (the BFT grid by Gal-Chen and Somerville (1975) and the SLEVE coordinate by Schär et a. (2002)) to the `cut cell' method via idealized test cases. This reveals the accuracy of the two modeling paradigms for tracer advection experiments with prescribed flow fields, a steady-state test in the presence of topography and nonlinear flows with orographically-induced gravity waves. The new aspects of the manuscript are that the TF and cut cell methods are compared in an identical model framework, and that a new advection test is proposed that prescribes the flow along sloping TF coordinates. This reveals the differences between TF and cut cell methods more objectively.
Another theme of the paper is that the comparison also involves different numerical methods.

In general, the results are interesting. It is found that the accuracy of the methods strongly depends on the alignment of the flow field with the model levels, and that the cut cell method can trigger a computational mode that is present with the vertical Lorenz staggering. In addition, it is interesting that the curl-free formulation of the non-hydrostatic model significantly improves the results of the steady-state test case. Even when using the TF coordinates this mimetic property diminishes the spurious vertical velocities that have been documented in various other publications for TF coordinates.

There are a couple of weaknesses in the paper that need to be addressed:

1) A sign error in the derivation of the new advection test case in section 4b. Maybe it is just a typo in the manuscript, but if this wrong formulation has indeed been used in the example results, they all need to be rerun and corrected (see the detailed information below).
2) Inconsistent sign convention for the stream function and the derived velocities. The sign is switched in sections 4a and 4b, which needs to be unified.
3) Model description in section 3: Since this paper not only compares the orography treatment but also the accuracy of some numerical methods it needs to provide more information about these numerical variants. E.g there is no information about the `centered linear' finite-volume scheme, not even a reference. Is the linear scheme 2nd-order accurate and the cubic scheme 4th-order accurate? In addition, it is unclear what method the nonlinear steady-state test at rest and the gravity wave test used (the cubic option?). Include a

short description of the linear scheme, the cubic scheme and the fourth-order scheme from Schär et a. (2002) since the manuscript emphasizes these aspects in combination with the orography treatment.
The description of the Hodge dual operator is incomprehensible in its current form and does not seem to be crucial for the assessment of the orography analysis. I suggest moving this discussion into an appendix and extending it somewhat to make it self-explanatory. Currently, it is very confusing to read about a semi-implicit formulation, and see the explicit Runge-Kutta time stepping algorithm in the next paragraph.

Detailed comments:

1) Line 21: The Scorer parameter and Froude number are mentioned in the abstract, but never again in the manuscript. Either remove this sentence from the abstract, or add the missing discussion why large Scorer parameters and Froude numbers are a challenge for the cut cell method. This is not obvious.
2) Line 33: another way of treating orography are 'immersed boundary methods' or 'embedded boundary methods'. Please add this information including a reference. -- done, cited Simon et al. 2012
3) Line 33, typo: ' ... approaches to representing ... ' -- done
4) Line 53/54: do you mean interpolations to z-levels in order to compute the pressure gradient? -- yes, rephrased this sentence to say so
5) Line 58/59: conflicting information: you highlight the variable spacing and then the fact that cell sizes remain almost constant -- removed the latter statement (*)
6) Line 67: provide some brief information about the 'several approaches'. A single sentence does not justify a paragraph. -- added description of each approach
7) Line 80: add '... artificially weak in the Eta model'. -- done
8) Line 102: be more explicit: '... of the Cartesian coordinate surface at the model level with transformed height z*'. Spell out explicitly that z varies between h and H and that z* varies between 0 and H. -- done
9) Line 106-111: This manuscript does not discuss or use pressure-based vertical coordinates at all, and the information about sigma and HTF is irrelevant. Furthermore, the sigma definition is only correct for models with zero pressure at the model top. Remove these lines. -- removed discussion of sigma and HTF
10) Line 124: It is not clear what you mean by 'unstructured grids'. The grids seem to be very structured. Clarify. -- changed to 'non-orthogonal grids' (**)
11) Section 3: It would be helpful to point out up front that you use the model in a 2D x-z slice configuration without Coriolis forces. -- added
12) Line 142: typo '... temperature, ...' -- done
13) Line 145: There is no 'Lorenz C grid staggering', the model uses the C-grid staggering in the horizontal and the Lorenz grid in the vertical. -- amended
14) Lines 145-160: This information is disconnected from the whole paper. Expand the explanations to make them self-explanatory and move the paragraph into an appendix. Clarify that this treatment is only needed for nonlinear flows and that the advection uses an explicit time-stepping scheme. -- description expanded and moved to appendix

(*) since cell sizes are not constant even with constant layer spacing, especially over steep terrain
(**) I hope this clarifies that the issue is the treatment of non-orthogonality: metric terms or the grid

Yes, now says, '... where the tracer density, phi, ...'

15) Eq. (7): The tracer symbol ϕ is undefined from a physical viewpoint. Do you imply the tracer density? Later, in Eqs. (8) and (12) symbol φ or a mix of ϕ and φ are used. Unify the presentation. -- \varphi removed, now using \phi everywhere

16) Section 3: Add information about the different numerical options that are assessed in combination with the orography treatment. -- clearer description of models, including advection schemes

17) Line 189: the term 2a needs be in parenthesis -- done

18) Line 194: be more precise: '... constant wind above z_2 ... -- done

19) Line 198 and Eq. (16): Two different sign conventions for the stream function are used with flipped signs that either lead to a negative (line 198) or positive (Eq. (16)) sign in the definition of the zonal velocity u. Unify the presentation. -- line 198 should have been 'u=dPsi/dz', corrected

20) Line 199: rephrase 'A tracer with density φ is .... ' -- done
Why do you highlight a tracer 'anomaly' in lines 202, 203 and 204? Is there a background tracer distribution? If not, rephrase the 'anomaly'.changed 'anomaly' to 'tracer'

21) Line 211: The information about different numerical schemes is very sudden here without enough information. Add a description and references for all numerical options in section 2. -- moved to section 3, more details of schemes added

22) Line 223: typo : artifact -- done

23) Eq. 14: It is highly unusual to use a superscript (instead of a subscript) for the error norm -- changed to subscript everywhere

24) Line 247: The result that the increase of the model domain from 300 to 301 km leads to a 50% increased in the l_2 error seems to be highly questionable. This needs to be double-checked and explained in greater detail.

25) Eq. (16): see comment 19 concerning the different sign conventions and unify the presentation. In addition, the formulation for w is wrong and has a sign error. The vertical velocity w needs to be multiplied with (-1) to correct it in its current form. In case you used the wrong formulation for the results, they all need to be rerun.-- sign error in w equation fixed, code was already correct (**)

26) Line 281: '... Courant number ...' -- done

27) Section 3d: what is the grid spacing for this test case? -- dx=500m,dz=300m (***)

28) Line 344: The physical unit of μ is missing. -- dimensionless, see (*)

29) Line 414: '... Charney-Phillips ...' -- done

30) line 414: I assume that 'eta' need to be capitalized here, correct? -- yes, is now 'Meso-Eta'

31) Line 428: reference is incomplete -- now includes type, number and address

32) Line 443: ' ... Eta model ...' -- done

33) Line 466: '... Cartesian ...' -- done

34) Table 1: instead of showing these strange numbers like 13.6, -623 and 3480 it would be more helpful to say 'unstable'. -- done

35) Figure 3: Replot this figure. The lines need to be thicker, e.g. the SLEVE line is almost invisible. -- done, and made the SLEVE colour darker to improve contrast

36) Figure 5: The BFT θ line needs to be thicker.
-- done, and changed to black for consistency with figure 3

Also fixed capitalisation of 'Euler' in references

(*) mu is dimensionless. it is now included in the the momentum eqn 4a and noted that it is only used in the gravity waves test. we also note that mu=0 for the resting atmosphere test in section 4c

(**) github.com/hertzsprung/AtmosFOAM/blob/master/src/orography/SchaerCosVelocityProfile.C#L26

(***) another reviewer suggested further tests with other values of dz; these are also specified in the manuscript