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Interactive comment on "Adaptive Cartesian Meshes for Atmospheric Single-Column Models, a study using Basilisk 18-02-16" *by* J. Antoon van Hooft et al.

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Response to Reviewer #3

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The authors thank the reviewer for taking his/her time to comment on the manuscript. We hope to be able to address all points brought forward by the the reviewer in a point-by-point response in this document accompanied with relevant changes to the manuscript. A PDF highlighting all changes is also provided as a supplement.

The manuscript describes a prototype Single-Column Model (SCM) that employs dynamic grid adaptation in the vertical direction. The adaptations are guided by error estimators which increase the resolution where needed for accuracy, and remove grid points if additional accuracy is not needed. The method is tested in the atmospheric boundary layer based on the two GABLS intercomparison cases. In particular, the GABLS cases test the fine-scale diurnal variations of the planetary boundary layer (PBL). In general, the PBL structures in the two test cases are reasonably well captured in the adaptive SCM. The authors conclude that the adaptive-grid algorithm is able to dynamically coarsen and refine the numerical grid whilst maintaining an accurate solution.

Overall, the manuscript is interesting and well written, but the application area is rather narrow and it is difficult to judge the performance of the model based on two test cases. Therefore, it is difficult to draw more general conclusions concerning 3D General Circulation Models (GCMs). However, this is not the focus of this manuscript, which only addresses a prototype 1D adaptive model. The main criticism is that the description of the adaptive method is rather short which makes it difficult to understand the methodology even at a fundamental level. The main methodology is described in Van Hooft et al. (Boundary-Layer Meteorol., 2018) and the reader is referred to this paper. I recommend expanding the description of the Adaptive Mesh Refinement (AMR) algorithm somewhat in this paper, especially with respect to the error estimation technique. This will help make this a stand-alone paper. In addition, this review lists some clarifying questions that need to be addressed in a revised version.

Leaving out the the details of the error estimation technique was a choice that was made to prevent repetition of material that is published elsewhere. The referenced work in Bound.-Lay. Meteorol. (BLM) can be easily found, is freely available for everyone (open access CC 4.0) and in addition to that the work is also hosted via a mirror website at the HAL repository (https://hal.archives-ouvertes.fr/hal-01689036). This provides confidence that an interested reader will be able to find the more

detailed information if they wish so. The authors argue that repeating the 3-page story is therefore not necessary and does not really add to the proof-of-principle we aim to illustrate here. In the original manuscript we have opted to briefly describe the algorithm on a more conceptual level. In hindsight we agree that this may not suffice as it is indeed the key ingredient of the method. Therefore, based on the reviewers suggestion; a serious effort was made to include a didactical example based on an extension of the analysis of the laminar Ekman-spiral case that was formerly in the Appendix (in Sect 3.1). Now the manuscript includes a more detailed analysis of the usage of the error estimation technique. We feel that this is a valuable complementary example of the aforementioned work in the BLM paper, and not a repetition.

Specific comments:

Page 2, line 23: In which way does your work depart from the work by Van Hooft et al. (2018)?

Based on the point brought forward by the reviewer, we realize that we have not chosen our words careful enough here. The new manuscript is revised accordingly.

Page 3, line 17: Define the Richardson number. There are many variants, so please provide the equation.

The revised manuscript is now explicit on the used closures for turbulent transport and states all the relevant definitions.

Page 4, line 12 onwards: Clarifying questions concerning the refinements:
1) Is the grid only refined if both error criteria are fulfilled, or is it enough if one error indicator is flagged?
2) How often do you adapt (e.g. every time step)?
3) How are newly-created grid points initialized, and how are coarsened grid points merged?
4) Is the initialization (interpolation, merging) algorithm mass-conserving with respect to dry air mass & water mass and/or energy conserving?
5) Does the interpolation/merging technique observe the hydrostatic balance? If not, one might expect a lot of gravity waves in 3D versions of this algorithm.
6) Do you interpolate with respect to a height or pressure coordinate in the vertical direction?
7) What is the order of the interpolation technique? If linear, are oscillation-free high-orderinterpolation techniques available?
8) How many ghost cells are used?
9) Typical GCMs work with stretched grids and not equidistant grids. Can your algorithm be applied to stretched grids? The algorithm seems to rely on the fact that the grid spacing differs by exactly a factor of 2 (also refers to Fig. 2).
10) Have other error estimators (variables) and error thresholds been tried? If yes, comment on the pros and cons of these alternative choices.

The following answers in black are added to the main text, the answers in blue are not in the revised manuscript as they are considered off-topic for the present work:

1) If the estimated error in one (or more) of the three (u, v, θ_v) fields exceeds the respective criteria, the corresponding gridcell is refined.

2) The algorithm assesses the fidelity in the representation of the numerical solution at each time step, this garantees that no big developments in the solution take place in between grid adaptations. Noting that, courtesy of the tree-grid data structure, it is computationally cheap to do the assessment and refinement/coarsening compared to doing the time integration (i.e. typically less than 10% of the effort, for the presented

cases).

3) For refinement a bilinear interpolation technique is used whose second-order accuracy is consistent with the used solver. For coarsening, two cells can be merged into one by taking their average value which is exact for our finite-volume formulation.

4) The bilinear interpolation technique (for refinement) that is used in this study is not conserving for the first order moments of a scalar field, and not for higher order moments. However, the error introduced by this refinement step is directly controlled by the refinement criteria and can hence be tuned to any desired accuracy. Noting again that the second-order accuracy is inline with the solver's accuracy and hence is consistent with the overall method

5) Yes, see e.g. the 3D studies of van Hooft et al. (2018), or a more clean example online via the link: http://basilisk.fr/sandbox/Antoonvh/internalwavesAMR.c

6) No, In the model, height above the surface is used for this purpose instead

7) We use second order accurate bilinear formulation (using a 2-point stencil). Note that a conservative, linear interpolation technique based on a 3-point stencil is available and has an accuracy of the 3-rd order. Recent work of Radjarshi Roy Chowdhury does enable higher-order (of 3-rd, 4-th and 5-th order) methods that are non oscillatory. See an example test of his work that is based on so-called WENO schemes online: http://basilisk.fr/sandbox/rajarshi/WENO_CODES/weno_prolongation_scaling1D.c.

8) Two ghost cells are defined foreach resolution boundary and one at each "end" of the domain. This means that that in theory, for a worst-case-scenario, there may be as many ghost-cells as there are "solution grid cells". This is not really a concern since the values of the ghost cells are relatively cheap to calculate and only depend on the values of the "solution grid cells" that are solved for when time integrating the equations (typically $\mathcal{O}(10\%)$). Furthermore, figure 3 shows that the solver is well behaved.

9) The underlying (local) grid structure is Cartesian, and therefore any relevant mapping may be applied/implemented. (see e.g. http://basilisk.fr/src/README). However, the factor of two in the grid resolution between levels of refinement levels is an intrinsic

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property of the tree-grid structure we use. Our results show that in an ABL the scale separation can differs by two-orders of magnitude within the GABLS2 domain (8m vs 1024m res.), meaning that the factor of two is not really an issue. Alternatively, adaptive unstructured grids exist that do not have this limitation (e.g. the code by the name of "fluidity" http://fluidityproject.github.io/). However, a complete discussion of the pro's and con's of such an approach would entail a new study of its own and is a considered to be outside the scope of the present work. Finally, the reference work of Dunbar (2008) uses dynamical grid stretching. But the authors would not call such a dynamic approach to be truly adaptive (see text).

10) Yes, this was part of the tria l-and-error approach and the pro's and con's may be obvious from the analysis in the new Sect. 3.1. The values of the refinement criteria may be used to tune the balance between accuracy and the speed performance of the code. Furthermore, for cases that are more driven by e.g. cloud-top radiation etc. it would be sensible to also refine based on the estimated errors in the moisture fields and cloud fraction field. Similar to pre-tuning a static grid, the balance between accuracy and speed performance remains at the discretion of the model user. The authors feel that the adaptation algorithm provides a more user-friendly, consistent and mathematically-rigorous approach compared to pre-tuning a stretched grid. This is especially true when the results from a model run are not know beforehand. Yet we cannot provide a universal recipe for finding suitable refinement-criterion values and this is part of our continued research.

Page 4, Eqs. (4)-(6): Since all operators are 1D, I suggest using partial derivatives with respect to z instead of the Laplacian operator in Eqs. (4) – (6)
The equations are updated according to the reviewers suggestion

Page 4, line 29: The time steps are extremely short. Can the adaptive method also be applied to more usual physics time steps on the order of minutes to half an hour?
The time-integration scheme is only first-order accurate and this hampers the time stepping. Not the adaptive grid scheme as we see a similar deterioration in our results for the fixed-grid solution when the time stepping parameter is relaxed to larger values.

Additionally, time stepping is small because the fine-scale features that are resolved, courtesy of the $O(10m)$ resolution. We note that there would be no reason to assume that adaptive-grids and/or the Basilisk code cannot handle the style of time-stepping schemes of operational GCM's. We argue that the manuscript is clear on the fact that we do not aim to present a GCM but we rather focus on a possible avenue for it's gridding.

Page 5, line 10: the geostrophic flow cannot be described as a 'forcing' mechanism. A forcing term needs units of m/s^2 .

The reviewer is right, the text has been improved accordingly in the revised manuscript.

Page 5, line 19: is this the potential temperature or virtual potential temperature? Fig. 1 shows the potential temperature, Fig. 3 show the virtual potential temperature. Is this difference intentional (provide some reasoning)?

We simply follow the original intercomparison papers regarding GABLS1 and GABLS2 of Cuxart et al. (2006) and Svensson et al. (2011), respectively. We think their reasoning was that the GABLS1 case is dry and hence the concept of virtual potential temperature is not relevant over the its non-virtual counterpart. This is not the case for GABLS2, where in the original work of Svensson et al. it was chosen to intercompare the virtual potential temperature.

> **Page 5, line 17: should read 'physical closures for K'**
> **Page 5, line 23: should read 'turbulent transport coefficient'**
> **Page 5, line 28: should read 'on the order'**
> **Page 6, Fig. 3: explain the meaning of the gray shading**
> **Page 6, line 22: should read 'Fig. 5'**
> **Page 6, line 27, should read 'presented a one-dimensional'**
> **Page 7, line 15: explain acronym RANS**
> **Page 8, Appendix: what is the equation that is solved here?**
> **Page 8, line 10: provide the values for gamma, U geo , the Coriolis parameter f, and the density rho**
> **Page 8, line 19: Start sentence with 'The adaptive' instead of 'Were'.**
> **Page 10, line 32: Update the Van Hooft et al. reference**
> **Page 14, Table 1: State 'Number of time steps' instead of 'Solver timesteps'**
> **Page 14, caption of Fig. A1: explain that the slope of the dashed line shows the second-order accuracy**

We thank the reviewer again for his/her carefull reading of the manuscript and hope to have adressed all these points at their corresponding locations in the revised manuscript.

Except that we have not added the numerical values for U_{geo}, Ω etc... In stead, based on the reviewers comment, we present the results now in a properly scaled frame work. Making the results universal, as is allowed by the fact that the (scaled) analytical Ekman solution is not a function of the dimensionless ratio: $\Pi = \frac{U_{geo}\gamma}{\nu}$.