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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.

**J. Antoon van Hooft et al.**

j.a.vanhooft@tudelft.nl

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Discussion paper

# Response to Reviewer #1

Antoon van Hooft, Stéphane Popinet, and Bas van de Wiel

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The authors thank the reviewer for taking his/her time to comment on the manuscript.

**The article describes tests of an adaptive grid scheme in a single-column model for two ABL cases. The results are compared with a fixed-resolution version of the same model, and with various other models. This is an interesting study and the results are clearly presented. The quality of the adaptive scheme solutions is encouraging. While the overall scope of the research is limited, it presents a possible avenue for future adaptive GCM development. There are a few places where more detail and clarification would be helpful (described below). I recommend acceptance of this article pending minor revisions.**

The authors agree with most points brought forward by the reviewer and we have therefore revised the manuscript accordingly. We hope that the manuscript is now more clear, and a point-by-point response is presented below. Also a PDF highlighting all the changes that were made with respect to the original manuscript is available.



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Discussion paper

**In the model overview, it is stated that the grid refinement criteria are tuned based on trial and error. How sensitive is the scheme performance to the tuning? If this type of adaptive scheme is implemented in a full GCM, will different tunings be necessary at different heights, geographical regions, or seasons?**

It is the authors opinion that a complete discussion on the meaning, interpretation and selection of the refinement criterion warrants a study of its own. This is in fact part of our continued research, and hence is considered to be outside the scope of the present work. However, we agree that it is an important part of the grid adaptation algorithm that forms the basis of the present work. Therefore we have revised the manuscript considerably and have extended the analysis that was formerly in the appendix and moved it to the main text. Here we share our views on the usage of the criterion and argue that is provides a convenient and consistent framework for finding a balance between accuracy and performance. The new figures also provide quantitative results on this topic. How to translate these results obtained for the Ekman-spiral case to an SCM/GCM is still not obvious.

Furthermore, at this moment we cannot give a conclusive answer to the question regarding a variable refinement criterion. The authors do not see a good reason to employ a variable refinement criterion in time and space. Such an approach would mean that similar processes would be resolved with different accuracy depending on their localization in space and time. Our (current) philosophy is that for consistent meshing a pre-defined wish for accuracy can and should dictate the mesh's resolution depending of the resolved physics. On the otherhand, the physical processes that are dominant for the statistics of interest may change, depending on the time and location. Then, in practice, the accuracy requirements may vary and this could be refelected in the refinement criterion.

**p.2 Sec. 1, Line 17: Sentence starting "However, it is important..." is unclear and needs to be rewritten.**

We hope the section on the concept of fractal scaling is more clear in the revised manuscript.

**p.2 Sec. 1 Line 23: Sentence starting "This work departs..." makes it sound like this work uses different methods than Van Hooft (2018), yet the next page Line 25 suggests the opposite. This sentence needs to be modified to make the meaning clearer.**

We hope the sentence in clear in the revised manuscript.

**p.3 Sec. 2 Line 12: Could you state at least the nature of the surface fluxes parameter- ization (e.g. bulk flux). Which type of closures in Holtslag and Boville are you referring to?**

Based on the reviewers comment, we have added a detailed description of the used closures in section 2 of the revised manuscript.

**p.56 Sec. 3.2 : Could you add a little more qualitative description of this GABLS case? Were there clouds? Is it a surface driven convective BL? Is the wind shear significant or important?**

We have added a short overview of general conditions as they are modelled by the GABLS2 scenario. (lines )

**p.5 Sec. 3 Line 12: It would be good to reiterate here that each 'level of refinement' halves the local grid spacing.**

We have re-iterated that in the revised manuscript.

**p.5 Sec. 3 Line 21: Are you saying the differences are only minor compared the the LES spread, or only minor compared to the SCM model spread?**

We have revised the sentence to be more clear. We hope to convey the message that our SCM results are relatively close to the LES-ensemble results compared to the SCM results presented by the participants pf the original GABLS1 SCM intercomparison. Taking the LES as the 'truth': Notice that the fidelity in our results is due to the good performing description of the mixing closure by England and McNider (1995) (eq. 1 / 5 and 10 , original/revised), for this particular case.

**Figure 3a: Unlike most models, thetav in your solution has a negative slope in the boundary layer, more negative even than the one other plotted model with a negative slope. Is this slope also consistent with your fixed-grid solution? Is there something atypical about your SCM physics that would allow this?**

Yes, this is a known feature of the to the used eddy-viscosity closure. Using this local

description for turbulent transport, a gradient is always "needed" for vertical mixing. This is not a realistic feature and better closures that account for counter-gradient transport are available. We have added a remark on this feature in the revised manuscript. Next we show that this gradient is indeed a formulation-specific feature. Therefore, we re-plot our results alongside results from runs with a different value for the maximum mixing length ($l_{bl}$, see Eqs. 7 and 8 in the revised manuscript). The results are presented in the figure below (or see attached figure in case it that now appear in this document). The default value for $l_{bl}$ was suggested in the reference literature and the used closures for transport are not the topic of our study. The figure also shows that the slope is not due to the usage of the grid adaptation algorithm (as was suggested by reviewer #2). In fact, the results for $\theta_v$ are within $0.2K$, which is much smaller than the difference with other models (up to multiple Kelvins). The used scripts to obtained these results or available and presented online (www.basilisk.fr/sandbox/Antoonvh/GABLS2forrev1and2.c). We choose not to add this analysis to the main text as it does not really add something new to the results or change the overall analysis of the manuscript. As all reviewers already agree: the results are encouraging.

figure-1.pdf

**Figure 3c: What is the difference between the various shades of gray in the figure?**

They are associated with different measurement techniques (see Svensson et al. (2011). Based on the reviewers comment we have stated this in the caption of this figure in the revised manuscript. Note that the forcings of for the GABLS2 case are idealized, and hence accuracy with respect to the measurements is not necessarily to be expected.

**Figure 5 should have the simulation dates somewhere on the x-axis or at least the time coordinates referenced to a date in the caption.**

We have added the date of the observation that inspired this (idealized) test case in

the caption.

**p. 6 Sec.3.2 Line 20: Stating that the evolution of the wind speed profile 'is the same' suggests that it is identical which is inaccurate. Perhaps 'is nearly the same'.**

The reviewer is right and the sentence is revised accordingly in the new manuscript.

**p. 6 Sec.3.2 Line 22: Could you clarify what is meant by 'Stullian image'? It would be helpful in this discussion if you qualitatively describe which parts of the diurnal cycle require the most/least refinement.**

Since the conceptual evolution of a diurnal cycle of the ABL as presented in Stull (1991) (His fig. 1.7) is such a well known image, we hope to coin the term "Stullian image". This was done with permission of Roland Stull (private communication). The manuscript aims to describe qualitatively what parts of the ABL required grid refinement. This was infact the goal of bringing up the similarities with the image of Stull. Therefore, we have chosen to keep that discussion 'as is'.

**Technical corrections:**
**p.1 Sec. 1 Line 3: 'receives' should be 'receive'**
**p.2 Sec.2 Line 18: 'have' should be 'has'**
**p.4 Sec.2 Line 10: 'trail' should be 'trial'**
**p.4 Sec.2 Line 28: is 'that' referring to equation 6?**
**p.5 Sec.2 Line 33: Would be clearer as 'Online links are provied in table 1.'**
**p.5 Sec.3 Line 23: 'Noting' should be 'Note'**
**p.6 Sec.3.2 Line 17: '22-th' should be '22nd'**
**p.6 Sec.3.2 Line 22: 'Fig. 2' should be 'Fig. 5'**
**p.7 Sec.4 Line 3: 'efficient' misspelled**
**Appendix: Line 13: Sentence starting 'Using a domain...' is not a complete sentence.**
**Appendix: Line 18: 'facilitate' misspelled**
**Appendix: Line 18: would be clearer as 'we diagnose the number of used cells...'**
**Appendix: Line 19: Sentence starting 'Were the adaptive grid results....' is not a com- plete sentence.**
**Appendix: Line 22: Should be 'This plot reveals' or 'These plots reveal'**

We thank the reviewer again for his/her careful reading of the manuscript and hope to have corrected the manuscript according to the suggested technical changes. Except for the suggestion that 'online' refers to the 'link (to)' rather than the location of the case set-up files.