Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-21-AC3, 2018

Geoscientific
Model Development
Discussions

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

Received and published: 21 June 2018

Printer-friendly version

Discussion paper

Response to Reviewer #2

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

June 2018

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

A Basilisk 18-02-16 based adaptive grid scheme is employed and compared with an equal-distant high vertical resolution grid scheme in the same single-column atmospheric model for two land atmospheric boundary layer case studies. The diurnal variations of fine vertical structure near the bottom and the top of boundary layer is well captured using the adaptive grid scheme. Results are encouraging and clearly presented, which shows potential for future applications in global climate models. However the following major concerns are suggested to be addressed before acceptance for publication:

We are happy the reviewer find the results encouraging and hope to address the concerns in our point by point response and in the revised manuscript. A PDF highlighting the changes that were made is also provided as a supplement.

[1] In current state-of-art SCM/GCMs, more than 20s or more variables are involved in physical and chemical process simulations. It is necessary to state clearly the basic rule for selecting the refinement criteria and to show sensitivity test results. For example in this study, the refinement criteria are assigned only for winds and temperature. The specific humidity Q is also a key physical variable in the SCM simulation, but no criteria is assigned, why? and how a new Q refinement criteria influences the scheme ghost points and overall cell points searching? And how a Q refinement criteria influences the boundary layer diurnal cycle (particularly the boundary layer clouds) simulation?

The reviewer is right, finding a suitable mesh that strikes a balance between computational efficiency and accuracy of the diagnosed solution statistics is a challenge when performing numerical simulations. The challenge becomes even more prominent when statistics of over 20 variables need consideration. This is true for pre-tuned static anisotropic meshes, equidistant grids and we do not claim that the grid-adaptation algorithm lifts this burden from the model user either. Therefore, In the absence of a procedure for selecting mesh sizes for the static-grid approach (that typically also rely on trial-and-error, ad hoc testing and experience), the authors do not agree that the present (novel) approach should come with such guidelines. Especially when in practice, the grid (in)dependence of specific solution statistics is an arbitrary concept. More concretely: The SCMs in the original GABLS1 and GABLS2 intercomparison projects all use different meshes and similarly, a second (theorized) adaptive grid model could use different values for the refinement criteria to strike a different balance speed performance and numerical accuracy.

That said, the authors agree that the concept of the refinement criteria may raise new questions for modellers and currently warrants more study. Based on the reviewer's comments we made a serious effort to extend the analysis of the case that was formerly presented in the appendix (i.e. the laminar Ekman spiral), and we have included in it the main text. This new section (3.1), aims to exemplify for this simple case, what the effect is of tuning the refinement criterion, and argue that it provides a user-friendly, convenient and consistent framework for finding a balance between computational efficiency and accuracy. Note that it is not obvious how these results would translate to an SCM and at this moment in time, we feel that the "trial-and-error approach" is an accurate description of how the current refinement criteria values for the SCM were found.

The reviewer is also right that it would be wise to extend the algorithm to also consider the atmospheric moisture content when pushing the method towards more realistic/applied scenarios. Note that this can be readily done by changing a line of code in the case set-up. However, for the GABLS1 case there is no moisture and for the GABLS2 case, moisture only slightly modifies the buoyancy and the location of its inversion corresponds to that of the inversion in temperature. Therefore, the present cases are too simple/specific to be suitable for finding a refinement criterion for the moisture content field. It is argued in the manuscript text that at this early stage within our developments/research, the simplicity of the cases is an advantage.

[2] In this study, the Basilisk 18-02-16 based adaptive grid scheme uses much shorter time-step (between “2 and 15 s” in page 4 line 29) than that of current state of art GCM/SCMs (which is around 10 to 20 minutes and vertical resolution is in the order of at least 100m). Considering Both radiation and vertical diffusion calculation is time consuming, using such a small time step will need much longer computing time. Is it possible to use normal time step of 10-20 minutes for the scheme? If yes, please add new time-step simulation results in Fig.1 to 5; if not, please discuss the limitations of the current adaptive scheme and propose a possible solution;

The reviewer is right to bring forward this ‘feature’ of the present model. The text mentions that the time integration method is (only) first order accurate (page 4) and this limits the maximum time step of the present model. Also, the fact that the used resolution is much higher than the typical ABL resolution of an operational GCM, the temporal variations in the numerical solution contain contributions of higher frequencies, which warrant a reduced time step. The authors feel that the original manuscript is clear and explicit on the fact that the present model is not an operational GCM. Rather, the the work aims reports on a possible avenue for an adaptive gridding strategy, that is also compatible with higher-order time-integration methods (see e.g. in the work of Rajarshi Roy Chowdhury online: http://basilisk.fr/sandbox/rajarshi/AllMach_O4/allmach_weno_poisson4_rk4.h). Therefore, the authors see no reason to expect that the adaptive grid strategy would be incompatible with the time integration strategies as they are currently used in GCM's.

Furthermore, when considering a process such as radiation for which it may be relatively expensive to calculate the corresponding tendency term but that is relatively slow in its evolution. The code also allows to only evaluate these tendencies every so often whilst the grid is able to adapt at intermediate time steps. This is exemplified here: www.basilisk.fr/sandbox/Antoonvh/smoke.c, and the results are published in earlier work of the present authors (Van Hooft et al. , 2018).

[3] In Fig. 3, the adaptive grid scheme simulated a slightly unstable (negative) virtual potential temperature profile above 100m while all other models simulate slightly stable (positive) profiles. Is it due to the adaptive-grid scheme or the short-tail stability function used in the model or the Q profile difference,...? It is suggested to also add the fixed- resolution grid scheme results for comparison;

The unstable profile is the results of the used K-closure. All of the other models from the intercomparison use a more recent (read: better) closure for their description of vertical mixing under unstable conditions (see text sect 2.). We have added a notion to this in the results section of the revised manuscript.

We have added a figure below (if it does not display, please see the supplement) that shows that the slope is controlled by the details of the used closure for turbulent mixing (i.e. the maximum mixing length l, see Sect. 2 of the revised manuscript). Also results for the default mixing length obtained with using the adaptive-grid and equidistant-grid approach are presented. It appears that the difference between both runs is small (max. $0.2K$) and that the slope is virtually identical. We choose not to add there results to the revised manuscript as we feel that it does not add anything new to the present results or would change the analysis.

figure-1.pdf

[4] Moist process is important in atmospheric boundary layer variations (both in diurnal and synoptical scale), to exam the effects of adaptive grid scheme on overall PBL simulation, vertical profile comparison of scheme simulated specific humidity Q is suggested to be added in the previous Fig.1;

The reviewer is right that humidity plays an important role in the boundary dynamics. However, the GABLS1 case (corresponding to fig 1) does concern a dry boundary layer, and hence the results for the q profiles are not presented.

[5] Add diurnal cycle of observed and SCM simulated 2m temperature inter-comparison (similar like that of Fig.3 c for near surface wind speed) in Fig. 3 in order to better understand the adaptive grid scheme performance

The maximum resolution in this simulation is 8 meters and the temperature at the surface is prescribed by the case definition. Therefore, the suggested statistic is not expected to be very sensitive to the used grid structure, but rather be a test of the used interpolation strategy. Alltough it would be interesting to extend the analysis, in all, the authors are confident that the current set of results supports the message we aim to convey sufficiently.