Geosci. Model Dev. Discuss., 7, C2718–C2723, 2014
www.geosci-model-dev-discuss.net/7/C2718/2014/

Geoscientific Model Development Discussions

Interactive comment on "ASAM v2.7: a compressible atmospheric model with a Cartesian cut cell approach" *by* M. Jähn et al.

H. Weller (Editor)

h.weller@reading.ac.uk

Received and published: 22 December 2014

Only one of the previous reviewers was able to re-review the manuscript and so I provide a review myself.

Many thanks for extensive improvements to the manuscript. I am particularly keen on the rotating annulus test case. However I do not feel that the changes were in line with the previous set of editorial and reviewer's requests. Both reviewers previously suggested splitting the paper into a paper describing and analysing the dry dynamics and a separate paper on the parameterisations. The authors decided against this, which may be acceptable, but they must still demonstrate the efficacy of the dry dynamics of their model using standard, 2D test cases so that comparisons can be made with other models.

My impression is that the test cases were chosen to show off exciting dynamics rather than to rigorously assess the model accuracy (apart from the rotating annulus test case). The test cases are not obviously discriminating and the use of energy as a proxy for accuracy is not illuminating. You are still jumping to the more challenging test cases without carefully comparing your results of simple test cases with published results. The GMD web page on model description papers:

http://www.geoscientific-model-development.net/submission/manuscript_types.html

mentions "comparison to standard benchmarks, observations and/or other model output". We would really like to be able to see what advantages and disadvantages this model has over other models. I cannot gauge that from the cold bubble interacting with orography test case or the moist bubble interacting with a mid-air zeppelin because these have not been used by others. The test cases are too complicated and all you are demostrating is that your model is stable and that it does something that looks realistic. Please simulate gravity waves over orography in 2d using a dry atmosphere. Eg: fig 13 of Schar et al, MWR 2002 Vol 130, no 10, pages 2459-2480. There is only an analytic solution for linearised equations. But this test case has been very widely used and the temperature profile near the ground is sensitive to the representation of the orography.

I would so like to see results of the DRY warm bubble WITHOUT a mid-air zeppelin. This would enable comparison with other models.

Both reviewers previously asked for a more precise definition of the model, either references to existing work OR more detail in the paper. You have provided both. However are both really needed. The model description is now very long and my impression is that only a little of it is novel. Would a precise definition of the model still be possible if you relied on citations to other work rather than describing everything in this paper? Having said this, this is a GMD model description paper, so it does make some sense to define everything precisely and concisely in one place. So I do not insist that the standard material is dropped. But please consider each of the model description subsections and consider if it would be best to replace a subsection solely with citations.

I would say that it takes more than physical parameterisations to make a CFD code appropriate for large scale atmospheres (large scale is implied by the name "All scale"). Your plan is to make the model suitable for global scales by using a latitude-longitude grid. This sounds entirely unsuitable for modern massively parallel computers since convergence of the meridians towards the poles prevents good scaling of implicit and other long time-step methods. Consequently organisations like the Met Office are investing millions of Euros into re-writing their model to use a quasi-uniform grid. It seems unrealistic to call this an all scale atmospheric model. I would imagine that it would be suitable for meso- and micro-scales. Cut-cells also seem to be a suitable tool for these smaller scales. I would prefer if you could explicitly mention that aspects of this model development are not suitable for global scales using modern computer hardware.

Detailed comments

* The abstract mentions a "linear implicit Rosenbrock time integration scheme" to ensure "numerical stability around small cells". Presumably this scheme is also needed to ensure numerical stability in the presence of fast waves.

* On page 3 you cite Lock et al and Good et al. They only managed to get zero horizontal pressure gradient errors by assuming that the cell centre of a cut cell is at the centre of the original un-cut cell, which could be underneath the terrain (Lock, personal communication). When using a more accurate representation of the orography with cut cells, horizontal pressure gradients may well be small but they will not be zero.

* On page 3 you say that using a z-coordinate system improves prediction of clouds and rain and you cite Steppeler et al, 2006. If you look at Steppeler's more recent work. Eg fig 4 from:

http://www.geosci-model-dev.net/6/875/2013/gmd-6-875-2013.html

You will see that terrain following models can do very well if you get the terrain following discretisation right. I am not convinced that cut cells would improve forecasts in all cases. Only in cases where terrain following coordinates have been implemented badly. Please tone down your comments.

* Page 4: I would not say that the possibility of local refinement is an advantage of cut cell methods. I would say that cut cells makes local refinement an essential ingredient of a large scale model in order to get reasonable vertical resolution at the top of mountains. However the incorporation of cut cells does not in any way make the incorporation of local refinement easier.

* Page 4: You mention "spherical grid types". Why is this plural? Are you considering more than one spherical grid type?

* Page 4 sounds like a sales pitch for ASAM which seems inappropriate for a scientific article.

* Page 7: "logically orthogonal" does not make any sense. I assume that you mean "orthogonal, logically rectangular structured grid". The orthogonality is about geometry, not topology. The "logically rectangular" bit is about the topology.

* Section 2.2.3 about momentum is not clear. Why do you use 2 cell-centred values? Where do they both come from? How are they computed? Are you saying that each vector component is transported separately? What is "back interpolation"? When you say "a version with only cell centred velocity ...", do you mean a colocated version with cell centred velocity as the prognostic variable?

* I cannot understand section 2.2.4

* The paragraph at the bottom of page 11 is not clear. Have you created a new RK method or are you using the one from Skamarock et al, 2008?

* Page 13: Can you give a reference from the CG-like method.

* Page 14: You say "different types of split-explicit time integration methods are available". This information is not suitable for a paper. Just describe the time integration scheme used in this paper. The time schemes that may be implemented can be described in model documentation or a manual.

* It seems that section 2.4 is not relevant for this paper. It seems utterly routine, not used in this paper and not coded in your model. Could you remove it?

* At the beginning of section 3.1 you say that you use "mathematical methods". This seems too general. Surely the whole paper is about mathematical methods.

* At the beginning of section 3.1 you say that the purpose of LES is to reduce the computational simulation costs. I would not describe the primary purpose of LES to reduce simulation cost. Sure, it is a technique for simulating high Reynolds number flow, but if you were to rely on DNS, your simulations would not be more computationally expensive, they would be impossible.

* Page 16. You describe the modification of the standard Smagorinskly model to account for stratification. I think that this is not new. Please give a reference.

* There are no citations in section 3.4. Is this entirely your own, novel work? If so, it really needs more independent verification.

* In section 3.5 you describe two different surface flux schemes. Please only describe the one that you use. (You should only describe both if you are planning to do a comparison between the two).

* Page 27: what is your evidence for saying that 1e-3 % is an acceptable level of energy conservation?

* Page 27: 10ˆ8 does not sound negligible small for mass conservation. It is not machine precision if you are using double precision. Where do the errors come in? Are they related to solver tolerance? Do they accumulate over time?

* The annulus test case is terrific. Really challenging for cut cells. Could you also show convergence of the infinity error norm to check the affect of the cut cells on accuracy and maximum and minumum values of the tracer to confirm boundedness.

* In section 4.4 you describe comparisons with COSMO. This is only useful if you can say which is more accurate, COSMO or ASAM? Cut cells or terrain following?

* The contours in figure 16 are very noisy. Why is this? Does COSMO also have such noisy results? Is this a result of the parameterisations, the numerics or the cut cells?

* Section 5 does not contain any useful conclusions or interesting ideas for future work. It sounds more like an advertisement for ASAM. I am not entirely convinced that your conclusions are backed up by your work. For example, you say:

"The model produces good results for typical benchmark test cases from the literature with respect to energy conservation and model accuracy when it comes to interaction with the flow and scalar fields in the vicinity of cut cells."

I am not convinced that your results are good in comparison to other models.

* Part of your future work involves incorporating local refinement. This will mean that your technique for C-grid staggering will not work. My impression is that this model is particularly unsuitable for combining with local refinement.

Interactive comment on Geosci. Model Dev. Discuss., 7, 4463, 2014.