Title: ASAM v2.7: A compressible atmospheric model with a Cartesian cut cell approach
Author(s): M. Jähn et al.
MS No.: gmd-2014-110
MS Type: Model Description Paper
Iteration: Editor's review

Thank you very much for your review. This document provides both our answers to your general comments on the paper and point-by-point responses to your detailed comments.

The Blue color in this document indicates replaced or deleted text. In our revised manuscript, we marked all changes in red color for verifiability.

Kind regards,
Michael Jähn (on behalf of all co-authors)

General topical editor's comments

Your comment:

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 out- put". 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.

Our answer:

We added the original Schaer test case (2D dry flow over orography) and compare the two cases from the referenced paper (different Brunt-Vaisala frequencies, surface temperatures and inflow velocities). See new section 4.2.

Your comment:

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

Our answer:

One main criticism of the reviewers was that rising bubble test cases do not interact with cut cells. However, due to your suggestion, we added the original dry and warm bubble test case from Wicker and Skamarock (1998) with information about min/max values of the perturbation potential temperature and vertical velocity field (e.g. like in Bryan and Fritsch, 2002). The results are sensitive to the spatial discretization (3rd order vs 5th order advection). See new section 4.1.

Your comment:

Both reviewers previously asked for a more precise definition of the model, either refer- ences 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.

Our answer:

We have shortened some of the descriptions, which might be too detailed, e.g. for the soil model.

Your comment:

I would say that it takes more than physical parameterisations to make a CFD code ap- propriate 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.

Our answer:

For global applications with a lot-lon grid we only have to change the weights and include an additional curvature term. With an implicit time integration method we overcome the pole problem. Parallel implementation and scaling properties are not the main focus of the paper. We are aware of all that new activities.

See also our results at the Dynamical Core Model Intercomparison Project (DCMIP) 2012: https://www.earthsystemcog.org/projects/dcmip-2012/asam

Point-by-point respsonses to topical editor's comments

Hilary Weller (Topical Editor)

1)

a) **Referee comment:**
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.

b) **Our answer:**
This is true and we added this to the introduction.

c) **Changes in the manuscript:**
"A linear implicit Rosenbrock time integration scheme ensures numerical stability in the presence of fast sound waves and around small cells."

2)

a) **Referee comment:**
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.

b) **Our answer:**
Indeed our preferential approach for the pressure gradient calculation also does not take into account the cut cell geometry. See also the last paragraph of Sec. 2.2.3.

c) **Changes in the manuscript:**
"With this attempt one remains within the Cartesian grid and the numerical pressure derivative in the vicinity a structure is zero if the cut cell geometry is not taken into account, ..."

3)

a) **Referee comment:**
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.

b) **Our answer:**
Thank you for this additional reference. Of course, we do not claim cut cells improve

*weather forecasts in **all** cases, which would be naive and we are not even able to prove this. However, we would be careful with the comparisons made in the mentioned paper for the following reasons:*
i) They have the old LM version that leads to poor results compared to the other models and observed data.
ii) They have a 10 year old LMZ version with good results in vertical velocity and precipitation patterns. It shows a better localization of precipitation but has some problems with the amplitude.
iii) They have a state-of-the art model CLM with a physics scheme tuned on, error corrections and improvements in their precipitation scheme. Of course do these improvements affect the precipitation forecasts.
From our understanding, it is hard to draw conclusions out of these comparisons when the model physics differs significantly.
Going back to our introduction, we will cite this recent paper and reformulate part of the relevant sentence.

c) **Changes in the manuscript:**
„Using a z-coordinate system ~~also improves~~ can also improve the prediction of meteorological parameters like clouds and rainfall...“

4)

a) **Referee comment:**
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.

b) **Our answer:**
It is true that this is not an advantage. E.g. terrain-following models have finer resolutions near the ground by design. We changed this sentence.
Regarding cut cells and refinement: in ASAM we have a relatively simple stencil at a refinement interface that makes local refinement not that difficult.

c) **Changes in the manuscript:**
~~“An additional advantage of cut cell methods is the possibility of local mesh refinement around the orography to reach high near-ground resolutions.”~~ →
“To achieve reasonable vertical solutions near the ground, the usage of local mesh refinement techniques becomes interesting for large scale models.”

5)

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

b) **Our answer:**
We changed to singular.

c) **Changes in the manuscript:**
~~"... or the change to spherical grid types."~~ → "... or the change to a spherical grid type."

6)

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

b) **Our answer:**
We changed some odd furmulations.

c) **Changes in the manuscript:**
"... and has a lot of different options ..." → "... with different options ..."
"ASAM is a fully parallelized software using the Message Passing Interface ..." →
"Parallelization is realized by using the Message Passing Interface ..."

7)

a) **Referee comment:**
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.

b) **Our answer:**
We agree and changed this according to your formulation.

c) **Changes in the manuscript:**
~~"logically orthogonal grid"~~ → "orthogonal, logically rectangular structured grid"

8)

a) **Referee comment:**
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?

b) **Our answer:**
The prognostic variables are staggered in each case. In the second case we mentioned the centred variable is computed from neighboured staggered points whereas in the shifted case it is taken from both sides individually. See the paper of Hicken et al. (2005).

c) **Changes in the manuscript:**
~~"A version with only cell centered velocity components for advection and back interpolation has also been implemented but seems to be more diffusive."~~ →
"Another version with only three staggered velocity components has also been implemented for the given advection scheme but seems to be more diffusive."

9)

a) **Referee comment:**
I cannot understand section 2.2.4

b) **Our answer:**
We described this section in more detail and added Fig. 5 for clarity. Note that this section was added after the Anonymous Referee #2 (13 August 2014) requested an explanation regarding this issue.

c) **Changes in the manuscript:**
See rewritten section 2.2.4 in the manuscript.

10)

a) **Referee comment:**
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?

b) **Our answer:**
We have a constructed a new Rosenbrock method based on the RK3 method from Wicker and Skamarock 2002.

c) **Changes in the manuscript:**
"Moreover, a new Rosenbrock method was constructed ..."

11)

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

b) **Our answer:**
We added a reference (Dongarra et al., 1998).

c) **Changes in the manuscript:**
see b)

12)

a) **Referee comment:**
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.

b) **Our answer:**
We removed this paragraph.

c) **Changes in the manuscript:**
~~Furthermore, different types of split-explicit time integration methods are available, which are especially suitable for simulations without orography methods like large eddy simula- tions over flat water surfaces (Wensch et al., 2009; Knoth and Wensch, 2014).~~

13)

a) **Referee comment:**
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?

b) **Our answer:**
We removed this subsection and its references.

c) **Changes in the manuscript:**
Sec 2.4 removed.

14)

a) **Referee comment:**
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.

b) **Our answer:**
We removed this formulation.

c) **Changes in the manuscript:**
The set of coupled differential equations can be solved for a given flow problem ~~by using mathematical methods~~.

15)

a) **Referee comment:**
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.

b) **Our answer:**
We changed the sentence.

c) **Changes in the manuscript:**
~~"The main purpose for LES is to reduce the computational simulation costs. For that, it is necessary to characterize the unresolved motion."~~ →
"Within the technique of LES it is necessary to characterize the unresolved motion."

16)

a) **Referee comment:**
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.

b) **Our answer:**
We changed the sentence and gave a reference.

c) **Changes in the manuscript:**
~~"To take stratification effects into account, the standard Smagorinsky formulation is modified by changing the eddy viscosity to …"~~ →
"To take stratification effects into account, Lilly (1962) modified the standard Smagorinsky formulation by changing the eddy viscousity to …"

17)

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

b) **Our answer:**
Section 3.4 deals with the change of the density potential temperature due to surface energy fluxes. A straightforward derivation of the source terms starting with the first law of thermodynamics can be found in Appendix A, where citations are included. The final equation (A33) is also referenced.

c) **Changes in the manuscript:**
No changes.

18)

a) **Referee comment:**
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).

b) **Our answer:**
Some own tests showed that the WRF scheme produces more reasonable results regarding diurnal variations in transfer coefficients, Richardson numbers and

temperature changes. Thus, we changed the description where only this scheme is mentioned.

c) **Changes in the manuscript:**
"Two different surface flux schemes are implemented, following the revised Louis scheme as integrated in the COSMO model (Doms et al., 2011) and the revised flux scheme as used in the WRF model (Jiménez et al., 2012)." →
"The implemented surface flux scheme follows the description of Jiménez et al., 2012, which is the revised flux scheme used in the WRF model."

~~„As described in (Doms et al., 2011), the bulk transfer coefficients are defined as the product of the transfer coefficients under neutral conditions Cmn, h and the stability functions Fm, h depending on the Bulk-Richardson-Number *Ri*B and roughness length z0. Cm, h = Cmn, hFm, h (*Ri*B,z/z0) ."~~

~~„Test cases for validation indicate that the surface fluxes are better reproduced by Jiménez et al. (2012) than for Doms et al. (2011)."~~

19)

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

b) **Our answer:**
I quote Bryan and Fritsch (2002, p. 2922), where it is said that an energy error of 1e-4 % is "quite small" and considered as "high degree of accuracy". For the moist bubble case, they reach these values by not neglecting or simplifying terms in the thermodynamic formulation (e.g. the specific heat of liquid water), which is exactly our approach.
In the cold bubble case, where no moisture is present, we are talking about a "still acceptable" level of conservation, which is already toned down. We justify this by mentioning "very sharp gradients in potential temperature and wind speeds" resulting from the test case design.
Of course it would be nice to see what other models produce concerning this issue, but we are not aware of any data.

c) **Changes in the manuscript:**
No changes.

20)

a) **Referee comment:**
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?

b) **Our answer:**
This was a mistake from our side (we made the output in single precision). Checking the values again leads to an "error" of 1e-15, which is machine precision.

c) **Changes in the manuscript:**
~~"A check for total mass results in a relative error of 1e-6 % which is negligible small."~~
→
"Total mass is always conserved within machine precision."

21)

a) **Referee comment:**
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.

b) **Our answer:**
We added this data to Fig. 15 and Table 5. Note that the infinity error norm was already indirectly included in our previous version since we showed mix/max values of the tracer differnece fields.

c) **Changes in the manuscript:**
See b).

22)

a) **Referee comment:**
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?

b) **Our answer:**
The motivation for using this test case was the following:
i) With now having five idealized 2D test cases, at least one 3D test case should be included to the description to show that our cut cell implementation is also working for three dimensions.
ii) Most of the physical parameterizations described before are necessary and therefore used for this test case and have not been used for the other cases.
We agree that these comparisons are rather qualitatively, but the results appear very similar to the COSMO simulations.

c) **Changes in the manuscript:**
No changes.

23)

a) **Referee comment:**
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.

b) **Our answer:**
We think that this could be an issue with our visualisation software (references are given in the section Code availability and visualization), where the values between grid points for the contour plots are only linearly interpolated. There is no option within the software to change the interpolation method. We also see this kind of noisy results at simulations without cut cells. For comparison to the COSMO results see Fig. 4b) in Kunz and Wassermann (2011).

c) **Changes in the manuscript:**
No changes.

24)

a) **Referee comment:**
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.

b) **Our answer:**
We added some additional conclusions based on the description (possibility of large time step due to implicit Rosenbrock method and of simulating flow around steep slopes around orography and buildings accurately). We also reformulated the above sentence you cited to be more precise. We did not intend to let this section sound like an advertisement. We hope that with adding the additional test cases give more room for comparisons and more convincing results.

c) **Changes in the manuscript:**
See Sec. 5 in the manuscript.

25)

a) **Referee comment:**
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.

b) **Our answer:**
We aware with problems concerning wave reflection at abrupt grid size changes. However at least for grid refinenment in the horizontal directions we have not encountered any problems. This is in agreement with the statement in Ullrich and Jablonowski (2011): "The inherent dissipation in upwind finite-volume schemes may be a natural and efficient way to control spurious wave reflection on non-uniform grids."

P.A. Ullrich, C. Jablonowski, An analysis of finite-volume methods for geophysical problems on refined grids, J. Comput. Phys. 230 (2011) 706–725.

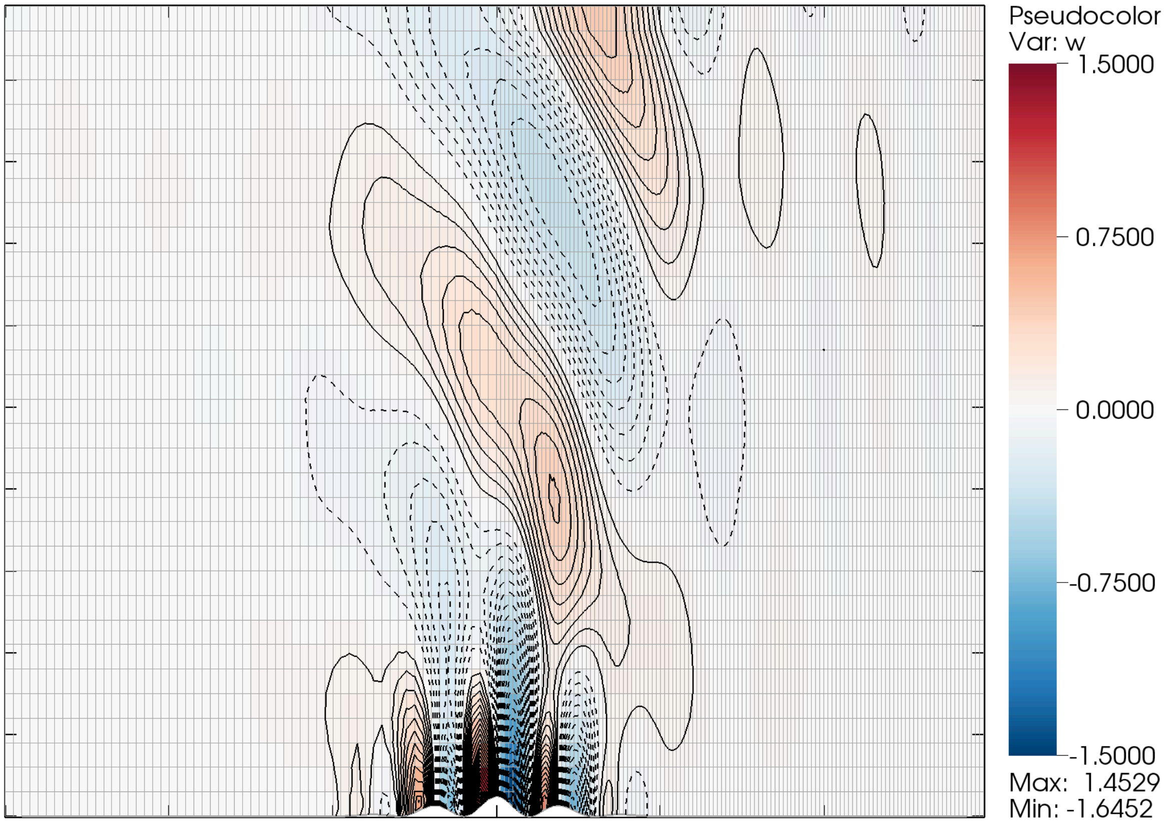

Fig. 1: Steady state solution of vertical velocity field of the Schaer et al. (2002) test case with a refined grid. Horizontal grid resolution is doubled at *x* > 200 km. Contour interval 0.05 m s^{-1}.

c) **Changes in the manuscript:**
No changes.

Luca Bonaventura (Topical Editor)

1)

a) **Referee comment:**
I am satisfied with the revisions. I would only suggest that figure 16 is replaced by two separate plots for the wet/dry case, since overlapping contours in the figure 16 of the present version are not very clear.

b) **Our answer:**
We agree that this plot version seems to look a bit confusing. However, in Kunz and Wassermann (2011, Fig. 4b) the same plot style is used. For comparative reasons, we kept to this. Note that also the other plots for this test case have the same style like in the cited paper. Also, in this particular figure, you can see the shift in gravity wave structure for the wet and dry case, which might be a bit harder to figure out when having separate plots.

c) **Changes in the manuscript:**
No changes.