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

Anonymous Referee #2

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General remarks

This manuscript presents a description of the fully compressible non-hydrostatic cut cell atmospheric model ASAM 2.7 and results for some benchmark problems. The use of cut-cell methods for resolving steep terrain is an exciting area of high resolution modelling, and so development of models capable of doing this is certainly research worthy of publication. My fundamental problem with this manuscript is that the aims are not clear and as a result it is not very coherent or convincing. The manuscript tries to cover both the dynamical core and all the model physics, but doesn't really use or test much of this functionality. I include some more specific comments below, but my overall conclusion is that this manuscript could not be published without substantial revisions. I would recommend a much more focussed paper looking at the dynamics of the model

and really testing the cut cell implementation. If the cut cells are the novel feature then focus on this and choose benchmark cases which actually test aspect of the model. The second stage would then be to test the various physics parametrisations with some proper benchmark cases / case studies where there is other model or observational data to compare with.

Specific comments

1. The introduction focusses on the cut-cell formulation of the model and the advantages of this for atmospheric research, however the review of existing cut cell models here is a bit patch and missed a number of relevant works including Adcroft et al (1997), Steppler et al (2006), Yamazaki and Satomura (2008, 2010, 2012) and Good et al (2014). No reference is included for the numerical schemes mentioned (Rosenbrock time integration).

2. The model presented is not a new model, but is a development of an existing model (used for CFD of flow round buildings) for atmospheric models. Given this, it is rather odd that there are no references for previous versions of the model or a clear statement of what is new here. There is just one mention of recent use of the model for urban environments.

3. You present the Euler equations in (1)-(3), but later on you include a LES sub-grid model so there should be some form of source term / Reynolds-stress term in (2). You also mention a "constant physical viscosity" in the cold bubble test? Where does this fit into the Euler equations?

4. I realise notation can be tricky in describing numerical schemes, but there is a problem throughout of using lots of notation, not all of which is clear and not all of which is defined. As an example on page 4468 you define FU_L as the area of the U face, but this is confusing with $F \times U$. Perhaps notation like F_{UR} would be clearer? Lower down the page you also talk about UF_L but don't say what this

is, nor do you explicitly define ϕ_L and V_C. I infer that the subscript means that variable on the L / R face or the cell centres to the L / R? There also appears to be dupliation of notation (e.g. ϕ is a scalar variable in the preceding sections then suddenly in equation (9) ϕ is the limiter. This whole section needs checking. Please make sure all variables are clearly defined throughout. Other examples include k_j, β_{ij} and γ_{ij} in equation (16)

5. How do your scheme for interpolating values onto the faces work near the boundaries where some adjacent cell values are not defined? How do you cope with faces which are only partial faces? Is interpolation from the cell values the most appropriate way of dealing with this? I found that your explanation didn't seem to really address how you handle the cut cells, which seems to be the critical bit of the whole model description.

6. I was confused by equation (13). It is discontinuous at $FU_L = FU_R$. I assume there is a mistake?

7. Given the focus on the cut cell capabilities of the model, it is rather important to see how the physics parameterisations such as the sub-grid scale model and the surface fluxes deal with this. Despite the detailed descriptions, there is relatively little detail or testing of this point. In particular, how does the interpolation in the cut cells affect the accuracy and conservative properties of the model? Some tests to prove this would be useful.

8. Overall I found the description of the model microphysics rather detailed. It appears to me that much of this is not particularly novel. I would suggest instead focussing the paper on properly testing the dry-dynamics cut cell aspects of the model. Some aspects (e.g. the surface fluxes and soil model) are not even used in the test cases presented here.

9. The test cases are useful, but I would question whether these are the most ap-

propriate test cases for a cut-cell model. The cold bubble is a common test case, but does not of itself test the cut-cells since the surface is flat. (Of course, this depends exactly where the surface is with respect to the grid, but there are no details given of this.) I found the comparison with the previous work rather superficial. There are no figures given from the original Straka et al paper, for direct comparison. Looking at this paper there seems to be some differences, with addition contours, despite the fact that the contour interval is 2K here (compared to 1K in the original paper). I would also like to see some values for maximum / minimum theta perturbations to compare with the original paper. This is also a useful test of the monotonicity of the scheme. The description of the setup mentions a fixed physical viscosity, however this is the first mention of this - it does not appear in the equation set given above. Where does the value come from? The original Straka paper used a fixed K, but here it appears that you have a turbulence model instead? Or is the turbulence model turned off in this case? If so why?

10. Moist bubble. Is equation (80) the perturbation in θ or θ_e - it's not clear. I assume this is only for $L \leq 1$? Again this problem does not test the cut cells at all. You might consider trying the moist bubble over a hill as done in Good et al (2014). With a cut cell model there should be negligible difference between bubble ascent with and without a hill. This is a useful sanity check, although still not a tough test of the cut cells. There is an additional test case with a uniform speed of $U = 20\,\text{ms}^{-1}$. How does this square with the (presumably) no-slip boundary conditions? I could find no proper discussion of the lower boundary conditions on velocity in the model. This is another important and tricky aspect to get right in a cut cell model so needs discussion.

11. The mountain wave case is a more useful standard test case for the cut cell model, however there was only a single paragraph presenting and discussion of this case with no real quantitative comparison with other studies. I would certainly

expand on this. Why not compare directly with the analytical solution in Schar et al?

12. The final test case over Barbados is a test in the sense that it will check the model runs with real terrain and does something sensible looking, but there are no observations, analytical solutions or equivalent simulations with other models to compare against. I would suggest leaving this test out of a preliminary paper, and making a more thorough microphysics test, comparing with observations or other models, the subject of an second paper. I have a couple of other questions pertinant to this test which need addressing too. The description says a stretched grid in the vertical is used. Does this mean the vertical resolution at the surface is less at altitude over the island? This is a problem for cut cell models and needs discussion. A test to look at the effect of this would be good (perhaps c.f. a terrain following model?) The initial profile used with constant N, a log wind up to 300m, with constant wind above, and a humidity inversion. This doesn't seem dynamically consistent. A plot of the profiles (particularly of humidity) would be useful. How long does the model take to reach a balanced state? Are the results you show in this state? Where do the specified values of z_0 come from? They seem very small over the ocean and quite large over land. Incidentally, this is the first mention of z_0 as far as I can see. How is it used in the model (see previous point about lower boundary conditions)?

Technical corrections

- p4466, line 6. Delete "used". item p4466, line 6-7. Not all of these are "mandatory". You can do LES of dry cases with no microphysics scheme.
- p4466, line 16. "island effects *over* the Caribbean island of Barbados."
- p4472, lie 3. "methods allow a simplified"

- p4472, line 5. How do you justify neglecting the extra term in this expansion? Explain.
- p4472, line 14, "from" not "form"
- p4474, line 4. I don't understand this sentence. Can you check the English and clarify what you mean.
- p4474, equation 29. Define the diagonal matrices, or at least say how the decomposition is done.
- p4474, line 11. The phrase "twice in dimension" doesn't make sense.
- p4475. Suddenly introduce u_i as velocity components, and also Reynolds averaging operations without definition.
- p4478. Equations (46)-(47). Define x_{min} and x_{max}.
- p4490, line 25. "perturbed by small"
- 4491, line 16. "leads to at least 3"
- p4492, line 17. "cloud coverage in RH80"

Additional references Adcroft A, Hill C, Marshall J. 1997. Representation of topography by shaved cells in a height coordinate ocean model. Monthly Weather Review 125: 2293-2315.

Good, B., Gadian, A., Lock, S.-J. and Ross, A. (2014), Performance of the cut-cell method of representing orography in idealized simulations. Atmosph. Sci. Lett., 15: 44-49.

Steppeler J, Bitzer HW, Janjic Z, Schättler U, Prohl P, Gjertsen U, Torrisi L, Parfinievicz J, Avgoustoglou E, Damrath U. 2006. Prediction of clouds and rain using a z-coordinate non-hydrostatic model. Monthly Weather Review 134: 3625-3643. Yamazaki H, Satomura T. 2008. Vertically combined shaved cell model in a z-coordinate nonhydrostatic atmospheric model. Atmospheric Science Letters 9: 171-175. Yamazaki H, Satomura T. 2010. Nonhydrostatic atmospheric modeling using a combined cartesian grid. Monthly Weather Review 138: 3932-3945.

Yamazaki H, Satomura T. 2012. Non-hydrostatic atmospheric cut cell model on a block-structured mesh. Atmospheric Science Letters 13: 29-35.

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