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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.

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The paper presents an interesting and novel numerical technique and represents a potentially valuable contribution for the high resolution numerical modelling community. However, the description of the numerical method does not include several critical points; while omitting on one hand an accurate description of the most original contribution of the paper, the authors devote on the other hand a large amount of space to material that is more standard or very well described in the literature. The validation of the shaved cell approach is quite limited and no accuracy or efficiency comparison is provided with the results obtained by other similar approaches presented in the literature (and ignored in the reference list). For these reasons, the paper can in my opinion be accepted for publication only after performing the major revisions that are listed in

detail below.

1) A general critical comment is that the paper seems to aim at the same time at testing a novel numerical technique AND documenting a model with full physics. Although the most common (and in my view best) practice is nowadays to discuss the backbone numerical core first, with more emphasis on the mathematical properties, and to apply it to more realistic problems once its pros and cons have been clearly assessed, the authors are perfectly justified in pursuing the more ambitious goal of achieving both aims at once. However, the paper in the present form is unbalanced, missing a lot of detail on the numerical part (as it will be discussed later). Given the premise of an insufficient description and validation of the fundamental numerical method, it does not make much sense to present realistic results with full physics. On the other hand, the appendices and the detailed description of the subgrid scale parameterizations are adequate for a full model presentation, but they are a bit off the point in a paper focused on a new numerical technique. The authors may consider splitting the paper in two and testing the adiabatic dynamics part first.

2) This is apparently the first attempt to describe properly the proposed shaved cell approach in the published literature (at least, no previous reference is given by the authors). Therefore, a full and detailed account of all the aspects of the method should be given. In particular, it should be clearly explained of how 'special' cell configurations are handled. The authors instead simply mention in the caption of figure 1 that some possible configurations are 'excluded', and classify such configuration by the number of 'markers' without defining this term, that is only used in the captions of figure 1. The authors should clearly describe the mesh pre-processing approach they employ, define all the relevant quantities and also explain in detail how the staggered control volumes for the momentum variables are defined, which is not at all obvious on this kind of meshes.

3) Several other important details concerning the numerical method are omitted. In particular, all the points below should be explicitly addressed in a revised version:

a) It is said that the method is a mixture of finite volume and finite difference approaches, but the model equations are in flux form and the only reference to finite difference approximation is the sentence 'The pressure gradient and the Buoyancy term are computed for all faces with standard difference and interpolation formulas with the grid sizes taken from the underlying Cartesian grid.' (at the end of section 2.2). However, if the 'shaved cell' structure is ignored when computing the pressure gradient, serious inaccuracies may result.It is up to the authors to prove that this is not the case, but they do not present any stringent accuracy assessment close to the lower boundary (see point 5). It is unclear why the pressure term is not included in the flux formulation, thus leading to a full finite volume formulation (for which however the issues discussed below in 3.b and 3.d would also be relevant). The authors should clarify this point and (possibly in future work) compare the results of the present formulation with that of a full finite volume approach, that should not be difficult to implement in their framework.

b) Nothing is said on the well balancing properties of the scheme and on the spurious velocities that may arise in an atmosphere at rest with a large mountain at the bottom; an explicit discussion of this point and a short description of the outcome of one such test should be included. Also related to this, the method described in the paper does not appear to require the use of a reference profile: for clarity, the authors might state explicitly if this is not the case.

c) From the sentence 'For each cell two cell-centered values of each of the three components of the cartesian velocity vector are computed and transported with the above advection scheme for a cell-centered scalar value.' (page 4470) it would appear that the proposed approach requires twice the computational effort than an approach based directly on staggered control volumes. The authors should clarify this point and, should this be really the case, justify this rather expensive choice with respect to more straightforward approaches based on the use of a staggered control volume. Furthermore, it is important to understand if and to which extent the flux limiters in the momentum equations are acting just to suppress some inaccuracy related to the proposed shaved

cell approach; the typical values of the flux limiter around the orography should be reported, to understand whether the method is mostly reverting to first order upwind or not.

4) As far as I know, one of the best known attempts to apply finite volume concepts to describe orography/topography in environmental models is

A. Adcroft, C. Hill, and J. Marshall, Representation of topography by shaved cells in a height coordinate ocean model, Monthly Weather Rev. 125, 2293 (1997).

The authors ignore this seminal paper in their reference list. Furthermore, in the paper by Adcroft et al a number of idealized cases for advection over orography/bathymetry are proposed, that would allow to assess the accuracy of the proposed method close to the bottom. The authors should consider performing one of such tests, in particular for the purpose of assessing which kind of accuracy is to be expected in the shaved cells, assuming that no theoretical argument to estimate the convergence order at the bottom is available. A method that reverts to first order in the lowermost cells could introduce excessive numerical diffusion in the lowest layers, thus making the proposed approach not extremely useful in practice. The authors should discuss this issue and present new results that clarify the properties of their method in this respect.

5) Since the main novelty of the proposed approach is the finite volume treatment of the orography, this technique should be tested in a much more systematic and quantitative way on idealized benchmarks, where its accuracy can be assessed much more clearly in comparison with similar or alternative approaches. In particular, besides trying an advection test as suggested in point 4, the following remarks should be addressed:

a) Concerning the cold bubble Straka test, the statement that 'These values and the contour field agree well with the results from the literature' is debatable at best. A number of different methods in recently published papers have been used to simulate this benchmark, I quote more or less at random

Klemp, Joseph B., William C. Skamarock, and Jimy Dudhia. "Conservative split-explicit time integration methods for the compressible nonhydrostatic equations." Monthly Weather Review 135.8 (2007): 2897-2913.

Giraldo, Francis X., Marco Restelli, and Matthias Läuter. "Semi-implicit formulations of the Navier-Stokes equations: application to nonhydrostatic atmospheric modeling." SIAM Journal on Scientific Computing 32.6 (2010): 3394-3425.

Norman, Matthew R., Ramachandran D. Nair, and Fredrick HM Semazzi. "A low communication and large time step explicit finite-volume solver for non-hydrostatic atmospheric dynamics." Journal of Computational Physics 230.4 (2011): 1567-1584.

In all these papers, different methods at different resolutions, either lower or higher than the one used by the authors, consistently give a front position at t=900 s that is on the left of the 15 km mark, while the solution in the present paper is well beyond that. Furthermore, a much larger spacing is employed between subsequent contour levels (2K rather than 1 K or even 0.25 K in the referenced papers). The authors should address this discrepancy and try to explain it, as well as replacing the plot with one using a contour spacing comparable to that used in the literature.

a) The only idealized test with orography concerns an orographic obstacle that does not go beyond the first model layer. This is hardly a tough test for a shaved cell method, in the sense that even rather inaccurate approaches may pass such a test. At least one lee wave test should be run in which a mountain profile is used that intersects several grid layers (several such tests are presented in the literature). Details of the flow around the obstacle should be analyzed and a quantitative comparison in terms of analytically predictable quantities (vertical momentum flux) should be presented.

b) If moist idealizd tests are to be included, at least one of them should concern lee waves with moisture, in order to compare with results like those presented e.g. in

Durran, Dale R., and Joseph B. Klemp. "A compressible model for the simulation of

moist mountain waves." Monthly Weather Review 111.12 (1983): 2341-2361.

Miglietta, M. M., and A. Buzzi. "A numerical study of moist stratified flow regimes over isolated topography." Quarterly Journal of the Royal Meteorological Society 130.600 (2004): 1749-1770.

Miglietta, M. M., and R. Rotunno. "Simulations of moist nearly neutral flow over a ridge." Journal of the atmospheric sciences 62.5 (2005): 1410-1427.

Also in this case, a mountain profile intersecting more than one grid layer should be used.

c) Concerning the more realistic test, I can hardly assess its meaningfulness until the previous issues concerning the numerical methods are cleared. However, should the authors want to include a more realistic test, I strongly recommend that they chose one on which other shaved cell approaches have already been applied, so as to allow for a comparison with alternative techniques.

6) Two different semi-implicit solvers are described in section 2.3. Firstly, it is not entirely clear whether the application of these methods to the Euler equations with gravity is a novel development of this paper or was proposed already in Jebens et al 2011. The authors should clarify this point. They should also make clear which of the approaches is used in the numerical tests and whether any significant difference in accuracy or performance is noticed between the two. The description of the two approaches is so intermingled that it is difficult for the reader to sort out what is actually done in each case. The authors should try to streamline the description of each variant. From the point of view of the linear solvers employed, it is unclear what the 'conjugate gradient (CG)-like methods' referred to on page 4473 exactly are (Bi-CGSTAB? GMR?) and what is an estimate of the resulting computational cost. The authors should provide e.g. some information on the average number of iterations as a function of the typical Courant numbers |c|*dt/dx and |u|*dt/dx, where c is the speed of sound and u the flow velocity. Finally, it would be interesting if the authors could comment on the possibility

of recovering within their framework the discretization of the pseudo-incompressible approximation of the Euler equations, as done e.g. in

T.Benacchio, W.P. O'Neill, R. Klein A blended soundproof-to-compressible numerical model for small to mesoscale atmospheric dynamics,Monthly Weather Review 2014 doi: http://dx.doi.org/10.1175/MWR-D-13-00384.1

Minor issues:

p. 4465 When the authors remark that 'To avoid instability problems around these small cells, the time integration scheme has to be adapted. For this, linear-implicit Rosenbrock time integration schemes are used in ASAM.', they should also observe that the same goal can be achieved by other means, such as e.g. by semi-implicit, semi-Lagrangian methods.

p. 4469: denoting the limiter and the advected quantity by the same symbol phi might be a bit misleading, a different symbol for the limiter could be used in formula 9

p. 4472: loosing –> losing

p. 4473: a reference for the Eisenstat trick might be useful for readers who are not experts in numerical linear algebra

p. 4486, introduction to section 4: in a 2D test the flow can hardly pass 'around' a mountain range. . .maybe this should be changed in 'above'

p. 4487: The statement 'Because of the fully compressible design in ASAM, mass conservation is always ensured' is not correct, since there are many fully compressible, but not mass conservative models, such as SI-SL models, see e.g. M. Tanguay,A. Robert, and R. Laprise, A semi-implicit, semi-Lagrangian fully compressible regional forecast model, Monthly Weather Rev. 118, 1970 (1990). Furthermore, when using semi-implicit time discretizations, the tolerance employed in the linear solver (see point 5) can have an impact on the effective conservation error. The authors should state explicitly which conservation errors were obtained at least in one of the idealized tests

in a closed box.

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