Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-159-AC1, 2017

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "An Adaptive Semi-Lagrangian Advection Model for Transport of Volcanic Emissions in the Atmosphere" *by* Elena Gerwing et al.

Elena Gerwing et al.

elena.gerwing@awi.de

Received and published: 29 August 2017

1. The biggest issue is the model initialization and forcing. It is said (page 7, line 6-8) that the initial concentration is derived by dividing the eruption rate by the injection volume. However, this results in a flux of ash mass per unit volume and time, not a concentration. This flux should have been maintained over the duration of the corresponding eruption phase. However, it seems that an initial perturbation/concentration was used instead. If this is true, then this is a major flaw of the paper. Only adding the ash emitted within one second underestimates the erupted ash mass by many orders of magnitude. Releasing all ash erupted during each phase (as listed in table 1)

instantaneously at the beginning of an eruption phase is equally unrealistic. This issue needs to be addressed, simulations repeated if needed before the manuscript can be published. Also, the total amount of ash should not depend on the choice of the grid. If the forced volume is different, then the ash flux into that volume should be (slightly) different to compensate.

The emitted ash per eruption rate was neither released instantaneously at the beginning of the eruption nor was only the ash amount emitted during one second introduced per time step. The calculated mass eruption rates in kg/s (listed in Table 1 in the manuscript) were divided by the injection volume and multiplied by the simulation time step, ending up in an ash concentration in kg/mˆ3. Since this process wasn't described in the manuscript, we will add a short explanation in order to avoid confusion. Since the injection volume was approximated better or worse by the adaptive mesh dependent on the refinement level, it is true that the total injected amount of ash slightly differs for the different grid resolutions. But these differences are quite small. In the grid configuration used for most of the simulations (an adaptive mesh with a fine grid level of 17 and a coarse grid level of 8) as well as for a uniform mesh with a refinement level of 17, about 99.5 % of the calculated mass was included in the initial cloud area, while the uniform grid with a refinement level of 14 still comprised 97.6 % of the original mass.

2. In the abstract (page 1, line 3) and conclusions (page 18, line 5) the authors state that adaptive meshes are useful to resolve filament structures of volcanic emissions. However, in the chosen example and in the presented results no filaments are present. The authors need to better justify and motivate the selection of the Pinatubo eruption as a case study.

The reviewer is right in observing no fine filamentation in the experiment data. The Mount Pinatubo case was selected due to available data sets both in wind fields and initial conditions (injection rates) and coverage data. The argument of filamentation was used due to earlier experience with similar but fictional simulations (e.g. Behrens et al. 2000).

3. The model description (chapter 2) does not describe the semi-Lagrangian transport model in any detail. The reference (Behrens, 1996) is given but it is not clear from the text that the transport model is described in there. More details at least about the model concept are needed with a proper reference to the Behrens (1996) paper for further details.

While the authors did not feel that gap in the description and decided to omit such technical details for better readability and brevity, we are happy to add some more detail on the implementation and algorithmic details in a revised version of the paper (see also our comments on the review of A. Folch).

4. Add in the last paragraph of page 4 that ash is treated as a passive tracer. Particle deposition (page 4, line 28) was not monitored. It is unclear why, when it would have been as easy to implement as suggested in the conclusions (page 18, line 14-15). Since deposited ash is the main source of information for historical eruptions it would have been good to test the resolution dependence of that deposition.

The treatment as a passive tracer will be added to the revised manuscript. It is certainly true that more model validation – especially the evaluation of particle deposition – could be done. It is, however, technically quite complicated because of the mesh changing over time, which is the reason for omitting it at this point. As already explained when replying to the review of A. Folch the main purpose of this paper is to focus on the adaptive mesh methodology applied to the simulation of ash advection and sedimentation and not on the exact reproduction of a specific volcanic eruption.

5. The mass eruption rate in table 1 has wrong units. I would assume it is kg per second instead.

Yes, thank you, this is a mistake. We changed it to kg/s.

6. In table 2 add 'refinement' to the word level to avoid confusion with vertical levels.

Done.

7. A cloud radius in degrees is an odd choice since this means an elliptical shape in physical space. This is inconsistent with the stated initial radial expansion of the plume on page 5. Discuss and clarify.

Since the simulation grid is in degree, we decided to define the initial radius in degrees as well; and with an initial radius of only three degree the shape only deviates from a perfect circle by a few kilometers, which is below the highest resolution of the model. But for very explosive volcanic eruptions with an initial radius exceeding a few degrees, or for studies with a very high vertical resolution, it should be considered to define the initial radius in kilometers.

8. I disagree that results are converged for fine mesh levels larger than 16 (page 9, line1-2). According to figure 3, ash concentrations in the centre of the plume to the south west of the volcano increase significantly from fine mesh levels 17 to 20 and to 23.Quantify the differences and discuss convergence or non-convergence in greater detail.

The problem with convergence is that it can only be tested for smooth and therefore idealized data. So, we decided to address the convergence issue not in detail but to focus on qualitatively similar results at different refinement levels. What is meant is the fact that from refinement level 17 onward, qualitative differences between the levels are very minor. Based on this observation we decided to run most of the experiments on such this refinement level.

Other minor issues: We corrected the minor issues directly in the manuscript. A revised version will be uploaded following the Editors decision.

Page 1, line 17: fall out of tephra

Done!

Page 1, line 18: tephra fall(s) out can lead...

Done!

Page 2, line 2-3: add: ...warmer winters and colder summer on the Northern hemisphere continents through dynamical feedbacks and radiative forcing, respectively (Robock, 2000).

Done!

Page 2, line 4-6: timescales of minutes don't influence the diurnal cycle

It is written that tephra is remaining in the atmosphere on timescales of minutes to weeks, which influences the diurnal cycle.

Page 5, line 3: remove 'one of' since it has been said before that Pinatubo was the largest eruption in the 20th century.

No, it was not stated before, that the Pinatubo eruption was the largest during the 20th century. In fact according to the Smithsonian Catalogue the largest eruption of the 20th century was the Novarupta eruption of 1912, the Pinatubo eruption was the largest eruption in terms of stratospheric disturbances.

Page 6, line 5: what is 'atypical' about the winds?

Not corresponding to the prevailing southwest wind owing to the passing typhoon (compare Wolfe and Hoblitt, 1996).

Page 7, line 20: say already here that 7 times means refinement level 8 (in table 2).Write 'in the initial model domain before refinement'.

Will be corrected in the revised version of the manuscript.

Page 10, figure 3: increase font of colour bar and text.

Will be corrected in the revised version of the manuscript.

Page 11, figure 4: use identical and more meaningful colour bar. There are no yellow or red colours visible. What defines the surface of the ash cloud? If it is a threshold concentration the figure should show an iso-surface. Explain.

This and following comments refer to the colorbars of the figures. This was also noted by the other reviewer A. Folch. We will update the figures in the revised version of the manuscript.

Page 12, figure 5: use same colour bar for both panels to enable comparison.

See above, last comment.

Page 12, line 15: delete 'however'.

Done!

Page 12, line 11-17: use information from table 3 for superposition of different ash sizes. Ideally, this should give the best fit and allow for a more independent validation.

The authors are not sure what the reviewer suggests: Should we use all different particle sizes simultaneously and superposition the different results for the different percentages of their respective contribution? What the authors intended to express was that with a sinking rate corresponding to the mentioned particle size the best correspondence to observations could be observed.

Page 13, figure 6: use identical and more meaningful colour bars. Yellow and red colours not visible.

See comment above.

Page 15, line 16: 'since none of our (model) simulations'

Done!

Page 16, line 3-4: write: will (not might) be underestimated

Done!

Page 17, line 5-7 and page 18, figure 11: it is not obvious to me that the shape is recovered well in all calculations. Quantify differences, in particular, discuss differences between top right and bottom right panels (identical fine resolution). Label for bottom right panel: shouldn't it read 'coarse=8'?

Will be updated in the revised manuscript.

Page 17, line 8-12: this is the common way to calculate performance gains due to adaptation. However, a transport model written and optimized for constant resolution can be significantly faster than an adaptive grid code run at constant grid resolution. Discuss to which extend this issue might apply here.

We assume that the performance of the semi-Lagrangian method employed here is relatively independent of the mesh design. Since we use a specialized algorithmic design that is based on a gather-scatter mechanism (see Behrens et al. 2005) for the ability to perform numerical operations on stride-one-vectors rather than unstructured meshes, earlier experiments have shown that the overhead imposed due to the adaptive mesh refinement is below some 5 % of the total run-time.

Page 17, line 12: unresolved reference

The following reference will be visible in a revised version: Madankan, R., Pouget, S., Singla, P., Bursik, M., Dehn, J., Jones, M., Patra, A., Pavolonis, M., Pitman, E., Singh, T., and Webley, P.: Computation of probabilistic hazard maps and source parameter estimation for volcanic ash transport and dispersion, Journal of Computational Physics, 271, 39 – 59, doi:http://dx.doi.org/10.1016/j.jcp.2013.11.032, http://www.sciencedirect.com/science/article/pii/ S0021999113007948, frontiers in Computational PhysicsModeling the Earth System, 2014.

References J. Behrens, N. Rakowsky, W. Hiller, D. Handorf, M. Läuter, J. Päpke, K. Dethloff (2005). amatos: Parallel Adaptive Mesh Generator for Atmospheric and Oceanic Simulation, Ocean Modelling, 12(1-2):171-183. J. Behrens, K. Dethloff, W. Hiller, A. Rinke (2000). Evolution of Small-Scale Filaments in an Adaptive Advection Model for Idealized Tracer Transport. Mon. Wea. Rev., 128:2976-2982. Wolfe, E. W. and Hoblitt, R. P (1996). Overview of the Eruptions. Quezon City : Philippine

Institute of Volcanology and Seismology ; Seattle : University of Washington Press, http://pubs.usgs.gov/pinatubo/wolfe/index.html.

We would like to thank M. Herzog for his thorough reading of the manuscript and his helpful and insightful comments and suggestions which will improve the manuscript if the editor requests a revised version.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-159, 2017.