Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-159-AC2, 2017

Natural Hazards and Earth System Sciences
Discussions
Open Access

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 model does not take into account for processes known to be relevant in ash cloud dynamics such as diffusion, dry/wet deposition mechanisms or ash aggregation. As a result, the model cloud dynamics limits to wind advection and particle sedimentation. Near-source effects (e.g. plume dynamics or gravity current) are ignored. On the other hand and more important, I do not understand why particle ground deposition is not contemplated. A part from the obvious interest of estimating ash/tephra fallout, the computation of the deposit is necessary (together with satellite imagery) for model validation (see below).

Printer-friendly version

Discussion paper

Indeed the model is simplified with respect to the ash cloud chemistry and microphysical processes. Our focus in this paper lies exclusively on the adaptive mesh methodology together with the simulation of sedimentation in context of the Lagrangian approach. Therefore processes like wet/dry deposition and ash aggregation are not considered. In order to consider plume dynamics and gravity currents other models like Atham or PDAC are certainly more appropriate to do so. After all, this is only an advective model like the basic Fall3D but with the add on of an adaptive mesh. Therefore we only focused on gravitatational sedimentation further away from the plume stem. The quantification of sedimentation to the ground (fall-out) could certainly be quantified but is at the moment not implemented in the model. Long term goal of our work is to include adaptive meshing into models like ATHAM or PDAC or any general GCM model. We consider this study as a first feasibility study for this endeavor and hope to encourage more scientists to work along these lines.

2. The innovative aspect of this manuscript is the use of adaptive grids in ash cloud simulations. However, several aspects regarding the numerical algorithm and its advantages are not detailed. Is the mesher embedded in the code? How often is the refinement applied? At each time integration step or at user-defined time intervals? The overhead of performing variable interpolation after each refinement and weather this is done in a conservative way or not is not mentioned.

You are right, we omitted these important aspects. But we will add this information in a revised version. The model uses a quasi-conservative semi-Lagrangian approach, as documented in (Behrens, 2006). This is a low order upstream interpolation method that can handle complex flow fields in 3D yet is simple enough not to impose too strict computational demands. The mesh refinement is triggered, as defined by the refinement criterion. As indicated in the manuscript, the tolerance for refinement is set to 0.002, which means that a grid cell is marked for refinement if the ash concentration gradient of the cell is above 0.2 percent of the maximum of all concentration gradients. The refinement criterion is calculated during each time step, but the actual refinement takes place only, if sufficiently many cells are marked for refinement. In our model at least 0.001 percent of the cells need to be marked for refinement to perform the refinement of the grid. The semi-Lagrangian scheme requires an interpolation in each step.

3. The authors conclude that (pg 18, lines 1-4): “we have demonstrated the versatility of adaptive meshing algorithms for modeling the dispersion of volcanic emissions. Especially the high performance of this code would allow, if implemented into operational ash dispersion models, a significant improvement of dispersion predictions as model runs could be carried out significantly faster compared to codes using a fixed grid”. Certainly there is a potential, but I think that this conclusion is too precipitated for many reasons. Constraining to Eulerian models (where this assertion would make sense), an important point not mentioned is code parallelism (most operational models actually run in parallel). The drawbacks of mesh adaptivity in the parallel execution of transient (time-evolving) problems are well-known: redefine the optimal domain decomposition at each refinement to ensure processor load balance has large associated interpolation and communication costs, code scalability breaks, etc. As a result, it is unclear whether the strategy would suppose a gain or not when running on hundreds of processors. This affects the main conclusion of this manuscript.

While it is true that our simulations were carried out in serial mode, the code has also shown parallel efficiency (Behrens et al., 2005) and recently even more effective ways of parallelization have been presented (Behrens and Bader, 2009), all compatible with our algorithm. So, the authors do not see a general problem with parallelizing the code efficiently. The main message here is that even without a large parallel infrastructure, one can perform reasonably highly resolved simulations with just a laptop. We have added a short part to the manuscript where we briefly touch upon the possibility of using this code also in parallel.

4. Another controversial aspect which is not discussed by the authors is that “we apply the model for individual particle diameters and then combine the results of different runs

to predict the sedimentation of the complete grain size distribution", pg. 4, line 22). I understand that this is necessary because the different particle sizes require of different refinements. However, how this affects efficiency in case on several (e.g. 10) particle classes is not even mentioned. Is the comparison shown in Figure 12 for a single class? This may not be fair, but it is difficult for a reader to extract conclusions since no details are given on the fraction of computation time of remeshing/interpolation. I suggest comparing the time of 10 simulations (coarse 8, fine 17) with that of a single run with all 10 classes and fixed grid.

Currently the sedimentation of the particles is calculated by adding a vertical wind component to the wind field that is equivalent to settling velocity of that respective particle size. In principle, the code could be rewritten to include an array of tracers of different size, each of which is advected with its individual wind field. The refinement criterion would then be applied to all tracer concentrations via the maximum gradient considering all tracers such that all tracers are well resolved.

5. I missed details on the model numerical algorithm. Is it explicit or implicit? What about the time integration step (only 10 min is mentioned in pg. 4 line 17, but based on what?).

The authors do not understand this remark. We use a semi-Lagrangian scheme. For the advection equation, this states that the upstream value of a material density needs to be preserved along trajectories. This in an unconditionally stable scheme. Now, the conservative scheme does not only consider material particles, but cells, which may be distorted due to shear, convergence, divergence in the flow field. This imposes a stability restriction, since the cells are not allowed to degenerate in one time step. But under mild conditions on the regularity of the flow field this still gives a stable scheme. Therefore the time step is relatively irrelevant and given here for information only.

6. Model validation could certainly be improved. I wonder why the Pinatubo eruption was selected to this purpose given some obvious difficulties: the role of the

Printer-friendly version

Discussion paper

gravity current (see Costa et al., Geophysical Research Letters, vol. 40, 1–5, doi:10.1002/grl.50942, 2013), the particular meteorological conditions at that time, the lack of extensive deposit sampling and inferred TGSD, etc. In any case, some quantitative model validation would be worth. On the other hand, it is stated (e.g. Table 4) that the refinement level goes down to <5 km. However, it seems that the driving meteorology is at 55 km and the wind field is linearly interpolated. Does it make sense? My impression is that refinement at sharp concentration gradients helps because it reduces numerical diffusion. . .

We accept the criticism that more model validation could be done. However, since this study is mainly of methodological character and since we used a very simplified chemistry, we decided to focus on a more qualitative approach. Regarding to the mesh refinement beyond the given wind field. This has been addressed in a former study (Behrens et al (2000)), were we showed that even in a very smooth/homogeneous flow field high resolution may be beneficial if shear in the flow field leads to stirring. So, we argue that even if the wind field does not contain small scale features, the transport benefits from high resolution. And yes, one of the effects that helps in this situation is the reduced numerical diffusion from high resolution.

7. Sensitivity study (section 4.1). The effect of variation in initial cloud height is actually a combined effect of injection height and driving meteorology (REMOTE). . .

We do not really understand, what the reviewer wants to express with this remark. Of course is this a combination of injection height with the wind field, and the simulated dispersion is a result of the prevailing wind conditions in the prescribed vertical layers of the injection.

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

Pg. 2, line 16. Parenthesis in reference

Done!

Pg 2, line 26. “very low computational cost”. This is too vague and generic: : :Also, values of “seconds” is not what Fig. 12 shows.

In Fig. 12 we actually compare one run with an adaptive mesh with calculations on a uniform mesh. Clearly the adaptive mesh with the same refinement level as the calculation on a uniform mesh is 10 times faster. Admittedly it still takes close to an hour so we rewrote the sentence accordingly.

Pg. 3, lines 8-9. The resolution of 0.5x0.5 is that of REMOTE? If so, I do not understand which is the gain with respect to driving global ECMWF (Era-Interim?) data, already available at this resolution. Do you mean that the mesoscale simulation is not used to increase the wind field resolution?

We used model results from a mesoscale model instead of meteorological analysis data, as they offer more flexibility for potential future applications, such as higher temporal resolution, e.g. in one hour intervals, increased spatial resolution, e.g. up to 10 km (Langmann et al., 2009), and model output for processes not yet included, such as rain rate per layer for wet deposition.

Pg. 3, line 12. Parenthesis in reference

Done!

Pg. 3, line 12. Like?

We did not really understand this remark.

Pg. 3., line 15. Model ! Domain

Has been changed.

Pg. 3, equations (1) and (3). Even if only advection is considered, shouldn’t these equations include the terms C(div_u) (where u = u_REMOTE+u_t)? Is the wind from

REMOTE divergence free? Can the z-gradient of the terminal settling velocity be ignored?

We are not sure what the reviewer is referring to. The advection equation is applied to the particle concentration (see eq. 1 and 3), i.e. the divergence of the particle concentration field needs to be known at this point not the divergence of the velocity field, as would be the case if we solve for the Navier Stokes equation that includes the divergence of the wind velocity field.

Pg. 4, line 26: "Since this work is a first case study of the modeling of sedimentation of ash particles on an adaptive mesh the impact of rain on the sedimentation and aggregation of ash particles is neglected." This is ok, but I have concerns about how aggregation could ever be incorporated in a future using this strategy. With sedimentation each particle class has to have its own mesh (different model runs) and aggregation requires concurrency.

We are certainly aware of this problem and in this version of the code this is not possible. See also answer to remark 4 above.

Pg. 5, lines 12-13. Are hours correct?

Yes, three hours after the onset of the climactic eruption (at 13:40) the intensity began to decline at 16:40 and nine hours after the onset (at 22:40) the climactic eruption phase ended.

Pg. 6, line 5: "This atypical wind in the lower and middle troposphere caused the wide distribution of tephra in nearly all directions around the volcano". Was this a meteo effect or because the radial gravity current?

We assume that it is mainly a meteorological effect (compare Wolfe and Hoblitt, 1996).

Pg. 8, Table 4. The horizontal/vertical element aspect ratio seems very large for tetrahedral elements (_100). Any hint on mesh element quality? In case of small angle, can this lead to oscillations/convergence problems?

Indeed the vertical vs. horizontal spatial scales are very distinct and the mesh quality in this respect is low. However, in the presented model, the mesh is used for interpolation and for maintaining conservation properties. For this type of application the mesh quality is of minor importance. Additionally, the non-uniform/anisotropic mesh supports the anisotropy in atmospheric scales (vertical vs. horizontal velocity ratio for example).

Pg. 9, line 15. “concentration on the surface”. What does it mean? Which value? How is this defined?

As indicated by the colorbar, the surface of the cloud is supposed to have an ash concentration of 1x10ˆ(-3) kg/mˆ3. Of course this value is quite arbitrary, but we had to pick one value. We added a description in the manuscript.

Figure 7 (and others). Why so many contours of observations? Only the corresponding to the time should be shown for clarity.

We will fix the coloring of the contours in a revised version of the manuscript.

References cited above:

J. Behrens (2006). Adaptive Atmospheric Modeling - Key Techniques in Grid Generation, Data Structures, and Numerical Operations with Applications. Lecture Notes in Computational Science and Engineering, 54, Springer Verlag, Berlin, Heidelberg.

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 and M. Bader (2009): Efficiency Considerations in Triangular Adaptive Mesh Refinement, Phil. Trans. R. Soc. A, 367:4577-4589, DOI:10.1098/rsta.2009.0175.

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.

Langmann, B., M. Hort and T. Hansteen, Meteorological influence on seasonal and diurnal variability of Nicaraguan volcanic emission dispersion: A numerical model study, J. Volc. Geotherm. Res. 182, 34–44, 2009

Wolfe, E. W. and Hoblitt, R. P.: 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, 1996.

We would like to thank A. Folch 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.