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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.

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

Summary

This manuscript presents a semi-Lagrangian atmospheric dispersal model adapted to volcanic ash. The model is solved using a Finite Element Method (FEM) using grid adaptivity on a tetrahedral unstructured mesh. The 1991 Pinatubo climactic phase is used as an application example, with some qualitative model validation. The mod-

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Discussion paper

elling strategy (novel in the context of tephra dispersal models) may have potential advantages. However, my overall impression is that several aspects of this manuscript should be clarified/better explained before accepted for publication.

General comments

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)

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.

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.

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.

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?).

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 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. . .

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)...

Specific comments

Pg. 2, line 16. Parenthesis in reference

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

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?

Pg. 3, line 12. Parenthesis in reference

Pg. 3, line 12. Like?

Pg. 3., line 15. Model → domain

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?

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.

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

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?

Pg. 8, Table 4. The horizontal/vertical element aspect ratio seems very large for tetrahedral elements ($\sim$100). Any hint on mesh element quality? In case of small angle, can this lead to oscillations/convergence problems?

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

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

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