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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volcanic ash modeling with the on-line NMMB/BSC-ASHv1.0 model: model description, case simulation and evaluation" *by* Alejandro Marti et al.

L. Mastin (Referee)

lgmastin@usgs.gov

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This paper describes the model NMMB/BSC-ASH, which solves both for meteorology and dispersion of volcanic ash. It provides to examples, from the Cordon Caulle (2011) and Etna (2001), where the model does a successful job of reproducing the meteorology and dispersion of ash clouds and deposits. And it shows that this model may be capable of running fast enough to be an operational tool, which would make it the first online operational model for volcanic ash dispersion. The paper is clearly written and presents significant advance in ash dispersion modelling. For these reasons I recommend that this paper be published. I have a couple of minor overall comments however that should be addressed.

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Discussion paper

First, It seems like the main advantage of an online model would coupling the volcanic processes with meteorology. For example, a big eruption with an umbrella cloud can modify the wind field. And eruption clouds can shade downwind areas, modifying processes like catabatic flow. But it doesn't look like you actually couple the dispersion back into the meteorology in the two examples. So, what were the advantages of this model in these cases?

Second, the physics of particle sedimentation and aggregation in Sections 3.3-3.5 and could be explained more clearly, and the equations more fully explained. Also, the equations are not numbered sequentially. Finally, some terms like monotonicity are probably understood by numerical specialists, but not by volcanologists. If you wish to attract volcanologists to the paper, I suggest you explain at least some of them. Specific ones are noted below.

These comments can be addressed with minor, technical changes to the manuscript.

Larry Mastin

Specific comments (some of which duplicate the ones above)

Page 1, Line 16: change "predicts" to "forecasts"

Page 2, line 19: Why do you call them time slabs? (rather than time slices or time intervals?). Is a slab a point in time or an interval in time?

Page 2, line 31. Is NMMB an abbreviation?

Page 3, line 7. Here you mention that the NMMB has low computational cost. Could you add a sentence or two quantifying that? Is it lower, for example, than a WRF simulation; if so, how is it more computationally more efficient? (maybe just note that you'll elaborate later in the article).

Page 3, line 38. What do you mean by a rotated latitude/longitude coordinate?

Page 4, line 17. Change "wind fields" to "wind field".

Page 4, lines 12-20. Could you explain what you mean by “effective wind fields”? Also perhaps explain the term “coupling interval” on line 19. It would also help to explain more clearly how the offline approach differs from the online approach.

Page 5, line 15. “the vertical distribution of the column shape”: do you mean “the vertical distribution of mass in the column”?

Page 5, line 24. Clarify that H_plume is the total column height above the vent (not above sea level).

Page 5, equation 4. Is S(z) in kg/s, or kg/(m s)? Seems like it should be kg/(m s), but the right-hand side of the equation appears to be in dimensions of the MER, i.e. kg/s.

Page 6, lines 27-28. “The model is based on a solution of the classical Smoluchowski equation, obtained by introducing a similarity variable and a fractal relationship for the number of primary particles in an aggregate.” Is this method described in Costa et al.? If not, you might have to describe it in more detail here.

Page 7, line 13. Change “Crank-Nicholson” to “Crank-Nicolson”

Page 7, lines 17-33. It’s interesting that you use the Costa et al. (2013) parameterization for radially spreading umbrella clouds. I would have thought that an online model would have the advantage of considering the momentum of umbrella spreading explicitly.

Page 8, lines 4-5. I’m a little confused by the statement that Stokes settling is considered an efficient removal mechanism for small particles (<20 um). Almost no particles of this size are removed from the atmosphere over eruptive time scales without aggregation mechanisms or rainfall scavenging.

Page 8, line 16. Change “relaying” to “relying”.

Page 8. Equation 18 needs more explanation. What are the dimensions of R_a dn R_s? It seems like they should be in seconds per meter if this equation is to be di-

mensionally consistent. And v_d and v_s are in meters per second? Are those settling velocities? Also, why are the equation numbers not sequential? They go from eq. 4 to eq. 13 to eq. 18!

Page 9, lines 6-10. Could you define monotonicity and positive definiteness? Not all readers will know what it means. Also define width halos

Page 9, line 10. Change “Nicholson” to “Nicolson”.

Page 9, line 14, change “of weak long-lasting eruptions” to “of a weak, long-lasting eruption.”

Page 9, beginning of Section 4. Could you say a little bit about how these eruptions could be simulated better by an online model than an offline model? Is there important coupling with the atmosphere in these cases that is not being considered with the offline model?

Page 10, line 25. Delete “over” after “spanned”

Page 10, line 26. Change “climatic” to “climactic”

Page 11, line 1. “a cloud” or “clouds”? Are you talking about the eruption cloud, or meteorological clouds?

Page 11, line 4. I’m not sure what makes this episode complementary. Perhaps just say “another episode? Did it occur at the same time as the first episode, or afterwards? At what time did it occur?

Page 11, lines 6-8. Please indicate which frame in Fig. 2 illustrates your point when describing these changes in wind.

Page 11, line 8. How is the trough illustrated in Fig. 2?

Page 11, lines 23-24: “Feedback effects of ash particles on meteorology and radiation were not included in this run”. So, what is the value added using this online model?

Page 11, lines 26-28, “Daily eruption source parameters (ESP) were obtained from Osores et al. (2014), who estimated column heights for each eruptive pulse using the Imager Sensor data from the GOES-13 satellite”. Could you be more specific about how height was estimated? By IR brightness temperature, assuming the cloud temperature equaled that of the surrounding atmosphere?

Page 13, lines 7-9. It seems odd that you are running the NMMB/BSC-ASH model at a horizontal resolution of 0.75x1 degree, but initializing it with ERA-interim meteorology at a horizontal resolution of 0.75x0.75 degree. What are you gaining by running the NMMB/BSC-ASH model?

Page 13, Section 4.1.2. For your global simulation, did you use all grain sizes?

Page 13, line 9. Change “reinizializated” to “reinitialized”.

Page 13, line 24. Change “airports closure” to “airport closures”.

Page 14, line 24. Change “terrain following grid” to “terrain-following grid”. Also, change “the model is used” to “the Fall3d model is used”.

Page 15, lines 32-36. It’s interesting that you got better fit to the Etna data using the NMMB/BSC-ASH model than using the Fall3d model. Why do you think you got a better fit? Was the wind field produced by the NMMB/BSC-ASH model very different from that used by Fall3d? The source terms were the same for both models, right? So it had to be the wind field? Where was the wind field different? In Fig. 10, it looks to me like the fits were most improved where thicknesses were highest, and where they were lowest. Why would the NMMB/BSC-ASH model have been better in those places?

Page 17, line 14. I’m curious that you mention gravity current conditions in the source term. This is generally not considered. What do you mean by this? The existence pyroclastic flows that could serve as a source?

Page 18, line 7. Change “climatic” to “climactic”.

Page 18, line 8. “maximum efficiency for the global simulation described in Table 7 is reached at $\approx$ 32 nodes”. I’m having trouble seeing this in figure 12.

Page 18, line 31. What does “(6x84+8)” mean?

Page 19, line 6. Change “long-rage” to “long-range”.

Page 19, lines 16-17. You say that NMMB/BSC-ASH has been validated against eruptions of Pinatubo, Etna, Chaitén, and Cordón Caulle. In this paper you only describe Etna and Cordón Caulle. Should you be citing another study for the validation against Chaitén and Pinatubo?

Table 1, equation 1. You might clarify that H_plume is the height of the column above THE VENT (not above sea level).

Table 1, equation 2. I don’t see a definition for n. Also, do you use a value of 2.8 for z1, as Degruyter and Bonadonna do?

Table 4: k and Delta-n_f are not defined. Also, is Delta-n_f the number of particles PER UNIT VOLUME that aggregate per unit time?

Figure 2: could you add a symbol indicating the location of Cordon Caulle volcano? Also, please label Argentine Patagonia. And add country boundaries, so we know where Paraguay is when you describe it in the text. It’s also not clear why you chose these times to illustrate in this figure. It’s not explained in the text or the figure caption.

Figure 3. In the satellite images, you need a scale for brightness temperature difference. And what IR bands were being differenced?

Figure 4. Mention that the color scale on the left is also g/m2. One can infer this from the text but it would be good to say it explicitly.

Figure 5. Add latitude and longitude tick marks to the left-hand maps so that they can be more directly compared with the right-hand ones.

Figure 6, left plot. Contour labels on this map are too small to read, even when enlarging the map on the computer screen.

Figure 9 caption. Perhaps change “predicted deposit load” to “modeled deposit load”.

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