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Interactive comment on "Inclusion of Ash and SO_2 emissions from volcanic eruptions in WRF-CHEM: development and some applications" *by* M. Stuefer et al.

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General comments to the manuscript: Inclusion of Ash and SO2 emissions from volcanic eruptions in WRF-CHEM: Development and some applications By M. Stuefer et al.

Introduction

All operational volcanic ash transport and dispersion (VATD) models currently use wind fields and other meteorological parameters such as temperature and pressure that are generated by numerical weather prediction (NWP) models. These VATD models are called "offline" in that they assume that volcanic clouds don't substantially change

meteorological conditions and hence the meteorology and volcanic-cloud transport can be decoupled.

This paper is the first I'm aware of that describes an "inline" model that solves for volcanic ash transport AND meteorology simultaneously. The model is a modification of the meteorological model, WRF-CHEM, which calculates both meteorological parameters such as the wind, temperature, and pressure; and transport and dispersal of volcanic ash and gas. The achievement is significant and in my view a paper describing this effort is worthy of publication. However I think several important issues should be addressed before this paper is worthy of publication.

General comments

First, the paper does not address the obvious question, "under what conditions does a coupled model better simulate observed events than an offline model?". I would have expected hypotheses and inline/offline comparisons of specific eruptions. One might expect coupling to be most important in the really big eruptions, like Pinatubo, where rapidly expanding umbrella clouds can push their way hundreds or thousands of kilometers upwind [Holasek et al., 1996]. But perhaps the model resolution is too coarse to adequately simulate plume dynamics of these events. So, what exactly are the advantages of this coupled model and how can they be demonstrated?

Second, there should be more explanation of what is calculated, and what meteorological/volcanic interactions can be simulated using WRF-CHEM. WRF and other NWP models solve simultaneously for mass, momentum, and energy; thus, phenomena like catabatic winds, which are generated by warming conditions, can be simulated since WRF considers thermal and energy effects. I've heard secondhand that WRF-CHEM solves only for conservation of mass for volcanic clouds, not for energy or momentum. How does this limit the application? If dense ash clouds prevent heat from reaching the ground surface and generating buoyant thermals, does WRF-CHEM calculate those effects?

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Third, the paper could use more information in the Introduction explaining the purpose of this modeling effort. Is it intended to be used operationally to forecast ash and SO2 transport during volcanic eruptions? If it is too numerically intensive to be used operationally, is it intended for use analyzing past eruptions in greater detail? If so, how does the incorporation of the global ESP database add value?

Specific comments

The grain-size distributions used in Table 2 and the mass fraction of fine (<62 um) ash in Table 1 were not adjusted to account for particle aggregation. If they are used without modification, the mass of ash in distal clouds will be greatly overestimated, and the deposit is likely to be much more widespread and thinner than actual mapped deposits.

Explanations of how certain WRF-CHEM modules are implemented for volcanic ash and SO2, mostly in Section 3, are sometimes difficult to follow, largely because there is little explanation what modules like GOCART and MOSAIC calculate. Adding a paragraph or two explaining these modules (so readers don't have to go to the cited literature) would make it clearer.

Section 4: "initial applications". There is no explanation of the inputs used to calculate the wind field in these simulations.

Section 4.1: there should be a table listing source parameters for the Redoubt 1989 simulation and explaining why these values were chosen. In addition, there should be a more detailed an critical comparison between the simulation results and observations. As noted on p. 2581 (line 11)), the WRF-CHEM simulation appears to produce a lobe that is generally in the same direction as the mapped lobe of Scott and McGimsey (1995), but the isomass values in the downwind lobe appear to be less than mapped values by at least a factor of two. There is no mention of this discrepancy. Did the authors use the erupted mass estimated in Scott and McGimsey in their simulation or did they estimate it from the plume height and duration using the height-eruption

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rate relation of Mastin et al. (2009)? The chosen grain-size distribution may also have resulted in the lower isomass values in proximal areas.

Section 4.2: the discussion of the Eyjafjallajökull cloud simulation seems rather cursory. Eruption source parameters are not adequately described and the comparison with observations is not systematic or very quantitative. There should be a table of source parameter values and explanations of how they were chosen here as well. Does the plume height and eruption rate vary with time in the simulations? Other modeling studies [e.g., Devenish et al., 2011; Stohl et al., 2011; Webster et al., 2012] had to use time-varying parameters to accurately simulate this event. The use of an S2 ESP particle-size distribution (40% <63 um) from Mastin et al. (2009) to simulate the Eyjafjallajökull ash cloud would place too much ash in the distal cloud, by about an order of magnitude (for Eyja, Devenish et al. [2012] found that simulations matched best with observations when 1-5% of the erupted mass was fine enough ($<\sim$63 um) to get into the distal cloud). Surprisingly, the ash concentration simulated at Leipzig (Fig. 6) matches remarkably well with that inferred from Lidar measurements there [Ansmann et al., 2010]. It also seems noteworthy that the WRF-CHEM simulation produced a cloud nearly as thin as the observed one in the Leipzig Lidar (Fig. 6). Attempts by at least three modeling groups [B J Devenish et al., 2012; Folch et al., 2012; Kristiansen et al., 2012] to duplicate the thinness of the cloud at this distance were unsuccessful. In private discussions, these investigators seem to believe this lack of success is because the numerical wind fields used don't have the vertical resolution to resolve very thin clouds. Perhaps the WRF-CHEM simulations were able to resolve wind variations at higher vertical resolution? There is no mention of the model resolution used in these simulations. These points should perhaps be noted and discussed.

On page 2578 near the bottom the discussion seems to imply that the WRF-CHEM model uses the MOSAIC module (mentioned on the previous page) to calculate aerosol properties. If this module is used, it would be worthwhile adding a few sentences explaining a little more about what the MOSAIC module calculates and how it does

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those calculations.

On p. 2579, lines 7-9, what are the more complex chemistry options that you allude to? Use of the terms species (e.g. p. 2580, line 16), modes, and sections (p. 2578, lines 21-22) are not defined and it's not always clear what they mean. In several places (e.g. p. 2578, line 3; p. 2580, line 16), it may be clearer to use "grain sizes" or "grain size bins" rather than species.

Figure 4: left side, it should be noted that only the dashed contour lines delineate the deposit of December 15, 1989; solid and dotted contours denote deposits of December 14 and 16, respectively. And the meaning of the large and small dots should be mentioned. There is also no explanation of the significance of the bold contour lines on the plot to the right (they look like 1, 10, 100, and 1000 g/m2 contours). Figure 5: The labels on the color tables are too small to read and there is no comparison with satellite mass retrievals for this time period.

Figure 6: It would seem more appropriate to illustrate the ash concentration in Figs. 6C and 6D using histograms that reflect the vertical cell spacing. And I think the mass concentration from the Lidar measurements which were noted by Ansmann et al. [2010] could be displayed in this figure.

Technical corrections

p. 2573: Line 3, change "developed in collaborative efforts" to "collaborated to develop"; Line 16, the meaning of the word "inventories" is unclear. I suggest using a different word; Line 21, delete "emissions as a major natural source of volcanic ash" (for brevity); Line 23, change "comprised" to "composed

p. 2574: Lines 10-11, the data generator package for system initialization could be described more clearly; Line 18: the term "ash emission fields" appears to really mean "eruption source parameters". I suggest changing it to "eruption source parameters" or clarifying the meaning.

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p. 2575" Line 11, change "%" to "percent".

p. 2577: Line 25, what does the term "first and second indirect effects" mean?

p. 2578: Lines 6-7, there is no mention of whether WRF-CHEM considers particle shape when calculating settling velocity or simply assumes a spherical particle; Line 14, change "100-200" to "<200"

p. 2579: Lines 8-9, please clarify what you mean by "these more complex chemistry setups". Are you referring to the atmospheric tracking of various sulfur species?; Lines 13-14, delete "in the WRF-Chemnamelist" (for brevity); Line 17, add a space between "Mastin" and "et".

p. 2580: Line 7, change "experiments" to "simulations"; Line 8, change "that" to ", which"; Line 15, change "interactions" to "reactions"; Line 16, change "species" to perhaps "grain sizes" or "grain-size bins".

p. 2581: Line 11, add a space between "fall" and "as" at the end of the line; Line 12: add a space between "WRF_Chem" and "was".

p. 2584: Lines 9-10: Delete the phrase "since there are no aggregation observational data and only very vague model theories in existence." There are quite a few papers of aggregate observations and at least a couple of theories that are somewhat detailed [Costa et al., 2010; Gilbert and Lane, 1994].

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