Review of Stuefer et al., GMD 2012

Inclusion of ash and SO_2 emissions from volcanic eruptions in WRF-Chem: development and some applications

Ratings

Scientific significance:	2 (good)
Scientific quality:	3 (fair)
Scientific reproducibility:	4 (poor)
Presentation quality:	4 (poor)

Overview

This manuscript presents a summary of how a volcanic ash emission and transport scheme has been implemented into the WRF-Chem model. The basics of this work, as well as half the results, have been published already in JGR (Webley et al. 2012). The additional detail in this new manuscript primarily focuses around the input dataset used to generate emission details. The methods used are based on the work of others that have been incorporated into WRF-Chem. The manuscript appears quickly written without a lot of specific detail. The model description is high level, which is fine for some model aspects, but could benefit from added detail for processes of particular importance to the ash prediction, such as the deposition processes.

Technical Comments

p. 2578, l. 9-10: The computational cost is claimed to be "minimal." However, would it be reasonable to expect to use this model in an emergency situation? How long would it take to configure and run the model for a new event? Also, what is the actual computational cost? Some perspective based on actual model timings would be useful. For example, how much more expensive is it to run the model with tracers in the simplest ash setup than in standard meteorological mode for a 300x300 point domain? How about when running with wet deposition as well, which would require twice as many tracers due to activation of particles into the cloud phase? By adding 10 tracers, the model cost is most likely about double the standard model configuration used for typical meteorological forecasts. For the case with wet deposition, the addition of 20 tracers would make the model roughly three times more costly.

Section 3: Deposition processes are critical for getting accurate tephra-fall deposits, such as shown in the first case study, as well as in determining long-range transport, such as shown in the second case study. This section would benefit greatly from a detailed description of the wet and dry deposition processes in WRF-Chem and how these perform and influence the ash calculations. For example, the manuscript states that the simplest approach only uses a Stokes law methodology. It should be noted that this setup would severely underestimate deposition when rain is present since there would be no treatment of wet deposition. Another example is the more complicated approach with wet and dry deposition. Currently, the manuscript states that the ash is treated as dust in terms of hygroscopicity. However, no additional detail is given. For those unfamiliar with cloud-aerosol interactions in WRF-Chem, there is not enough detail present to understand what is done. What model settings are

appropriate at different scales in terms of resolved and parameterized clouds? Are the removal processes treated consistently for both resolved and unresolved cloud processes?

Are the particle activation routines expected to be accurate in the context of pyrocumuli, which would be an extreme case in terms of particle loading?

How representative is assuming the hygroscopicity of ash is the same as dust (which in WRF-Chem is typically 0.1, which should be stated in this manuscript)? Others have looked at this issue, and it should be noted in this manuscript, e.g., Lathem et al. (2011) show that the κ=0.1 assumption is probably sufficient for the Mt. Redoubt eruption, but overestimates hygroscopicity for many other eruptions, including Eyjafjallajökull.

What is the range of grid spacings that can be used with this model? Because the ash is placed directly at specific heights, the model presumably cannot accurately reproduce pyrocumuli induced from heat generated by the volcano and aerosol effects in the immediate vicinity of the eruption. The ash particle activation would not be accurate since the strong vertical velocities from the explosion would not be present. Also, activation in WRF-Chem only occurs at cloud base. When strong vertical velocities are present that would cause some particles to activate in model levels above the cloud base, within the cloud, the activation will never happen.

p. 2579: The parameter *d* is not clearly defined and its units seem to be inconsistent. On line 17 it is given units of seconds. But, on line 23 there is the statement "a 500 m error in d" that implies the units are meters.

Section 4: Not enough information is given for others to reproduce the case studies. For example, what are the grid spacings? What is used for the initial and boundary conditions? It is stated that no chemical interactions take place, but are there cloud-aerosol and radiation interactions?

p. 2580, l. 15-18: This states that both case studies include dry and wet deposition as well as ash settling. What is the difference between the "settling" and "dry deposition"? Also, p. 2578, l. 8-9 implies that the simple setup without wet deposition is used for the Eyjafjallajökull case study, which contradicts the statement in Section 4.

Section 4.2: The presentation of the Eyjafjallajökull case study adds nothing new over what has been previously published on this case study in Webley et al. (2012). Given this fact, does it need to be repeated here? The two associated figures (5 & 6) are even identical except for the change in color space from CMYK to RGB.

The two case studies come across poorly. The impression is given that they are only included because the authors felt like they had to show some sort of results, but did not have very much to show. Instead of giving cursory results from two unrelated case studies, a more useful approach would be to show one case study with results from the different model modes, e.g., the simple (cheap) configuration versus the full treatment. This could then be used as a basis to discuss the benefits and tradeoffs of each approach. As the manuscript currently stands, we only have a simple comparison against a single observation for the Eyjafjallajökull case study and a few stations for the Redoubt case study that make it really hard to actually know how well the model reproduces each situation. We also do not have any information about how the model results differ when the model is run using the different settings presented in the model description section.

Fig. 4: More information is needed for panel (a). What is the contour interval? What is the difference between solid and dashed contours? What are the latitudes and longitudes? How were the observed isopachs determined, e.g., from kriging? Also, is it possible to put both panel (a) and (b) on the same map projection? It is difficult to compare the two plots right now because there is not enough spatial information in the northern half of panel (a) to match it to panel (b). What is the accumulation time of the isopachs in both panels?

Fig. 6: What quantity is shown in the lidar data? It is not clear what the RC signal represents?

Minor Comments

There are a lot of cases of missing and extra spaces throughout the manuscript.

Title: The WRF-Chem name is used inconsistently. In the title it is "WRF-CHEM" but in the text it is "WRF-Chem". The latter is the more consistent way to refer to the model within the WRF-Chem community.

p. 2574, l. 7-10: The sentence beginning with "The volcanic ash model includes as source…" is awkwardly worded.

p. 2574, l. 10-11: The phrase "volcanic emission data generator package for system initialization" is a bit of a mouthful. The sentence could be reworded to something like: "We have developed a volcanic emissions package for initializing the ash fields within the model based on a look-up table containing the ESP data."

p. 2574, l. 14: Missing "a" in "has been used as *a* template".

p. 2575: Multiple instances of using *correspondent* when meaning *corresponding*.

p. 2576, l. 27: The to cases of *on* should be *or*.

p. 2577, l. 9: Either a colon or a comma should replace the opening parenthesis.

p. 2578: l. 11: "The next step" would be better worded as "A more detailed approach".

p. 2578, l. 15: Missing "the" in "as well as *the*".

p. 2580, l. 7: There are only two experiments and two model versions, so explicitly stating which case study uses which version of the model would be more specific and not take many more words than the vague opening sentence to this paragraph.

p. 2582, l. 13: *proofed* should be *proved*.

p. 2582, l. 25: Missing *and* for "biomass burning *and* GOCART aerosol".

p. 2583, l. 27ff: "distributing the ash in an umbrella shaped plume in similar fashion to the ash" does not make sense. How does one distribute ash in the fashion of ash?

Fig. 2: "model do" should be "model to".

Fig. 5: The color scale does not need to be reproduced for every panel since it is the same.

Figs 4-6: The labels are too small to be readable.

Figs 4-6: These are said to be "adapted from" previous material when in fact they actually appear to be identical copies. Should this be stated as "used with permission from …" assuming proper permission has been obtained from the previous publishers?

References

Lathem, T. L., P. Kumar, A. Nenes, J. Dufek, I. N. Sokolik, M. Trail, and A. Russell, 2011: Hygroscopic properties of volcanic ash. *Geophys. Res. Lett.*, **38**, L11802, doi:10.1029/2011GL047298.

Webley, P. W., T. Steensen, M. Stuefer, G. A. Grell, S. Freitas, and M. Pavolonis, 2012: Analyzing the Eyjafjallajökull 2010 eruption using satellite remote sensing, lidar and WRF-Chem dispersion and tracking model, *J. Geophys. Res.*, **117**, D00U26, doi:10.1029/2011JD016817.