Response to Review #1

We thank the reviewer for taking the time to read and we appreciate the helpful comment and suggestions for improving the manuscript given in this review.

Answers to the specific comments are given below, reviewer comments are given in black, answers are given in blue, and changes in the manuscript are noted in quotations (“”), also in blue.

Specific Comments:

At the end of section 2, where the operational set-up is discussed, the authors give a nice description of the scheme used for real-time comparison with satellite retrievals and for inverting for source terms. Section 3 is more of an investigation on the sensitivity of the model to variations in the meteorological conditions by running eEMEP with 24 ensemble cases. At the end of section 3, however, there is a discussion of ensemble runs in an emergency forecast environment. It is not really clear how the ensembles are used in forecasting. Are they available in real-time? It looked like this was just a retrospective on the Bardarbunga event. If ensembles are used, is the source-receptor inversion used with each realization of the ensemble?

The authors are grateful for the reviewer pointing out that this is not clear in the text. Running an ensemble forecasts is very computationally demanding, especially on a high resolution. The ensemble meteorology is available in real-time, and dispersion forecasts may therefore be run if the ensemble data show a large spread in the placement of pressures systems that can influence the dispersion of the volcanic emission. For the emission estimate, this is another source of uncertainty that is possibly bigger than that caused by the weather. Running an inversion to produce a new source term for all the ensemble members is probably too computationally demanding, and running the ensemble members with different source term would not reflect the uncertainty due to only the weather as presented in the manuscript. To include this aspect in the description, this sentence will be added on p. 8, l. 4

“Using here the same best guess source term in all our ensemble model simulations offers the opportunity to study the results based on only the different weather situations, meteorology uncertainties and resolution.”

And these two sentences will be added in the manuscript p. 10, l. 23.

“As in this study, to exclusively look at the spread due to the uncertainty in the weather forecast, the same source term should be used in all the members. Therefore the model simulations used as input for the inversion calculations will only be driven by the deterministic meteorology.”

It is not really clear what is the range of variability in the ensembles. They seem to be primarily subdivided based on the description of cloud physics. As opposed to explosive eruptions that simultaneously release SO2 and ash, Bardarbunga was primarily fire-fountaining with a continuous surface emission of SO2. I would think that the main discriminating aspect of the meteorology is the characterization of the planetary boundary layer and how vertical diffusivities are calculated. In Hawaii, lowlevel winds within the boundary layer play a critical role in the SO2 advection, especially the diurnal variations (sea-breeze, nocturnal katabatic winds, etc.). The VMAP project has found that they need to calculate meteorology with WRF at a resolution of 1 km over the Big Island in order to capture the surface winds properly. Is this not as important in Iceland?

Of the 24 ensemble members used in the study, they are divided over two cloud physics parameterization, these are again divided over two forecast start points were six of each model parameterization start at 00 UTC and 12 UTC and the last six of each start at 06 UTC and 18 UTC. The six remaining members with equal model parameterization and start time are perturbed using the EuroTEPS (European Targeted Ensemble Prediction system) that perturbed the members both in the initial field and on the model domain border (Frogner and Iversen, 2010). We hope that will be made clearer in the manuscript by adding on p. 8, l. 20:

“All of the members are perturbed by using EuroTEPS.”

Comparing the VMAP project results (Businger et al., 2015) to this study, the strength of the Barðarbunga eruption, especially at the start of the eruption period is stronger releasing more SO_2 higher up in the atmosphere (700 m for VMAP and up to 3000 m assumed here). The area of interest is also different, where the VMAP hopes to forecast the pollution due to the volcanic degassing on a local scale, the scope of this study is to investigate the regional scale transportation of volcanic emission. Gíslason et al. (2015) investigates the environmental pressure on Iceland due to the Barðarbunga eruption with a 4 km model resolution, and found that only a fraction of the released SO_2 up to 500 m (10-20% of total) is affecting the surface concentration at distances up to hundreds of km from the volcano. The geographical location of Iceland also makes the dispersion more influenced by large scale pressure systems compared to the meteorological conditions Hawaii experiences. During the Barðarbunga eruption most of the pollution was transported far away from the volcano.

Section 4 focuses a bit more than necessary on the benefits of including gravitational sedimentation. It is widely recognized in the volcanic ash dispersion modelling community that it is the dominant removal process for ash > 64 um. It become less and less important with smaller and smaller particles, to the point where it is negligible compared to the effects of wet scavenging or aggregation. The vertical position of distal ash will be very sensitive to the characterization of the grainsize distribution and on the specific source terms used (mass-loading as a function of height and grainsize at the vent). It is difficult to compare model results with lidar data as evidence supporting including or neglecting sedimentation since the airborne grainsize distribution above the lidar station is not really known.

The authors agree that it is difficult to validate sedimentation inclusion. However lidar measurements are the only ones which offer a chance to detect vertical displacements of ash plumes, something which should happen as a consequence of significant sedimentation. We believe its an original idea, and do not claim, that it’s the ultimate idea to validate sedimentation. The study shows that subsidence due to a high pressure is much more important than the vertical displacement caused by gravitational settling. The authors still find it necessary to include the process of gravitational settling for a more correct description of the ash transport. A 1 km displacement as found in our study is important if the vertical wind shear is strong. Wet scavenging is already included in the model, and wet deposition of e.g. SO_x is validated every year in the EMEP MSC-W reports and are therefore not included in this paper. Aggregation of fine ash to large ash is still not included in the model.

The manuscript has been extended in this section to include other model comparisons to lidar measurements during the Eyjafjallajökull eruption as suggested by the short comment 2. One of them were from Webley et al. (2012), where WRF-Chem simulations show that ash particles larger than 62.5 µm were not transported further than 120 km from the volcano. This may indicate that including larger ash particles would be extensive in model simulations were the goal is to asses the airspace over Norway.

References:

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