Response to Reviewer #2

We thank the reviewer for taking the time to read our manuscript 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:

Section 2.2

eEMEP is run with 40 or 42 levels. Please precise the corresponding top altitude (even if it is specified in section 4).

The authors agree that this is worth noting, and will add the corresponding heights and change p.5 l.3 to:

“Model simulations presented in this paper are either done with 40 or 42 vertical levels depending on available meteorology pre-processing, with a model top at 32 km and 30 km respectively.”

Section 3.1

Is the eruption column between 1500m and 3000m uniform or is there a specific shape? Does it correspond to what would be done in real time (during an emergency) or is it meant to be as near to the reality as possible?

The emission flux is distributed uniformly over the eruption column. During an emergency, either the source term is from inversion calculations and then may include specification of the height of emissions or an a priori height distribution estimate will be used. However, here we did not vary nor test the a priori emission height, and rather wanted to be near to reality. The sentences changed in the manuscript:

“…emission estimate with a 120 kt d-1 flux uniformly over an eruption column between 1500 m to 3000 m matched best for the first days of September. This emission term is also supported by Thordarson and Hartley (2015) and used here. In an emergency case an a priori source term would be used first when little information about the volcanic source term is known. 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. ”

Please define the SO2 ‘free state’ used as initial.

Free state means that there is no volcanic SO_2 in the atmosphere at the start of the forecast. This will be added to p.8 l. 29:

“In contrast to what is possibly done for a real case, all the forecasts are started from a model state with no volcanic SO_2 in the atmosphere,…”

The sentence about the simple reduction of the meteorological input data is not clear for me. How is the ‘representative point (every fourth one) chosen?

This is a very simple reduction where the algorithm loops in both horizontal directions trough the original grid and picks out every second and every fourth grid point to obtain the values of the grid points in the coarser grid resolutions,. As stated in the paper, this may not provide a smooth field and some maximum/minimum values may be representing a larger area than originally found by the NWP. Sentence changed to:

”A simple reduction in resolution of the meteorological input data is obtained here by letting every other or every fourth original grid value become the grid value representing the coarser grid resolution respectively.”

Section 3.2

Figure 3 is interesting, but it would be very helpful for the reader to have another one showing the different trajectories according to the different members of the ensembles.

Since eEMEP is an Eulerian model it is not possible to compute single trajectories, however the authors agree that including a figure showing volcanic SO_2 VCDs from different ensemble members would increase the understanding of the results. A new figure has been created and will be added as figure 4 in the manuscript, showing the 5 DU contour line for four of the members, one from each of the model parameterizations and starting times (figure introduced here as fig 1, see below).

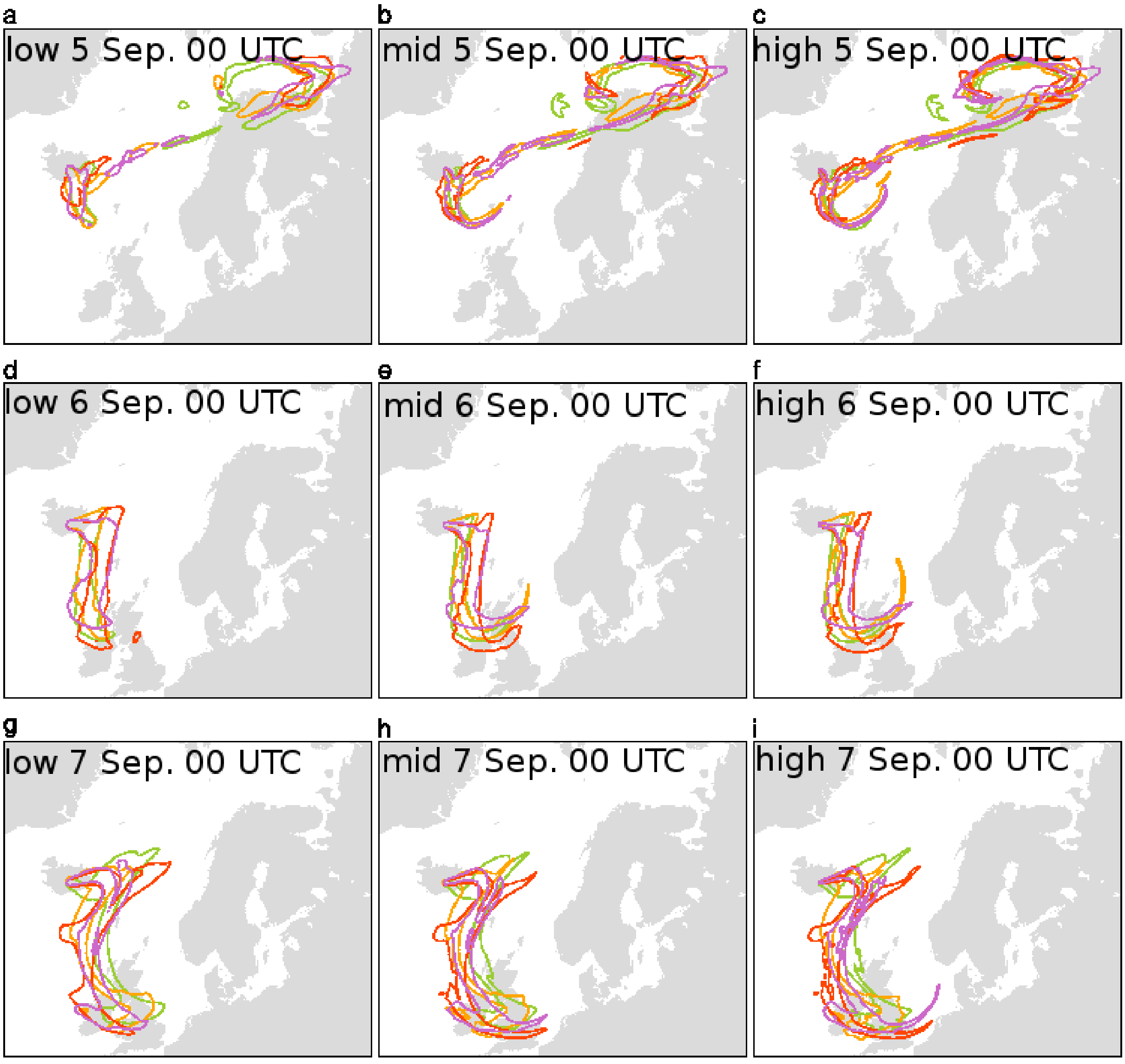

Figure 1: 5 DU contour lines for four exemplary members after 48 hours of forecast in the low, mid and high resolution ensembles, in the left, middle and right column respectively, for start time 00 UTC 3. September (panels a,b,c), 00 UTC 4. September (panels d,e,f) and 00 UTC 5 September (panels g,h,i).

This Figure will be added to the manuscript in addition to additions to the paragraph starting on p.9 .23:

“The difference in the spread is also seen to be weather dependent especially when using a low threshold. Figure 4 shows the 5 DU contour line for the forecasts corresponding to Figure 2, for four of the ensemble members. Each of the four members represents one of the perturbed members from the two different model parameterisations and starting times. For the first forecast started there are large differences between the members for areas where they have VCDs above 5 DU. In the second forecast started, the differences between the members are smaller, while the last forecast from 5 Sept 00 UTC shows that, although the members all have plumes with VCDs over 5 DU going south from Iceland, they have quite different positions indicating a different position of the low pressure system. ”

The authors mention that they believe that a part of the observed SO2 plume is not seen by the model because the emission is older than the beginning of the run. Maybe. But it would be very easy to prove it by a run beginning 24 hours earlier.

This has been proven before. The evolution of this plume has been studied in Schmidt et al. (2016) where both satellite and surface concentrations are compared to NAME model results for September 5. Steensen et al. (2016) also studies the evolution of the plume over the three first months of the fissure eruption. The authors agree that the manuscript is not clear from the manuscript and will add the previous studies on the evolutions. P 10 l. 12 will be changed to:

“An area with high SO_2 concentrations in the southwest is not captured by either of the forecasts. Previous studies of this eruption (Schmidt et al., 2015, Steensen et al., 2016) show that this area is affected by older emissions compared to what is included in our model simulations that start 00 UTC 4 September, and is thus not apparent in the model simulations presented here.”

I fully agree with the conclusions on the compromise to find, to launch ensembles only when the weather is unstable etc. But I think this conclusion is too general. All this work (which is huge!) considers only one meteorological situation, one eruption. Maybe the 20x20km is the optimal choice here, but one can not be sure that it will be true under other conditions.

The authors agree that to fully conclude on this, more meteorological situations and eruption styles should be studied than what was feasible in this study and will make the statement less general, and add at the end of the paragraph:

“Ideally more studies should be done, that include other weather situations as well as different types of eruptions to conclude on the best grid resolution.”

Section 4.1

please precise how the ash is distributed over the nine bins, to help the reader understanding how the sedimentation will impact fields.

The size distribution is:

4 µm 16 %, 6 µm 18 %, 8 µm 15 %, 10 µm 13 %, 12 µm 10 %, 14 µm 8%, 16 µm 6 %, 18 µm 7 %, 25 µm 7 %.

, and will be added to the manuscript p.10 l.

“The ash is distributed over nine size bins with characteristic size of 4, 6, 8, 10, 12, 14, 16, 18 and 25 µm and the ash in the source term is distributed among the bins as follows: 16, 18, 15, 13, 10, 8, 6, 7 and 7%”.

Section 4.3

In this section, I feel that the authors are more confident in their model than in the observations! (p 12 line 13 and line 26). I understand they can have some doubts, but I think they should 1) reformulate and 2) ask the people in charge of the observations their expert opinion on the eventual uncertainty of these observations. - It would help to have a (global) idea of the computed gravitational velocity according to the bins. Moreover, the whole study is focused on the position of the ash layer. But does sedimentation impact on the quantity of ash?

Concerning the measurements at Cabauw: Figure 5 in Pappalardo et al. (2013) shows that there were no measurements taken during this time, and the study also commented on that low clouds often prevented observations at this station. The sentence p.12 l. 13 is changed in the manuscript:

“At Cabauw, the first part of the ash plume is not covered by the lidar because no measurements are available, while the second part shows similar simulated and observed level of maximum concentrations.”

For the Leipzig stations, no reason is given for the apparent outliers in Pappalardo et al. (2013), however the centres of mass oscillates from 4 km to 12 km in an unphysical way, and centres of mass or even the height of the ash layer are below 12 km for all the other stations. The sentence p.12 l. 26 is changed in the manuscript:

“In Leipzig a few observations of centre of mass on 16 and 18 April are much higher (at 12 km) than the model centre of mass heights and the corresponding heights at the other stations at this time, implying these high altitude measurements may not represent ash.”

Figure 2 shows the ash concentrations for the model simulation with no gravitational settling, equivalent to Figure 7 in the manuscript. The difference in quantity of ash between the two model simulations with and without gravitational settling is minimal, which is supported by the small difference in centre of ash layer. This is because the size range only includes the very small sizes of ash, up to 25 µm. Model comparison studies where also the coarse ash (2 mm > d > 64 µm) is included in the model simulations show that these larger ash particles fall out before reaching central Europe and the lidar stations (Webley et al., 2012). This is in agreement with the relatively minor fall speed for fine ash particles (d < 64 µm) around 0.01 km/h (Bonadonna et al., 1998; Rose et al., 2001)

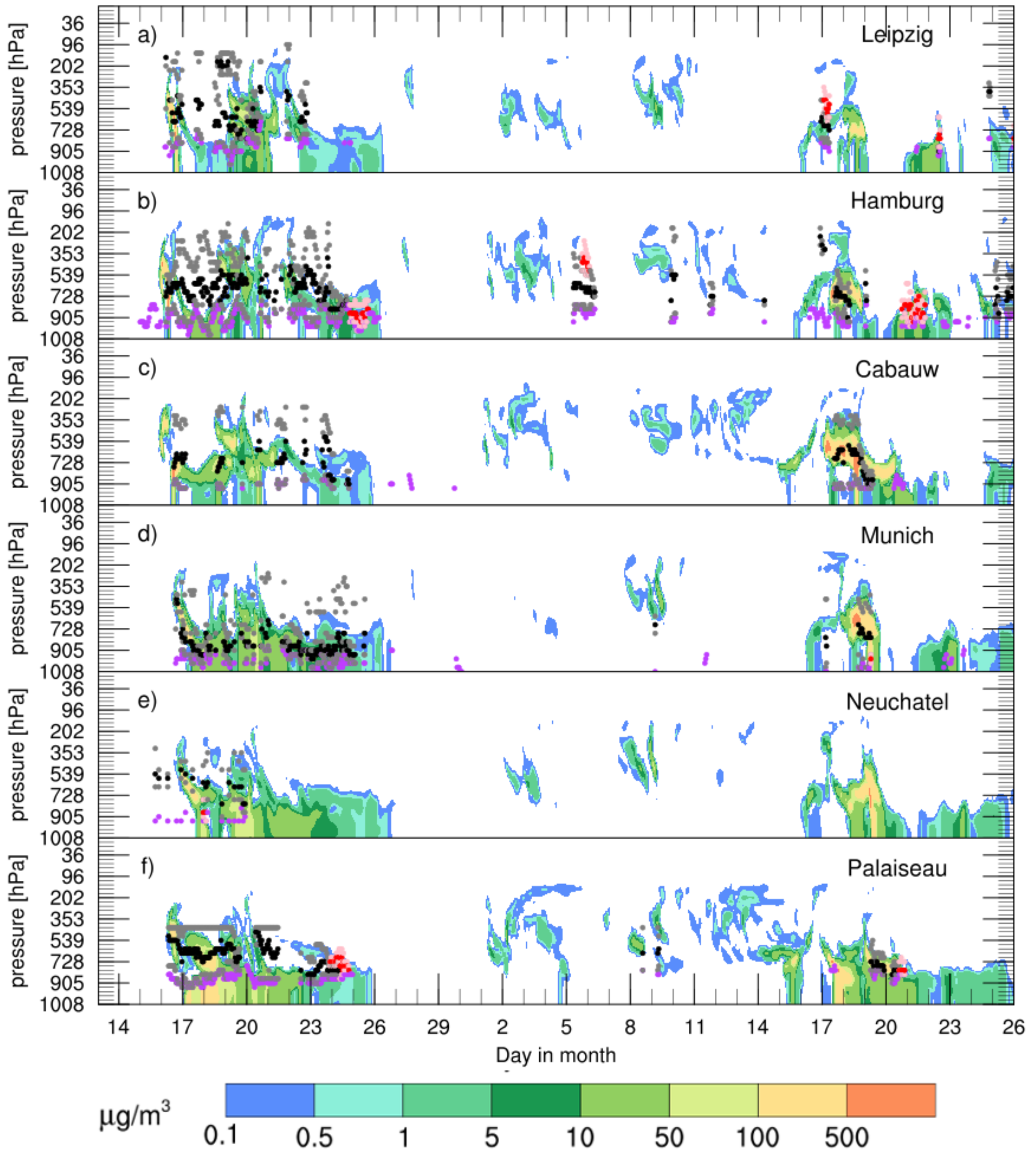

Figure 2: Height-time profiles of ash concentrations from eEMEP model, not including gravitational settling, at the six EARLINET lidar stations in April-May 2010 episode (contour graph in background). Lidar-detected upper and lower height of ash layer is presented as grey dots. The lidar retrieved centre of mass for ash is plotted as black dots. For mixed layers where ash is identified with continental aerosol, the height of the layer is presented as light pink dots, and centre of mass are red dots. The height of the planetary boundary layer is shown in violet. Due to weather conditions and technical difficulties the lidar measurements are not a continuous series.

Typo p5, line 23 : Apart form → Apart from

Changed accordingly.

References:

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