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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Horizontal and vertical structure of the Eyjafjallajökull ash cloud over the UK: a comparison of airborne lidar observations and simulations" *by* A. L. M. Grant et al.

Anonymous Referee #1

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General comments:

The manuscript presents a comparison of observed and modelled volcanic ash as detected by an airborne lidar and as simulated by a transport model, for the eruption of Eyjafjallajökull in April and May 2010. The comparisons evaluate two different vertical profiles of the ash in the eruption column used in the model. A thorough discussion on the eruption source parameters (ESP) is also included. The paper is one of many that analyses this particular eruption, and it is difficult to see the new details and analysis this paper brings that are not already published. The vertical and horizontal structure of the ash cloud and the fraction of ash that survives near source fall out were analysed

by Dacre et al. (2011) and Devenish et al. (2011) (though for partly other periods of the eruption) using the same model and different vertical profiles of the ash, the BAe-146 lidar data were evaluated previously by Marenco et al (2011) and maximum concentrations by Webster et al (2012) also using the NAME model. Comparisons of BAe-146 lidar derived ash concentrations to NAME modelled ash concentrations for some of the same days analysed here were also given by Kristiansen et al. (2012) ((Performance assessment of a volcanic ash transport model mini-ensemble used for inverse modeling of the 2010 Eyjafjallajökull eruption, J. Geophys. Res., 117, D00U11, doi:10.1029/2011JD016844). Therefore it seems that the idea in this paper to compare simulations using two very simplified vertical profiles of the ash in the eruption column to the airborne lidar data is to a large extent already covered by other publications. Also as specified below, the assumptions for these simulations are questioned and need to be re-evaluated. Due to the numerous other papers on the same eruption, and to make sure that the paper is of interest and will be read by others I recommend that the authors clearly highlight the new contribution to knowledge that the paper brings on the top of these other publications. Only after this clarification and subsequent re-evaluation of what to present in the paper that is of new interest can I recommend the paper to be published in ACP.

Specific comments:

The paper concludes that the vertical profile of the ash emissions seems to be more uniform when the eruption is weaker, and that ash is emitted more at the top of the eruption plume when the eruption is stronger (and likely more explosive). This in a way seems logical as larger explosivity presumably emits the ash more condensed to higher altitudes whereas a weaker, diffusive eruption just spews out ash more continuously over a larger fraction of the lower eruption plume. However, other studies that have reconstructed the emission source term of ash for this eruption do not clearly support this. From the paper by Stohl et al. (2011, Figure 2 and 4) the ash is not seen to be more uniformly distributed in the vertical for the low activity period of interest in this paper (4-5 May), but ash is also not found only in the upper 1000m of the eruption plume for the high activity period of interest (14, 16, 17 May). I think one of the main reasons for the discrepancy of the simulation you call top-emission-profile is that you release the ash uniformly only over the 1000 m of the top of the eruption column. As seen from the figures of Stohl et al. (2011) the ash extended over roughly 2-3 km at the top of the eruption plume. Also Kristiansen et al. (2012) (Performance assessment of a volcanic ash transport model mini-ensemble used for inverse modeling of the 2010 Eyjafjallajökull eruption, J. Geophys. Res., 117, D00U11, doi:10.1029/2011JD016844) found that for the period 5-18 May, 54 % of the ash emissions were released over the 2.5 km deep height range of the plume where the maximum emission strength occurred. Therefore, when releasing the ash only over the top 1000 m the model simulation will certainly not capture the whole structure of the down-wind ash cloud over UK. It would be interesting to see a comparison of simulations where the release of ash is over the top 2500 m of the eruption plume rather than over the top 1000m.

Abstract The abstract needs to more clearly highlight the new findings of this study compared to the wealth of other Eyja-papers in this and several other journals. The abstract should also indicate the period of interest to the study (4-17 May). I also think the two last sentences in the introduction (“The comparison is both qualitative...”) would be good to include in the abstract before the second paragraph starting at line 14 on P9126. In addition a more general mentioning of the discussion of ESP would be good to include in the abstract.

3. Lidar P9133, L1-4: I don’t understand how flight times are ignored. If the model output is 1 hourly averaged ash concentrations then the flight time of 30-60 min is within the model output time step and thus is not ignored as such.

4. Results P9135, L1-2: You say that the observed ash layers might reflect the depth of the near source eruption plume. Does this support your choice of a 1000 m deep eruption plume for the top-emission-profile? In the cited relationship ∼0.3H of Carey and Sparks I assume that H is the height of the eruption plume above the vent. Then

H in Figure 1 range 2.8-8.3 km (above the vent at 1.6 km) for 4-17 May, which gives a ∼0.3H range of 0.9-2.5 km. Thus your choice of 1 km depth of the eruption plume is on the low side of this range, and it would be interesting as suggested before to see the results of a simulation using 2.5 km as the height range over where the ash is emitted in the top-emission-profile.

P9135, L6: The text says the ash concentrations are averaged from the surface to 8000m whereas the figure caption says column integrated. Please clarify.

P9136, L19-24: In comparing Figure 5a (4th May) to Figure 3 of Marenco et al (2011) I do not see the same observed ash layers. Particularly the high 6 km layer at 0-150 km distance. Please clarify what part of the flight you have studied and why it looks different to the Marenco figure.

P9138, L3-4: Please add to Figure 5 the results for 4 May for the top-emission-profile simulation as your statement of a better fit to the uniform on this day cannot be evaluated by the reader.

P9138, L6-9: You indicate that the eruption activity was more stable after 5 May. What do you mean by more stable and how does this explain the difference? From the estimated ash emissions from Stohl et al. (2011) the emissions changed considerably after 5 May with strong emission pulses on certain days. Please elaborate what you meant by how this can explain the difference. Also from Figure 4 of Stohl et al. (2011) there seems to be some low emissions (1-4 km) of ash on 4 May whereas most is emitted at 5-6 km altitude, strengthening the suggestion in your paper that some ash was emitted not only at the top 1000 m of the eruption plume. But it doesn’t seem to be uniform either.

Figures Figure 4, 5, 6 and 7: The contour lines are very difficult to see. Please consider to use different colours.

Figure 10: Please indicate which days and flights the various panels show.

Technical corrections:

P9129, L4: Please specify “by the volume of the 3D grid box of the model” or similar. Now it reads as the volume is not defined.

P9129, L6: Change “fallout” to “fall out”.

P9129, L16: Please consider to change “the size distribution of the ash cloud” to “the size distribution of the ash particles” or similar to reflect the size of the particles and not the cloud.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9125, 2012.