Atmos. Chem. Phys. Discuss., 12, C6004–C6007, 2012
www.atmos-chem-phys-discuss.net/12/C6004/2012/

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.

A. L. M. Grant et al.

h.f.dacre@reading.ac.uk

Received and published: 22 August 2012

Reply to Referee 1

Main Comment. ————

The comparison between the BAE-146 lidar observations and NAME simulations in this paper have been carried out to investigate the ability of the NAME model to simulate the structure of the ash clouds. Dacre et al (2011) and Devenish et al (2011) consider a short period in April using ground based lidar observations. The structure of the plume was seen in terms of time variations over southern UK or Germany. In the present study it is possible to investigate the spatial structure of the simulated ash

clouds directly. Also it is not clear that the source characteristics are the same during the two periods and the estimates of the fine ash fraction obtained in this study are useful whether the source characteristics changed significantly.

Marenco et al 2011 describes the method for determining ash concentrations from the BAe-146 lidar. The data in Marenco et al (2011) is obviously the same as that used here, but the comparisons shown were done against the operational configuration of NAME and as stated in Marenco et al the analysis of the NAME situations is more detailed.

The comparisons in Webster et al (2012) are statistical in considering the concentrations at particular times and locations. The present paper has identified position errors in NAME and accounted for them in the comparisons.

Kristiansen et al 2012 focus on estimating the emissions from the volcano using satellite data. Their comparisons are largely limited to the actual flight tracks of the aircraft. The emphasis is on location of ash features rather than the details of their structure.

While the various studies cited by the referee have considered the accuracy of NAME and other models for providing predictions to be used to provide aviation warnings we believe that it is also important to assess how well such models predict the details of the ash cloud structure. This is of interest to a wider range of applications than just forecasting volcanic ash.

Specific comments. ————————

1. It is unclear from the figures in Stohl et al (2011) and Kristiansen et al (2012) to what extent the source departs from being uniform because of the low emission rates. One of the problems in this early period is that the height of the eruption plume is not well defined since it was often not visible to the Keyflavik radar. We have tried to clarify this in the text in that a variable plume height might be best represented using a uniform source, even if at any time the source was not uniform.

2. As the referee states the results in Stohl et al (2011) and Kristiansen et al (2012) suggest that the region of ash emission at the volcano is broader than the 1km we use. Our choice was motivated by the lidar observations which show relatively shallow ash layers. We also show that the ash from NAME are about twice as thick as the observations, and that the comparison between the column integrated mass load and maximum concentrations are consistent with the thickening within NAME. This point is made in the text. It is also unclear how this in the vertical distribution affects the source characteristics determined by Stohl and Kristiansen since they only use the horizontal ash distributions. We have added some text on this, although we have no answer to the question. In some ways being able to raise this question shows the benefit of the detailed analysis presented.

We have rewritten the abstract to make clearer the purpose of the paper.

3. I'm bit unclear on what point is being made, whether we shouldn't ignore the flight time, or whether it is obvious that we ignore it given the averaging time for NAME. We have added some text to explain, to justify not obtaining the NAME results over shorter periods (statistics) and the relatively small effect that that changes over an hour have on the results.

4. We make it clear that the choice of source depth is based on the depth of the layers observed by the lidar. The Carey and Sparks estimate was the only one we could find in the literature. It is only used as a rough connection to other volconological results, rather than a detailed comparison. We have added text to make it clear that we are not attempting to obtain the best simulation from NAME so we make extreme choices for the emission scenarios. Further, in the section on maximum concentrations we point out that the thickening of the simulated ash layer occurs relatively close to the volcano so effectively the emissions are over a greater depth.

5. P9135 L6 Changes made to make figure caption and text consistent.

6. P9136 L19-24. The feature mentioned has been removed. It is a very weak feature

in the lidar and is apparent in the plot of extinction coefficient, but is not apparent in the plots of ash concentration.

7. The referee's comments made us look at the 4th May again. We haven't added the top profile plot because it shows very little. However, we have added a new figure showing the results for the 4th May for both emission profiles as a cross section along 52N.

8. The dotted contour on these plots represents the edge (or very low concentrations) of the ash cloud. It is not intended to be a dominant feature since the main interest is in the shaded areas, so we have not changed it.

9. Figure 10 The plots are all for the 14th May as stated in the figure caption. We have, however, added latitudes and longitudes so it is stylistically similar to Fig 2.

Technical Corrections ————————

10. P9129 L4. This has been done.

11. P2129 L6 This has been done.

12. P9129 L16 This has been done.

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