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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of Eyjafjallajökull volcanic aerosols over Southeastern Italy" *by* M. R. Perrone et al.

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Dear Referee #1, many tanks for reviewing the manuscript and for your comments and suggestions which have contributed to improve the manuscript. The title of the paper has been changed and the paper structure has been reorganized in accordance with your comments Point by point answers to your comments are reported below.

Specific comments:

This study is another one in a longer series of studies which have appeared in this journal (and in several other journals as well) after the eruption of this Icelandic volcano since the second half of 2010. Six of these earlier papers which appeared in this journal are already mentioned. Nevertheless, there are two more papers in this series which

have some relevance to the topic of this new contribution and which should be taken into account by the authors: there is a special lidar paper by Gasteiger et al. (2011) and a paper which discusses the input of the volcanic aerosol into the atmospheric boundary layer and air quality issues (including size of aerosol particles) by Schäfer et al. (2011).

The two papers have been included.

The major issue which is not fully done in this study is the final proof that the observed aerosol particles are really of volcanic origin. There are no in-situ data from higher aerosol-laden layers (aircraft data giving, e.g., particle consistency, chemistry), and there are no other independent proofs of the volcanic origin (e.g., depolarisiation ratios). The only proof offered in this study are the FLEXPART simulations. Although the general weather situation makes it highly likely that volcanic material has reached atmospheric layers above Southern Italy in those days, this is not a strict proof. This deficiency is worth mentioning because the authors claim in the conclusions that this study has been a validation of the abilities of the FLEXPART model. The argumentation goes a bit along a closed circle: the model results are used to identify the arrival time of the volcanic material over Southern Italy and finally the good simulation of the model is taken as a proof for the quality of the model. Another independent data might be helpful here. Maybe the SO2 information given in Fig. 12 may be helpful in this respect.

The paper structure has been changed as it turns out from the marked-copy where all changes are highlighted. HYSPLIT backtrajectories and other published studies have mainly been used to show the arrival of volcanic particles over south-eastern Italy. Experimental data referring to volcanic aerosols are presented and then, they are compared to FLEXPART simulations to contribute to the model validation.

But a comparison of Figs. 5 and 12 gives at least a 16-hour delay in the FLEXPART simulations compared to the surface observations at site G. Maybe, the delay is even

larger because the measurement at site G is made at the ground and FLEXPART shows the first arrival at about 3 km height (Figs. 3b, 3c). It probably took some time until this material was mixed downward to the surface. This would point to a about one-day delay in the FLEXPART simulations. This aspect should be discussed in more detail and should be included into the FLEXPART evaluation.

Looking at Fig. 12, a further interesting detail is that high peak SO2 concentrations are observed earlier at site G than at site C. This is astonishing because site G is further south than site C. It would be nice to have an explanation for this. Do the dispersion model simulations or the HYSPLIT trajectories give any hint why this happens? Or is this due to local boundary layer downward mixing processes? Anyway, it is an interesting detail deserving a bit closer explanation.

The following sentences have been added at the end of Section 3.4 of the revised manuscript:

"Figure 10b reveals that a fast increase of PM10 and SO2 mass concentrations occurred at $\sim$ 06:00 UTC on 20 April and that high peak SO2 mass concentrations were reached earlier at site G than at site C, even though site G is further south than site C. The advection over south eastern Italy of an inhomogeneous cloud of volcanic particles has likely been responsible for these results. Volcanic particles were detected at the CNR-IMAA Laboratory which is $\sim$ 150 km away from site G, since the night of 19 April (Madonna et al., 2010; Mona et al., 2012) and backtrajectory pathways reveal that the backtrajectory ending on April 20, 06:00 UTC at 100 m a.s.l. had crossed the CNR IMAA Laboratory area before reaching site G."

Further technical comments:

The abstract is too long and contains too much details. Please concentrate it to the main facts and conclusions but do not give explicit numbers here.

The abstract has been changed and made shorter as it turns out from the marked-copy

where all changes are highlighted.

p. 15312, l. 13: what is SDA?

It has been defined in the revised manuscript. SDA-Spectral Deconvolution Algorithm

p. 15324, legend to Fig. 1: it would be desirable to have a hint to HYSPLIT in the Figure legend.

Done. The revised legend of Fig. 1 is:

"Fig. 1 8-day back trajectories from the Hybrid Single Particle Lagrangian Integrated Trajectory Model (HYSPLIT) of air masses ending at the Physics Department of the Salento University, Lecce (Italy), (a) on 20 April at 18:00 UTC, (b) on 21 April at 06:00 UTC, (c) on 21 April at 18:00 UTC, and (d) on 22 April at 12:00 UTC."

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C7496/2012/acpd-12-C7496-2012-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15301, 2012.