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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.

Anonymous Referee #3

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Author(s): M. R. Perrone et al. MS No.: acp-2012-387 MS Type: Research Article Special Issue: Atmospheric implications of the volcanic eruptions of Eyjafjallajökull, Iceland 2010

"General comments" The intention of the authors is to compare different instruments and models to characterize the volcanic aerosols from the Eyjafjallajökull volcano, when the volcanic ashes cloud is detected over SouthEastern Italy.

It seems that they have a suitable amount of data from surface and boundary layer, for fulfilling the task and the paper has good potential.

Nevertheless, my opinion is that the paper should be re-organised and in my opinion the title might be reconsidered.

The paper reads more as a comparison/integration of different measurements techniques and model outputs to confirm that the volcanic aerosols reached Southeastern Italy rather than characterizing the volcanic aerosols.

There are different papers published in the literature about detection of the ashes from Eyjafjallajökull transported over Europe and their detection over South Italy; therefore there is little doubt that what the authors observe is of volcanic origin. The paper should be re-organised and focused on the integration of different measurements and comparison/validation of the model.

When discussing the agreement between model and measurements, the authors use too often words as "satisfactory", "reasonable" and "similar" without quantifying. Actually sometime the agreement is a bit doubtful (see the following specific comments on the figures)

The authors also use "for selected hours" without explaining the selection criteria.

"Specific Comments"

1) The Abstract is too long and too detailed i.e. time of arrival etc. It should be optimized.

2) Introduction: Lines between line 27 page 15303 and line 3 of page 15304, are no relevant.

3) Comparison measurements/model: A – In the body of the paper, the authors refer to the FLEXPART model to confirm their measurements; however, in section "Introduction" and "Conclusions" they state that the measurements are of use to validate the model. I think the "interplay" model/measurement deserve to be clarified.

B - Based on the FLEXPART model, the authors note that the concentration modeled over Puglia is lower than the concentration over other parts of Europe. An interesting issue here would be to present and discuss the characteristics of the aerosols and the statistics of the aerosol concentrations in different time of the day, month, year in the

area, especially concerning surface measurements.

4) In Table 1, I acknowledge an increase of the PM10 concentration from 19th of April to the 20th of April; however, this increase is not striking and sites show maxima in different days. I believe that it could be of use to discuss the sites according to geographical position or characteristics i.e. urban, rural etc and discuss the statistics as request in comment 3B) above.

5) HYSPLIT. The authors show the back trajectories from HYSPLIT in figure 1, for different days but some features of the results have not been discussed. As an example, Figure 1a. 20th of April: it is a bit strange that only the 500 m back trajectory arrives from Island. Could the authors please comment this?

6) Figure 5. I am not sure about the “similarity” between FLEXPART MA and AOT from lidar and from AERONET.

7) Figure 11. Where are the dotted and dashed lines in the legend? What are the values shown within the figure? I imagine they are mean daily values.

8) Figure 12. 8a) In figure 12a, I can see high values of PM10 around 00:00 of 19th April; and in figure 12b I note high values of PM10 during 19th April. This is connected to my comment in 3)A about the need to have information of the annual/daily statistics on concentrations at each site.

8c) Figure 12b. How is the high value of SO2 explained at 00:00 on the 20th, at least 18 hours before the arrival of the volcanic plume?

"Technical corrections":

Pag.15306 line 18. AOD is not defined. Sometime the authors use AOT, Aerosol Optical Thickness and sometime AOD Aerosol Optical Depth

Pag 15302 end line 13. “has allowed” should be “have allowed” or “allowed” Figure 1 a,b,c and d. The x axis labels are too close. The figures come from the HYSPLIT run

at the web site but something must be done.

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