Atmos. Chem. Phys. Discuss., 11, C15189–C15195, 2012
www.atmos-chem-phys-discuss.net/11/C15189/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Monitoring of the Eyjafjallajökull volcanic aerosol plume over the Iberian Peninsula by means of four EARLINET lidar stations" *by* M. Sicard et al.

M. Sicard et al.

msicard@tsc.upc.edu

Received and published: 8 February 2012

Answer to Referee 1

Manuscript: "Monitoring of the Eyjafjallajökull volcanic aerosol plume over the Iberian Peninsula by means of four EARLINET lidar stations" (acp-2011-863)

General Remarks: This is a well written paper on the Eyjafjallajokull volcanic plume that reached parts of the Iberian Peninsula as observed by EARLINET lidars and AERONET sun photometers at four stations in Spain and Portugal. The authors show aged and appreciably weakened plumes with optical properties barely registering above background levels.

Reviews and Comments: A major strength of the paper is the use of multiple measurements along with back trajectory analyses to track the plumes and characterize the optical properties of volcanic aerosol at the four stations. The paper's weakness lies in the inability to show convincingly that the observed lidar and sunphotometer derived optical properties, especially the aerosol optical depths and concentrations are well above background levels at the measured altitudes and do represent layers that are not usually found above these stations. Fortunately, this flaw is not fatal and can be addressed by auxiliary measurements in this region. It may be instructive to examine some CALIPSO - CALIOP observations of plumes in this region during the study period -May 5 to 8. In particular, a CALIPSO nighttime orbit that passes close to the IP stations on May 06 near 37.66 N, 7.31W shows some lofted layers with properties consistent with a volcanic plume (Note that while CALIPSO does not classify volcanic plumes, these plumes have properties akin to dust or polluted dust and are classified as such). There are similar weak plumes mixed with clouds on May 08. The horizontal context afforded by CALIOP observations enhance the argument that the observed weak layers are indeed plumes transported over a long distance and predicted by the trajectories shown in Fig. 2.

»»» We would like to recall that in the first version of the paper which was rejected by the editors of ACPD there was a section centered on CALIPSO about the observation of volcanic aerosol plumes from space. Because of the high variability in space and time and the small optical thicknesses of the VA layers observed over the IP our study showed that a systematic comparison was not appropriate. The authors decided to remove this section because it was out of the scope of the paper to address all the editor's points (identification of layers and aerosol typing in CALIPSO, sensitivity of CALIPSO to thin layers, ...). However it is totally true that the "horizontal context afforded by CALIOP observations enhance the argument that the observed weak layers are indeed plumes transported over a long distance and predicted by the trajectories". For this reason the aerosol plumes observed by the two CALIPSO overpasses over the IP during the period 6 – 8 May have been mentioned in Section 3.2 now.

Abstract - part of last sentence should read ‘1.5 times higher’ (you have omitted times)

»»» It has been corrected.

Introduction - you do not cite any work that shows the greenhouse gas emissions decreased significantly during the air travel disruptions. The operative word ‘significantly’ denotes some quantified measure relative to the total global emissions of GHG. If you choose not to cite any work or quantify the GHG decrease relative to global output, then I suggest deleting the word ‘significantly’.

»»» The word “significantly” has been deleted so as not to introduce a quantified measure of the decrease of GHG.

In section 2.2 explain why the aerosol optical depth at Caceres is expected to be representative of conditions at Madrid though the distance of 250 km is larger than scales at which aerosols are expected to be homogenous . Furthermore, Madrid is a much larger metropolitan area than Caceres.

»»» The authors are aware of the lack of representativeness of AOT data obtained at Caceres respect to the Madrid area, both due to the distance (Cáceres is 250 km away from Madrid) and the different conditions of a small town (Cáceres: 100.000 inhab.) respect to a larger metropolitan area (Madrid: 5 million inhab.). Anyhow, the authors decided to show the Cáceres data for completeness of Figure 2, as this is the closest AERONET station to Madrid. Originally, the decision to use data from this station was based on previous studies of long-range-transport events, namely Saharan dust intrusions, where PM10 concentrations from several Iberian Peninsula EMEP stations correlated reasonably well (Escudero et al., 2006). Of course, such long-range-transport events are more homogeneous spatially than volcanic events, so the decision is somehow compromised. Differences between the AERONET station during the volcanic event have been published (Toledano, 2012), although the center of the Iberian Peninsula, where Madrid is situated, is not well represented. A sentence has been added and references given in Section 2.2 saying that the Cáceres AOT is a good indicator of

long-range transport aerosols.

Also, authors need to say something about why they chose to use AERONET Level 1.5 instead of 2.0 at some sites.

»»» Level 2.0 from AERONET at Granada and Barcelona has been released while the paper was in review. We have used those data in the revised manuscript and clarified in the paper that the highest AERONET level available was used: 2.0 for Madrid, Granada and Barcelona and 1.5 for Evora.

Section 3.1 - Part of the last sentence should read ‘several days of unstable’ [instead of ‘instable’]

»»» It has been corrected.

Figure 2 is difficult to read. I do not know how much control you have over these plots but please try to improve the legibility of the axes labels, and the sharpness of the tracks. The labels for time and altitude are not legible. In addition, mark the location of the volcano on the maps to aid the reader verify the origin of the plumes. Finally, the plumes takes a shorter time to get to Granada (3 days) than say Evora (4 days) yet the authors use a standard 5 days for all sites. I suggest a number of days back that will ensure plume arrival at each site to exclude irrelevant tracks and regions (N. America, Africa etc).

»»» A similar comment has also been made by Referee 2. In the revised Figure 2 the lowermost parts have been removed. All the subplots have been re-drawn with thicker lines, labels with a better resolution, fixed projections (min lat., max lat., min long., max long.), . . . A red star indicates the volcano location. The minimum number of days back that ensure plume arrival at each site changes a lot with the day and the arrival height. For example, it is true that 3 days are enough at Granada at the beginning of the event but on 8 May 4-5 days are necessary to check the origin of the plume arriving at 500, 1500 and 2500 m. For this reason the backtrajectories have been maintained of 5 days

but the projection plots have been restricted to the area defined by 40W – 10E and 25N – 75N independently of the tracks continuing towards North America or Africa.

Section 3.2 Figure 3 is introduced but not sufficiently discussed. The authors should explain why the measurements are so discontinuous. For example, why does Barcelona have only a few hours of lidar measurements on the 5th and 8th of May?. Why are there no range squared corrected signals (RSCS) on May 6, 7 and sunphotometer measurements on May 6? Why are the measurements at Madrid so spotty? Have the authors excluded cloudy scenes? If so, I suggest including all measurements (both cloudy and clear) for completeness. You can then present a companion Figure without clouds if you so choose.

»»» The text now clarifies that all the measurements in both cloudy and clear scenes are plotted. None of the four systems used in this study perform automatic, 24h/day measurements. In Barcelona only very few measurements are available 1) because the system was in parts for upgrade and had to mounted in an hybrid configuration for the monitoring of the volcanic aerosols and 2) because of the lack of manpower (the system operation requires an operator). In Madrid the nighttime measurements are acquired with an automated software that fires the laser for 1 hour and switch it off for cooling for 2 hours. This is the reason why the Madrid measurements look so spotty.

The authors state that the presence of clouds prevented further analyses. Are these clouds completely opaque and impenetrable by the lidar signal? If the volcanic aerosol (VA) is not embedded in the cloud, is it not possible to analyze a VA that is above or below a cloud? I suspect it is albeit with a degraded signal, higher uncertainties, and without a corresponding sunphotometer measurement. With reference to Table 2, discuss why there were no measurements at Barcelona on the May 5,6, 7 and Evora on May 8.

»»» During the period 5-8 May when clouds were present, 2 situations occurred: 1) the clouds were completely opaque and impenetrable by the lidar or 2) the clouds were

at the altitude of the VA layers and mixed with them. In the latter situation we did not perform any further analysis because we wanted to avoid situation of mixing of VA particles and droplets and to limit our investigation to pure VA layers only. A sentence, line 14 of Section 3.2, explains this choice. On May 5 a 3-hour measurement was performed in Barcelona but no VA was detected because of the presence of clouds. This is the reason why no VA information appears in Table 2. It is discussed in the text, see line 26 of Section 3.2. On May 6 and 7 no measurements were performed. In Evora no measurements were performed on May 8 (a Saturday) because of manpower shortage.

Section 4. Are these the only 3 cases with good quality inversions? If not say why the other good quality inversions are not included in these intercomparisons?

»»» The 3 cases shown in the paper are not the only good quality inversions available. There are a selection made out of the good quality inversion based on a chronological criterion during the most intense period (7-8 May) and sowing profiles from all stations (for Barcelona there were no other choice than showing the profile on 8 May at 1600 UTC). The number of cases was limited to 3 in order not to make the paper too long.

Section 4.1.1 Authors give an explanation for low lidar ratios as being a result of dehydration of the air mass. These should lead to the size distribution shifting to smaller mean radii. Can this be confirmed by the angstrom exponents and/or AERONET size distributions? Is a better explanation that the aerosol has lost a significant portion of its coarse component by sedimentation in transit and the effect of the remaining fine particles is dominant?

»»» Since it was a nighttime measurement there is no AERONET coincident measurement. However between the last AERONET size distribution on 6 May and the first one on 7 May no significant change is noticeable between the fine and the coarse mode. The explanation that a significant portion of the coarse mode has been lost by sedimentation in transit and therefore that the dominant mode is that of the remaining fine

particles is totally plausible, but how to connect this explanation to low lidar ratios? If we assume that the remaining fine particles were mainly non-ash particles which have a typical lidar ratio of 60 sr then higher lidar ratios would be expected, not lower. Because this explanation does not lead to the results observed (low lidar ratios) we have preferred to mention only the hypothesis of the dehydration of the air mass. However we do state that it is “one possible explanation”, not the only one.

Estimating mass concentration: The paper Tesche et al.(2011) does not seem to be accessible. However, I find the paper Ansmann et al.(2011) to be quite adequate in explaining this method. I suggest omitting the reference to Tesche et al. (2011) since it is an extraneous reference.

»»» The reference to Tesche et al. (2011) has been deleted as suggested.

Concluding Remark: In general, the AOT values are very low. If these led to air travel disruption in the IP the authors should point out this societal inconvenience and overreaction by policy makers.

»»» A final sentence has been added at the end of the conclusion to emphasize this comment.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C15189/2012/acpd-11-C15189-2012-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 29681, 2011.