Answer to Referee 2

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

The paper by Sicard and co-authors is a nice and thorough description and analysis of measurements performed by lidar and sun-photometer over the Iberian Pensinsula in May 2020. The manuscript is clearly structured and easy to understand, and the observations will be a valuable brick-stone for the scientific community. Nevertheless, the measurements are not well put into context with other observations (satellite, e.g. SEVIRI, CALIPSO, mm) and model results. See e.g. Comparison to publications showing satellite-retrieved and modeled ash, e.g. also from on 8 May at 04:00 UTC.00 (see Stohl et al., Atmos. Chem. Phys., 11, 4333–4351, 2011 (see Fig. A1, http://zardoz.nilu.no/_sabine/MAY.gif). Parts of the data have been already shown by Toledano et al. (2011) [AOD and Ångstrøm coefficients from the AERONET sites & one selected lidar scene from Granada], and it should be made clear how the interpretations and conclusions drawn shown by Sicard et al. compare to their results. Although there is a slight repletion of the sun-photometer data set, Sicard use the distinction of AOD in fine & coarse mode from O’Neil, which clearly gives an added value for the discussion.
>>>>>> As far as CALIPSO is concerned, Referee 1 also made a comment in that sense. Here is our answer: “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.”
The paper by Stohl et al. (2011) is a very interesting paper. It has been added in the references.
The horizontal context given by the images of MODIS, IASI and SEVIRI as well as by FLEXPART simulations has been commented day by day (for 7 and 8 May) in Section 3.2. In addition to CALIPSO profiles, it gives a new insight of the overall picture of the situation.

Specific comments/questions
Abstracts: Question: The layer at 11-12 km, is that already in the stratosphere ? The last sentence describing … and “probably did not exceed the value” – this sentence should be rephrased to a more clear scientific statement…

>>>>>> According to the Madrid radiosounding launched on 6 May at 00Z, the tropopause is around 10.6 km, so that 11 – 12 km is no longer in the troposphere. This clarification has been made in the text, not in the abstract because the latter gives general information about the results without distinguishing between troposphere and stratosphere.
"In Granada the ash mass concentration […] probably did not exceed the value of 200 µg•m-3 during the whole event." is a highly probable hypothesis based on the fact that the case selected in the morning of 8 May had one of the highest optical thicknesses. Because the measurements were discontinuous there might have been intrusions of volcanic plumes denser than the ones observed. For this reason the statement can not be made with more security at this point. The sentence has been left as it is. If the referee does not agree to leave a highly probable hypothesis in the abstract it could be replaced by "In Granada the ash mass concentration […] did not exceed the value of 200 µg•m-3 during the lidar measurement."

Section 1, Introduction:
The event might have contributed relative quickly to the decrease of the global surface temperature since … Has this been observed for this particular event ? Can you include any reference ?
>>>>>> This part of the introduction was a very general statement. It has been totally rewritten. It says now: "From a climate point of view, since no significant amount of ash and sulphur dioxide was injected into the stratosphere, their residence time in the atmosphere was rather small compared to other recent eruptions (Parker et al., 1996). Thus the impact of the Eyjafjallajökull eruption on the Earth radiative budget and climate is very unlikely.".

"On the contrary the residence time of sulfate aerosols is much longer for they can resist in the atmosphere for several months" Is this general in the atmosphere – stratosphere-PBL/moisture, …?
>>>>>> It is a general statement which is valid in the atmosphere. The statement of the sentence has been "smoothed" and rephrased as: "On the contrary the residence time of sulfate aerosols is in general longer for they can reside in the atmosphere for several weeks or months.".

Section 2. A topographic map showing the lidar and sun photometer sites in the Iberian Peninsula will be helpful to understand the special variations and typical meteorological situations, as those described in section 3.2 (Over the central plateau …)
>>>>>> A topographic map of the lidars can be found in Sicard et al. (2009; 2011) cited in the text. The position of the Caceres sun-photometer compared to the other sites can be found in Toledano et al. (2011) also cited in the text. The position in the Iberian Peninsula of each of the four lidar stations has also been indicated in the backtrajectory subplots by a dark star.

The 16 trajectory subplot of Figure 2 are too small and should be renewed (crop of the area and remove the lowermost parts).
>>>>>> This was also a comment from Referee 1 (please see the corresponding answer). 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.), …

Figure 3 should be larger as well. The layer in the lidar RSCS are very hard to distinguish
>>>>>> A similar comment was made by Referee 3. There are several things to take into account if we want to maintain Figure 3 as it is now: we are dealing with 4 days of measurements going from clear to cloudy skies and from volcanic aerosol layers of 200-m thickness and AOT < 0.005 and layers of ~2-km thickness and AOT on the order of 0.1, in average. Putting in evidence all the volcanic aerosol layers in Fig. 3 would require a different colorbar for each measurement. However the chronological plots of the RSCS as they are shown in Fig. 3 are represented with a single colorbar per station for the sake of clarity and for comparison purposes, so that a compromise has to be made in the selection of the colorbar. We have chosen to lose some information at the beginning of the event when the layers are very thin and to enhance the contrast starting on 6 May. In the revised manuscript Fig. 3 has been enlarged to fulfill an A4-size sheet and its resolution/quality has been improved.
If the referees prefer, the authors could change the format of Fig. 3 that would result in a loss of information that the authors would like to avoid. It would be possible to make two panels and zooming in the 2 most intensive periods of the event, e.g. Évora-Madrid on 6 May and Évora-Madrid-Granada on 7 May. But again the authors would prefer not to lose the continuity of the plots as they are presented now in the current manuscript.

Section 2.2/4: A discussion on capability of sun-photometer AERONET data to detect ash would be valuable. The authors are using cloud-screened data from AERONET (lev. 1.5). Can ash be miss-classified as clouds and removed by this process (e.g. SEVIRI data show an ash laminae passing over Barcelona, 8 May).
>>>>>> A question of the level of the AERONET data used was also raised by Referee 1 and 3. Please see answer to Referee 1. 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.
The ash laminae detected by SEVIRI over Barcelona on 8 May at 1000 UTC (Fig. 4 from Toledano et al. (2011)) coincides with an increase of the sun-photometer AOT at the same time (Fig. 3 of our manuscript). The Barcelona lidar measurement at 1600 UTC detects a very optically thin plume between 1.9 and 3.1 km while SEVIRI data (see the referee's reference: http://zardoz.nilu.no/_sabine/MAY.gif) show no more ash at the same time.

As for the discussion on capability of sun-photometer AERONET data to detect ash we now refer in the text to the paper of Toledano et al. (2011) in which the question is extensively covered. Indeed in this paper they show how the sun-photometer products reflect the presence of ash over the Iberian Peninsula evidenced by lidars, satellite sensors and modeling. A sentence has been added in Section 2.2 in that sense.

Section 4.1: The hydration thesis is unclear and should, if possible be further analyses (humidity available ?). Can smaller sulfate particles explain the behavior.
>>>>>> The same comment was made by Referee 1. Please see the corresponding answer:
The explanation that a significant portion of the coarse mode has been lost by sedimentation in transit and therefore that smaller sulfate particles dominate 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.
We have also made a second hypothesis suggested by Referee 3 that a possible mixing with sea salt might also explain the lower values of lidar ratio observed.

Conclusions: One chapter should be added, discussion the measurements in the context with published observations (satellite, e.g. SEVIRI, CALIPSO, mm) and model results.
>>>>>> Please see also the answer to the very first comment of this review.
At the end of the first paragraph a sentence has been added stating in general terms that the presence of volcanic aerosols was also supported by satellite observations and model simulations. The authors think it would be inappropriate to include in the present paper a full paragraph in the conclusion about the agreement/discrepancy of our measurements with satellite observations and model simulations.

Technical corrections: - Please, check some of the flowery language in the abstract: the volcanic plume "hit"… >>>>> Replaced by "reached"

"Punctually" "… >>>>> Removed.

on - I would recommend removing the abbreviations VA in section 1 (it's not a standard abbreviation and makes it harder to read) "… >>>>> Done.

Introduction: The sentence: 'We ONLY concentrate on lofted VA plumes, … and because the distribution of the VA plumes in the troposphere is of great interest for air traffic.' Also layers in the PBL might be of interest for air traffic, the sentence needs to be

rephrased. "… >>>>> It is true that volcanic aerosol layers are of interest for air traffic at all heights. The last explanation has been removed.

Section 2.1 Coordinated measurements … and intensified "accordingly" with the intrusion. Use another word… >>>>> Replaced by "as the intrusion strengthened".