General Comment

The paper reports lidar vertical resolved observations of the volcanic aerosol over the Iberian Peninsula. These observations are interesting because cover a region poorly investigated concerning the Eyja eruption. On the other hand, lidar measurements are really valuable for studying the vertical stratification of the volcanic cloud.
Nevertheless, the presentation is confusing and results are not well presented. Data presentation should be strongly improved for the publication on ACP. In the present shape, reader could doubt about some data reliability.
These are the main questionable points:

- For sun-photometer data, authors do not use always data of the best available quality level.
- The performances of the lidar systems in terms of retrievable properties and related errors are not reported and are not available in the referenced literature for some stations.
- Lidar profiles are shown in such a way that readers cannot judge their reliability (see details reported in the following).
- Questionable differences are reported for almost near stations in terms of intensive optical properties. These differences should be explained and discussed in more details.
- Discussion of optical properties is often confusing

I suggest a careful revision of the paper for taking the most from the data and giving them the right visibility.

Specific comments

Page29684 Line 7:
air traffic was blocked over Southern Europe also in other periods. Maybe you refer only to Iberian Peninsula.

Page29684 Line 9:
Here there is a precise quantification of the cancelled flights and passengers involved. This kind of statement requires a reference. Otherwise authors should avoid this level of precision.

Page 29685 Line5-8
Cryptic sentence. Re-write it in a more readable way. About its content: what is the source of this information? Please include references. Or are these already results of the paper? If it is the case, this is not the right place to report it.

Page 29685 Line 15:
Authors state that these observations are interesting for testing models at boundary conditions but they say this was already done. This is misleading and gives the impression that this paper is completely useless. On the other hand, is the work reported in Molero et al. still in progress and will lead to the publication on a peer reviewed journal beyond the cited proceeding?

Page 29687, Lines 1-2:
How are the performances of the systems? In table 1 you reported the characteristics of systems but without any details about their observational capabilities. The cited abstract is not available online so it is impossible to the readers understand the quality of the systems. It is essential for the reliability of presented results that some numbers are provided: covered altitude ranges, absence of instrumental errors, typical performances and statistical errors.
"*systems…were satisfactorily compared*": the authors should at least report the most important results about the inter-comparison campaign as the altitude region where the systems are reliable

and their degree of accuracy at different altitudes level for aerosol backscatter and extinction coefficient.

Page 29687, Line 6:
Authors state that *"All nighttime measurements were also inverted using the Raman lidar inversion algorithm"*. What does it mean? Authors have two different inversions for each night-time measurements? And which is at the end used for the overall analysis?

Page 29687, Line 11:
For Madrid, sun-photometer instrument is located at 250 km far from the lidar station. Taking into account that Madrid is a big city, probably large differences should be expected at these distances. The appropriateness of combining lidar and sun-photometer data for so distant observational points should be discussed.

Page 29687, Line 20:
Angstrom exponent are also provided within AERONET products. Why do you calculate them from AOT? Are there any difference?

Page 29687, Line 21:
Why level 2 data are not available for those stations after 1.5 years? Indeed I checked the current database of AERONET and Barcelona and Granada has level 2 data for May 2010 and they should be used instead of level 1.5.

Page 29688, Line 18:
Authors should explain why they use 1200 UTC as representative for the whole day, if it is the case. Otherwise they should better explain that backtrajectories are reported as example but of course they are checked case by case accordingly to the measurement timing.

Page 29688, Line 13:
Figure 3 is not readable in term of RSCS structures and features are not visible at all. Nevertheless there are strange different colors reported for Madrid. Also AOT and its components are not well distinguishable into these plots.

Page 29689, Lines 21-23:
Some of the involved stations have Raman capabilities. Why authors do not reported really measured AOT? The 50 sr assumptions should be discussed and justified. Is this assumption in agreement with your results reported in the Section 4?

Page 29688, Line 27:
Typo error. Add daily mean **of the**

Page29690, Table2:
It is reported in table 2 that wide volcanic layers are observed while in the abstract it is reported that *"The volcanic aerosol layers observed over the IP were rather thin (< 1000 m)"*

Page29690, Line 12:
What does it mean here *"Taking into account the low values of* AOT_{VA} *found, those uncertainties are reasonable"*? Reasonable for what?

Page29690, Line 20:
This is also supported by the low AOT _VA

Page29690, Line 23:
Did the thermal low persist also during the following days? Is this in agreement with layers as observed by Madrid stations and reported in figure 4?

Page29690, Line 28:
Please report here that you are referring to Madrid

Page 29691, Line2-3:
Madrid reported observations are not linked in any way to the rest of the paragraph. Please comment on them.

Page 29691, Line 12-14:
Authors report about a new layer, but it is not visible in the RSCS in the current version.

Page 29691, Line17:
Is the cirrus discarded in VA AOT calculation? Maybe the strong AOT in VA reported in figure 4 is related to this case.

Page 29693, Line 10:
Raman inversion is limited but not the others, rephrase it in order to avoid giving the impression that during night-time nothing can be provided.

Page 29693, Line 11:
Are these cases selected also because of interesting situations or are these the only profiles you have?

Page 29693, Last line:
You should motivate the choice of lidar ratio

Page 29693, Figure 5:
This figure should be significantly improved. Profiles should cover the calibration range too and the low troposphere (above the overlap region, of course) to clearly show the quality of reported data. Moreover the high lidar ratio values observed over Madrid should be discussed in more details by the authors. Error bar on lidar ratio is very high (about 50%) and it is clear looking the figure that S in the central part of the layer is around 60sr.
A layer has been identified and its boundary reported for the Evora profile. What about the Madrid one? In the calculation of mean lidar ratio authors used also the values around 20 sr obtained at about 7 km asl?
How do authors explain the different behavior of Angstrom exponents with the altitudes? Extinction related Angstrom exponent suggests that particles below 3 km are larger than those at upper levels. But backscatter related Angstrom exponents show exactly the opposite.
What about other wavelength backscatters for Madrid? It could provide indication about the size of the particle if compared to the Evora measurements.
Error in figure4 caption: the 532/1064 is backscatter and not extinction angstrom

Page 29694, lines 12-end:
It is very difficult to follow this discussion. Authors report that lidar ratio values are *rather small compared to recent studies (Ansmann et al., 2010; Mona et al., 2011; Wiegner et al., 2011),* but then refer to Mona et al., for justifying it. In that paper it is reported: *the lidar ratio increasing with*

RH from about 40 sr at 20% as RH up to about 70 sr at RH of about 70% …..suggest the presence, besides sulfates aerosols, of some ash affected by the aging through the European continent I would expect a completely different situation over Iberian Peninsula: in this case there should be not contamination with continental aerosol, maybe with marine aerosol. This could justify the smaller lidar ratio values.

Page29695, Line 26:
This is a different phase of the eruption so that completely different particles in terms of depolarization could be present. However here you just want to say that it is reasonable you have a mixture of ash and no-ash and this is clear from the dep value as itself also without any comparison.

Page29695, Line 20:
I tried to understand something about these lidar ratio values and their ratio. But also this part is difficult to read. Authors report "*according to Mona et al. (2011) a ratio of lidar ratio values around 1 suggests the presence of both aerosol types: non-ash and aged ash.*" I found in Mona et al that "the lidar ratio increasing with RH from about 40 sr at 20% as RH up to about 70 sr at RH of about 70% and the ratio of lidar ratio values below 1 suggest the presence, besides sulfates aerosols, of some ash affected by the aging through the European continent." and "High S_{uv}, particle linear depolarization ratio increasing with RH and values of the ratio of lidar ratios greater than 1 suggest the presence of volcanic sulfates/continental mixed aerosol." I found no information about values around 1.

Page 29696, Line 1:
There are big differences in observations reported for Germany and Italy, consisting respectively of mainly ash and mixed ash/no-ash particles. Again authors are providing a confusing and misleading discussion of their results. Please try to be more clear and precise.

Figure 6-7:
Same comment of figure 4 applies also to these figures: it is not good to show profiles extending only over 1-2 km of altitude range. Why the Barcelona profile do not extend, for example, up to the beginning of Granada ones? If Barcelona profile is not reliable above 3 km how it was calibrated?

Page 29698, Line 8:

As authors pointed out mass concentration is an important parameter and there is a strong request from no-scientific community to have this kind of information and conversion factors between optical properties and mass concentration. However, the volcanic plume analyzed by Tesche t al., 2011 is completely different from what observed over Iberian Peninsula: different eruptive phase, different transport path, different potential mixtures. Authors should at least comment on the uncertainty related to the pure ash and pure non-ash particle depolarization ratios assumed values. The same applies also for density and S values.

Page 29702, Line 20:
Yes, but how large is the systematic error on backscatter? Which are the S and calibration contribution?

Page 29703, Line 9:

As above, how large is the statistical error on backscatter? Furthermore I do not understand the need for maximizing the error in particular the statistical one. You could calculate it precisely. I do not see the rationale for this Appendix, if at the end the error sources are not quantified.