The authors thank both referees for their constructive and detailed reviews of our article. Answers and modifications made to the manuscript are developed (in blue) below. Note that updated and new figures are included in our response in the 'Interactive Discussion'.

L. Mastin (Referee)
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This paper describes an advanced method to estimate SO_2 emission during eruptions and to model the dispersion of SO_2 clouds using an inversion technique that optimizes the fit between the model and the observations. The study uses multiple sets of SO_2 measurements from the April 10-11, 2011 eruption of Etna volcano and shows that ground-based measurements of SO_2 underestimate SO_2 flux by up to an order of magnitude when the SO_2 cloud becomes optically thick or is partially obscured by ash. This conclusion reinforces earlier studies suggesting that SO_2 could be underestimated by UV cameras looking into optically thick clouds [e.g., Kern et al., 2012].

I'm not a specialist in remote sensing techniques and therefore can't comment on the details of the methodology. But overall the article is clear, meticulously written, the main conclusions appear to be well supported by the data presented in the article, and the conclusions are of enough significance and broad enough interest to merit publication. I have only a couple of suggestions concerning broad topics and several specific comments, mostly trivial, concerning fine points of English.

Points of slightly more than trivial importance:
1) I had some difficulty following the explanation on p. 5040 of how the altitude is independently determined from IASI observations. The main point of uncertainty concerns what terms are in the Jacobian, mentioned on lines 17 and 18. Since this technique is already described in Clarisse et al. (2014), hopefully the clarification can be just a matter of adding a sentence or so.

We have clarified the language and added a couple of sentences in the relevant section, clarifying in particular that the weighted projection is a vector projection of the observed IASI spectra onto 30 Jacobians. Explanations of how these Jacobians were calculated numerically have been added. The full paragraph now reads (changes in bold):

"The altitude of the SO_2 cloud is retrieved from IASI observations using the algorithm outlined in Clarisse et al. (2014), which estimates the altitude independently from the column. Here we summarize the main features of the algorithm and refer to the aforementioned study for full details. Central is the use of a response function **f(z, S),** which is a weighted **vector** projection of the observed **IASI** spectrum **S** onto different Jacobians **representative of perturbations of SO_2 at different altitudes z.** The retrieved altitude corresponds to that altitude z for which the response function reaches its maximum. **The Jacobians at a given altitude were calculated numerically with the finite difference method, i.e. from the difference of two forward modelled spectra with and without SO_2 at that given altitude, and with the rest of the atmospheric parameters representative of the geographical area we wish to study. For this study 30 Jacobians were calculated, representing SO_2 perturbations from 1 up to 30 km."**

2) On page 5053, your discussion of Fig. 10 would be more quantitative if you gave the r^2 value for the regression.

A linear regression has been performed after having removed outlier values from the dataset of IASI altitudes. Outliers are identified using the following criteria, as discussed in the text (Section 4.2) : (1) pixels associated to very low SO_2 CA values (<0.3 DU), mostly found on the edges and in the tail of the dispersed volcanic SO2 cloud; (2) a few isolated IASI pixels with both abnormally very low retrieved altitude (≤ 2 km) and large SO_2 CA (> 4DU) which result from a failure of IASI retrieval algorithm.

Fig. 10 (Fig. 11 in the updated paper) has been updated to highlight these outlier values which are now represented by open circles.

An inset in Fig. 10 (Fig. 11 in the updated paper) has also been added, which includes the linear regression model. The correlation coefficient of the regression is found to be $R^2 \sim 0.6$. However, it is difficult to judge of the significance of the correlation solely based on the value of R^2. A slope near to unity (precisely equal to 0.93) is found, showing the nearly one-to-one proportionality between IASI and modelled altitudes. A Y-intercept of 1.3 km is found, which is small given uncertainties on IASI altitudes of 1-2 km and the resolution of the CHIMERE CTM vertical grid.

Also, on lines 25-26, I was confused by the reference to a cloud image in Fig. 10, since Fig. 10 shows no cloud image.

Indeed, the reference to Fig. 10 is wrong: Fig 6d is the right reference.

Specific, mostly trivial points: Page 5032, Line 23: change “confronted” to “compared” done

Page 5034,
– line 11: delete “which is still going on at the time of writing” (the Bardarbunga eruption is now over). This has been updated.

Page 5034,
–line 18, change “On its hand” to something like “additionally” (“On its hand” is not a normal English expression). And reword this sentence. It doesn’t make a lot of sense as currently written. “On its hand” has been replaced by “For its part” and the sentence has been reworded.

Page 5037:
–line 12: is CHIMERE Eulerian or Lagrangian? Eulerian as specified.
–line 18: If you’re referring to the NCEP/NCAR Reanalysis 1 data, the appropriate citation would be Kalnay et al. [1996]. This reference has been added.

Page 5039:
–line 11: is roughness the meta-parameter you refer to in line 10? Lower roughness means less smoothing?

Roughness is indeed the meta-parameter we referred to. Higher roughness means less smoothing. For clarification, this has been added to the text.

Page 5040:
–lines 16-19: If you are developing a weighted projection of the observed spectrum onto different Jacobians, does that mean that you were looking at the partial derivative of each specific wavelength to see how much its amplitude changed for a given small perturbation in SO_2 at a given altitude? Are the partials in the amplitude of a given wavelength the response function you mention? Perhaps some rewording would clarify for non-specialists like me.
See above (point 1) for further explanations on the vector projection on Jacobians and the modifications brought to the manuscript for clarification.

Page 5043:
–line 3: change “thiner” to “thinner”. done

Page 5044:
–line 19: the wording in this sentence seems awkward and unclear. Perhaps replace “maps, which is” with “, “ to clarify? done
–line 23: change “model” to “modeling” done

Page 5045:
–line 16: what do you mean by “a stretch of the blue color of these radiances”?
The histogram which represents the distribution of the number of pixels at each radiance level, for the blue channel of the MODIS RGB image, has been stretched to enhance fainter parts of the image and provide a higher contrast. Thanks to this image processing, we are able to visualize two Etna plumes including the weakest one associated to low radiances in the RGB image. The initial explanation was indeed very short and we have added to the paper additional developments in Section 2.2.2 and 3.2.

–lines 19-26: The agreement between the hysplit trajectories and the visible plumes Fig. 5b is indeed very good. It seems surprising that you would have sources at two discrete heights; 4 and 7 km, rather than a continuous source that extends from the vent up to the maximum elevation. The lower one is the vent elevation and the upper one is the top of the plume?
What is referred in the text as ‘the altitude of emission’ corresponds to the altitude at which the plume starts to be horizontally advected and dispersed. Fig. 3 shows that emissions can be simultaneously injected at different altitudes (here at 4 and 7 km) according to inverse modelling. While volcanic material is continuously emitted in time, we distinguish two phases in the eruption at ~ 09:00 UT and 11:00 UT. During these two periods, most of the volcanic material (in terms of mass) is respectively emitted at an altitude of 4 km (e.g. ~700 m above the vent) at around 09:00 UT and at an altitude of 7 km later at around 11:00 UT (corresponding to the paroxysmal phase of the eruption). This result of the inversion scheme is validated by forward HYPLIT trajectories in Fig. 5- right.
An analysis of uncertainties on HYSPLIT trajectories, estimated by varying (by +/- 1 km) the altitude of emissions set as inputs to HYSPLIT runs, has been added to Fig. 5 and discussed in Section 3.2. This analysis indicates that a volcanic parcel emitted at a slightly different altitude at 09:00 UT (either at 3 or 5 km) would follow a very different path. Therefore, the RGB MODIS image confirms the emission altitude of 4km estimated from the inversion. In contrast, volcanic material emitted at 11:00 UT at a slightly different altitude (at 6 or 8 km) would follow a similar

direction as the one observed on the RGB MODIS image. In this case, the RGB MODIS image does not allow for a tight control on altitude. However, emissions at 6, 7 and 8 km would take up a very different advection velocity, leading to very different locations of the distal plume at a given date of observation. IASI images acquired on 11 April allow for determining the optimal altitude (around 7km in this case), as shown in Figure 7.

This uncertainty analysis on HYSPLIT computations consequently confirms the results of the inverse modelling procedure, which show that the horizontal spreading of the volcanic plume does not take place uniformly at all altitudes. The mass centre of emissions may shift to a different altitude with time.

Page 5047:
–line 7: change “others” to “other”. done
–line 13: change “Lybia” to “Libya”. done

Page 5050:
–line 3: change “associate to” to “associated with”. Same on line 10. done
–line 19: change “consist in” to “consist of” done
–line 20: change “have already settled” to “had already settled” done
–lines 23-24: change “over night” to “at night” done

Page 5051:
–line 2: change “like in Siberia” to “as in Siberia” done
–line 8: is there a reference that can tell us what the “row anomaly” is for the OMI sensor? A more detailed description of the OMI row anomaly is available at http://www.knmi.nl/omi/research/product/rowanomaly-background.php. This address has been referenced in the text.

Page 5052:
–line 2: change “relatively to SO_2” to “relative to SO_2”. done
–line 26: change “instrument” to “instruments” done

Page 5053:
–line 4: change “a an automatic” to “an automatic” done
–lines 8-9: You say that there is good agreement between SO_2 cloud altitudes predicted by the inverse method and the altitudes derived by IASI observations, illustrated in Fig. 10, but You don’t give the regression rˆ2 or show the slope of the best-fit line in Fig. 10.

As explained above, details on the linear regression model (slope = 0.9, Y-intercept value=1.3, correlation coefficient=0.6) have been added in Fig. 10 (Fig. 11 in the updated paper) and the text has been modified accordingly.

–line 16: change “difference” to “different” done
–line 17: change “on altitude” to “in altitude” done
–lines 25-26: “They match the forefront of the volcanic SO_2 cloud in the 11 April p.m. image (Fig. 10)”. I’m not sure what this sentence means. There is no image of the cloud in Fig. 10.

It is indeed not clear because the figure which is referenced is not the correct one: it should be Fig. 6d. This has been corrected.

Page 5054:
–line 12: change “caveats” to “pitfalls” done
–line 19: delete “an” before “information” done

Page 5056:
–line 6: change “associated to” to “associated with” done
–lines 22-23: “The altitude of SO_2 emissions retrieved by the inversion procedure are confronted to the forward trajectories of the HYSPLIT Lagrangian model”. I’m not sure what you mean by “is confronted to”. I think you mean that you used the altitudes as input to Hysplit trajectory runs? (Fig. 5)?
Yes, that is it. Sentence in line 22-23 has been reworded.

Also, change “are” to “is” in this sentence (or change “altitude” to “altitudes”). done
.
Page 5057:
–line 10: change “simultaneously to” to “simultaneously with”. done

Figure 7 caption, second from last line: change “Lybia” to “Libya” done

References:

Kalnay, E., et al. (1996), The NCEP/NCAR 40-Year Reanalysis Project, Bulletin of the American Meteorological Society, 77(3), 437-471, doi:10.1175/1520-0477(1996)077<0437:TNYRP>2.0.CO;2.

Kern, C., T. Deutschmann, C. Werner, A. J. Sutton, T. Elias, and P. J. Kelly (2012), Improving the accuracy of SO2column densities and emission rates obtained from upward-looking UV-spectroscopic measurements of volcanic plumes by taking realistic radiative transfer into account, Journal of Geophysical Research: Atmospheres, 117(D20), n/a-n/a, doi:10.1029/2012jd017936.

Anonymous Referee #2

General
The paper deals with a very important and modern research topic: long term dispersal of volcanic clouds, with focus on sulfur dioxide emissions and fluxes in the troposphere. The paper includes satellite observations (passive as well as active with the CALIPSO lidar) of a recent Etna volcanic activity in the Mediterranean (April 2011). The paper is well written and contains a comprehensive discussion of the findings and on potential uncertainties in all the retrieval approaches. I have only a few comments (minor points)!

Abstract: The abstract is too long. The abstract is not an introduction section, in which the motivations of the paper is presented. Please briefly provide the goals of the paper, the used techniques and the main findings. Only that, and not more.

We disagree with the reviewer's opinion about the confusion between the abstract and an introduction. Only the two first sentences of the abstract may be referred to as introductory, whereas the rest of the abstract is focused on data, methods and conclusions directly related to the paper.

The abstract is indeed relatively long but it does not include, according to us, any subsidiary information. Such length for the abstract is required to detail the synergy of the various ground-based (UV-DOAS) and spaceborne (IR IASI, RGB MODIS, CALIOP lidar) observations as well as the different modelling approaches (CHIMERE chemistry-transport model, inverse modelling, HYSPLIT trajectory model) which are developed in this study. Various results have been also summarised covering (1) the reconstruction of both flux and altitude of SO_2 emissions by inverse modelling, (2) the under-estimation by one order of magnitude of SO_2 flux estimations from UV ground-based observations compared to space-derived retrievals (based on inverse modelling) during ash-rich eruptive phases, (3) the validity of modelled altitudes of the SO_2 cloud at distance from the source by the CHIMERE CTM initialised with reconstructed emissions, and (4) the observation of high-altitude tropospheric sulphate aerosols coexistent with Etna SO_2 from lidar observations.

Finally, the last result of this article consists in elaborating the procedure to follow to assimilate in inverse schemes recently-developed products on the SO_2 altitude from IR hyperspectral imagery in order to reconstruct the altitude of emissions. This is detailed in the last paragraph of the abstract. This part, which represents already a result and not only a motivation, consequently paves the way for performing such assimilation in another paper.

In this context, it seems hardly possible to shorten the abstract.

Introduction:

Page 5034, lines 7-13: You can find a good example for enhanced air pollution by volcanic sulfate particles after the Eyjafjallajoekull eruption in April 2010 (e.g. 19 April 2010) in the EARLINET/AERONET paper of Ansmann, JGR, 2011.

We had decided to illustrate our comments with massive episodes of air pollution triggered by volcanic eruptions. We mentioned in the text the air pollution induced by the Eyjafjallajokull eruption in Germany which is however of far lesser magnitude.

Page 5035, lines 23-26: Can you really be sure that the volcanic aerosol and sulfur emissions and transport were at all well defined in terms of height assignment after the Eyjafjallajoekull eruption. Is passive remote sensing sensitive enough to see all the traces of volcanic aerosols and gases? I have my doubts! Traces of volcanic material were at all heights within the troposphere according to the Eyjafjallajoekull JGR special issue, as for example shown by Seifert et al., JGR, 2011, in this special issue.

Passive remote sensing may miss indeed a part of volcanic gas/aerosol material if the volcanic cloud is largely dispersed or poorly concentrated so as gas/aerosol column amounts are below the detection limit (generally of ~ 0.3-0.5 DU for IASI SO_2 observations).

By the way, ground based lidars are much more powerful than the CALIPSO lidar, so they can detect all the volcanic traces, whereas the CALIPSO lidar observations often suffer from strong signal noise, as you show also, later on in the paper.

Indeed, ground-based lidars may be more powerful than the CALIPSO spaceborne lidar. However, for our specific case-study, the Etna volcanic plume did not overpass any ground-based lidar station (to our knowledge) as it mainly travelled over the Mediterranean Sea. That is the reason why we exploited CALIOP tracks.

Page 5036, lines 15-30: The approach is fine, but you still have no potential to obtain clear vertical SO2 profiling. Should one discuss the potential of an SO_2 Differential Absorption Lidar (SO_2 DIAL)? Would such a lidar be helpful? Or are the SO_2 concentrations too small? Please check the literature regarding SO_2 DIAL observations.
Indeed, if available, a SO_2 DIAL could provide simultaneously both SO_2 flux and altitude profile at the volcanic source. To our knowledge, a single experiment of this kind has been developed and proved successful to capture volcanic emissions (Edner et al., 1994; Weibring et al., 1998). SO_2 DIAL observations could detect weak SO_2 fluxes down to 10 tons per day. Sensitivity does not seem consequently to be a problem. However, such an experiment requires a costly, heavy and bulky instrumentation with a high power requirement. In the above-mentioned published studies, instruments were installed onboard a 10 tonne truck. The truck was then embarked upon a boat in order to capture emissions of Etna, Stromboli and Vulcano volcanoes. The difficulty to deploy such an experiment certainly explains why a single study of this kind has been developed so far in a volcanic environment (to our knowledge).

Passive remote sensing instruments, such as SO_2 UV-cameras which are increasingly used in volcanic environments, provide the 2D- distribution of SO_2 with a high frame rate allowing for the retrieval of SO_2 flux and altitude at high temporal resolution. The main disadvantage of such imaging systems, with limited spectral information, relies on the difficulty for correcting spectra so as to account for a realistic radiative transfer between the sun and the instrument, especially during ash-rich phases of eruptions (see Kern et al., AMT, 2010). In this context, the advantage of SO_2 DIAL observations, with an active source, may be a less complex radiative transfer in the presence of dense ash plumes compared to passive remote sensing techniques. This remains to be quantitatively estimated. A comparison between SO_2 DIAL and UV-DOAS observations has been carried out in the above-mentioned studies (Edner et al., 1994; Weibring et al., 1998) . This experiment, which took place during eruptive episodes without ash emissions, already highlighted the importance of describing complex light scattering within and under volcanic plumes in the retrieval of UV-DOAS spectra. A similar experiment would be welcome for comparing this time the retrieval of active SO_2 DIAL and passive UV observations (DOAS and SO_2 camera) of an ash-rich plume.

The interest of SO_2 DIAL observations is now discussed in Section 4.3 in the updated version of the paper.

Page 5038, line 25: you write: radar, but you prefer to write: LiDAR! Why? Usually the lidar scientists report lidar observations, and not LiDAR observations.
Modifications with ‘lidar’ has been done.

Page 5038, line 25: How can a radar help you regarding the altitude of SO_2 emissions?
The radar allows indeed for determining the altitude of volcanic aerosols and not directly the altitude of SO_2. We assumed that SO_2 is coexistent with aerosols, which is generally the case close to the volcanic source. This has been clarified in the text.

Page 5040, lines 12-19: The approach is reasonable, but leaves still open many questions regarding the uncertainties in this retrieval (as mentioned in lines 23-24)!

Uncertainties on retrieved IASI altitudes are partially discussed in Section 4.2 (pages 5053-5054), which mainly focuses on "outlier pixels". The accuracy of the algorithm was evaluated in Clarisse et al. (2014) and was typically found to be of the order 1-2 km from coincident CALIPSO and MLS observations. This information is now added to the section on the description of the algorithm (section 2.2.2):

"For the Nabro eruption, Clarisse et al. (2014) found from coincident CALIPSO and MLS observations that the retrieved altitudes from this algorithm are typically accurate up to 1-2 km for all but the weakest concentrations (e.g. at the edges of the plume)."

Page 5041, lines 12-30: How can the CALIPSO lidar track the volcanic aerosol plumes? The spaceborne lidar provides just snapshots (as you use one: 00:26 UT on 11 April 2011). The lidar crosses the same location every 16th day only. The horizontal resolution is rather coarse. And as mentioned, the spaceborne lidar is not just a powerful lidar as all these ground-based lidars presented by Ansmann (2011), Pappalardo (2013), and many other papers in a variety of special issues in Atmospheric Research or Atmospheric Environment and ACP (search for authors like Papayannis, Gross, Sicard, Lucas Arboledas and others). You may even mention airborne (UK) lidar activities of Franco Marenco (in JGR? or ACP?). Sure, the CALIOP lidar is unique, but all the ground-based and airborne lidar activities were much more helpful to describe the Eyjafjallajoekull plumes over Europe.

The term 'track' is indeed confusing and has been changed to 'provide snapshots of the vertical distribution of volcanic aerosols along the satellite track".

As above mentioned, we agree that ground-based stations are generally more powerful than the CALIOP spaceborne lidar. However, in our specific case-study, the Etna plume travelled over the Mediterranean Sea and did not overpass any ground-based lidar station. This is the reason why unfortunately, we could not exploit any ground-based observations. A sentence has been added to this paragraph to underline this point. Nevertheless, the interest and advantages of ground-based lidar measurements have been discussed in Section 4.3.

Page 5042, lines 5-11: Please provide uncertainties in all the HYSPLIT trajectory computations. Many groups no longer use just single trajectories, they prefer to use products of ,e.g., FLEXPART and other dispersion models.

HYSPLIT trajectory computations are used here to assess qualitatively the relevance of (1) the altitude of emissions retrieved by inverse modelling and (2) the altitude of Etna SO_2 at distance from the source predicted by the CHIMERE chemistry-transport model initialised with the emissions determined by inverse modelling.

Nevertheless, uncertainties on HYSPLIT trajectories have been added to Fig. 5 by varying the altitude of emissions (by +/- 1 km) initialising HYSPLIT runs. We observe the weak uncertainty on the altitude of emissions at 9:00 AM. Whereas the HYSPLIT trajectory for a 4 km-high emission is in agreement with the trajectory of the Etna plume described by the RGB MODIS image, emissions injected at an altitude of 3 or 5 km follow very different paths.

Concerning emissions at 11:00 AM, they follow approximately a similar direction whichever their altitude of injection (at 6, 7 or 8 km). However, the uncertainty on the emission altitude remains weak as the velocity of the volcanic cloud is drastically different depending on the altitude of emission according to HYSPLIT runs. Precisely, the velocity increases with the

altitude of emissions. Hence, only the HYSPLIT trajectory run computed with a 7 km –high emission as input is able to reproduce the length of the volcanic plume observed with the RGB MODIS image.
This uncertainty analysis is now discussed in Section 3.2 of the updated manuscript.
Note that uncertainties resulting from a varying altitude of the distant SO_2 cloud are already shown for backward HYSPLIT trajectories in Fig. 7.

Page 5048: Concerning the CALIPSO lidar comparison, I have some remarks:
Line 14: How is the color ratio defined (attenuated backscatter (1064 nm) / attenuated backscatter (532nm)) or just 1064nm backscatter coefficient divided by 532nm backscatter coefficient.
The total color ratio represented here (precisely the integrated attenuated total color ratio) is the ratio formed by dividing the layer-integrated attenuated backscatter at 1064 nm by the layer-integrated attenuated backscatter at 532 nm. This has been added in Section 2.2.2.

Line 15: meteorological clouds are just clouds (or cloud layers)
We prefer using the term 'meteorological clouds' rather than 'clouds' alone to avoid any confusion with the term 'volcanic cloud' which is also used throughout the manuscript.

Line 23: particle depol ratio in volcanic plumes is always less than 0.4.

Silke Groß, Volker Freudenthaler, Matthias Wiegner, Josef Gasteiger, Alexander Geiß, Franziska Schnell, Dual-wavelength linear depolarization ratio of volcanic aerosols: Lidar measurements of the Eyjafjallajökull plume over Maisach, Germany, Atmos. Environment, pages 85-96, 2012.
The range of particle depolarization ratios associated to volcanic ash that we provided in the manuscript (in 0.17-0.6) includes various ground- and satellite-based studies. Whereas ground-based lidar observations do not present ratios exceeding 0.4 (e.g. Ansmann et al., 2011; Wiegner et al., 2012; Gross et al., 2012; Derimian et al., 2012; Pappalardo et al., 2013), depolarization ratios up to 0.59 could be observed on few occasions in the Eyjafjallajokull plume in April 2010 by the CALIOP lidar (Winker et al., 2012).

The reference of Gross et al. (2012) mentioning indeed particle depolarization ratios < 0.4 has been added, as well as the reference to Wiegner et al. (2012).

What is the uncertainty in all these CALIPSO products, must be large for these optically thin layers.
As detailed below, the analysis of the uncertainty on CALIPSO ratios, which may indeed be significant for some products, does not call into question our interpretation but rather confirms it.

Around the cirrus, the backscatter may not be caused by aerosols but by remnants of cirrus. Color ratio is not so helpful when signals are low and very very noisy!
Due to backscatter signals of low intensity, relative uncertainties on CALIOP total color ratio up to 90% are recorded for aerosols coexistent with modelled SO_2 (i.e. aerosols surrounded by a black line in the updated Fig. 9 at latitudes between 34.2 and 35.5 deg), which corresponds to an absolute uncertainty on total color ratio up to 0.15. Nevertheless, taking into account this range of

uncertainty, total color ratios less than 0.3 are recorded (see updated Fig. 9), far lower than the total color ratios of neighbour meteorological clouds in 0.5-0.8. Hence, these results show that the signature of the aerosols of interest is clearly distinct from the one of meteorological clouds. Despite the range of uncertainty, a low total color ratio is nevertheless still recorded, which confirms the presence of aerosols of small size.

Uncertainty on aerosol total color ratio has been added to Fig. 9 and discussed in Section 3.3.3.

Particle depolarization ratios of 0.3 indicate ash, 0.1 a mixture of ash and spherical particle (maybe volcanic sulfate particles). 0.2-0.5 indicates cirrus.

Your comment is in perfect agreement with our interpretation. We consider that the particle depolarization ratio associated to the aerosols collocated with modelled Etna SO_2, ranging in 0 - 0.17 (including uncertainty) with a mean at 0.08, indicates the presence of aerosols with a near-spherical shape. The signature of these aerosols is drastically different from the one of volcanic ash or cirrus ice clouds. To illustrate this difference, we have added a new figure (Fig. 10 in new submission) which summarises the range of particle depolarization ratios observed for these different species. Particulate depolarization ratios of Etna aerosols in this study are also similar to those observed for sulphate aerosols in stratospheric volcanic clouds (Kasatochi and Sarychev) which are smaller than 0.2 (O'Neill et al., 2012, Krotkov et al., 2010) – note that these two last references have been added in the updated paper. Consequently, we propose that Etna aerosols are likely sulphate aerosols or partially crystallised sulphuric acid droplets as discussed in the text.

When discussing cirrus and volcanic layers, please add Seifert, JGR, 2011 to the reference list (not only Sassen et al., 1989). Added

Figure 8: Total attenuated backscatter...! What does that mean? Rayleigh plus particle attenuated backscatter?

Indeed, 'total' means the combined signal from molecular, aerosol and/or cloud backscattering. This has been added to Section 2.2.2.

Figure 9: If the volume depolarization ratio is so low (2% or so), the particle depolarization ratio is not very trustworthy at the low backscatter coefficient conditions. Please provide uncertainty range.

Absolute uncertainties on particulate depolarization ratio up to 0.07 are recorded for the high-altitude aerosols coexistent with modelled Etna SO2 (contoured by a black line in Fig. 9). Given this range of uncertainty, particulate depolarization ratios for these aerosols, with a mean at 0.08, never exceed 0.16. This value of 0.16 is still lower than the ones characterizing neighbour meteorological clouds evolving at the same altitude in the range 0.29-0.41. This value is also lower than the range of particulate depolarisation ratios measured for volcanic ash in 0.17-0.59 (references mentioned above) and illustrated by the new figure added to the updated version of the paper (Fig. 10).

Uncertainty on aerosol particulate depolarization ratio has been added to Fig. 9 and discussed in Section 3.3.3.

Also the color ratio is always 0.2-0.3 outside of clouds, more information can not be extracted, So there is not only ash!

Total color ratios up to 0.24 are recorded for aerosols collocated with modelled Etna SO_2.

Relative uncertainties on total color ratio up to 90% are recorded, corresponding to an absolute uncertainty on total color ration up to 0.15. Hence, total color ratios for the aerosols collocated with modelled SO_2, which present mean values of 0.16, reach maximum values of 0.32 (the value of 0.32 takes into account the large uncertainty above-mentioned).
Ratios associated to other aerosols detected along the CALIOP track (but not collocated with modelled SO_2) present ratios up 0.7. Clouds neighbouring the aerosols collocated with modelled volcanic SO_2 present even higher values in 0.5-0.8.
The retrieved ratios for the aerosols of interest (collocated with modelled Etna SO_2) are consequently low and reveal the presence of aerosol of small size. These aerosols likely correspond indeed to sulphate particles, given the above discussion on particle depolarization ratio, and not to volcanic ash.

Page 5052, lines 7-10: Again, what about the use of a SO_2 DIAL, please discuss, would that be sensitive enough. One could think about deploying several SO_2 lidars on the Mediterranean islands (close to Etna, Crete, Cyprus…).
Please refer to the answer provided to comments to page 5036 which detailed the advantages and disadvantages of SO_2 DIAL observations, compared to passive UV remote sensing techniques, for the characterization of volcanic plumes.

Page 5055, section 4.3.: SO_2 DIAL, please discuss again, would that be helpful? And again, please mention ground-based lidar activities in addition (such as, e.g., presented by Ansmann 2011). For lidar people it is joke to say that the CALIPSO lidar is much more powerful than ground-based lidars regarding high altitude plume detection.
Comparison between ground-based and spaceborne lidar observations was awkwardly developed in the ACPD manuscript. Section 4.3 has been consequently rewritten. Discussion of SO_2 DIAL observations has also been added to this section, as well as the reference to Ansmann et al. (2011).