We would like to start by thanking the reviewer for helping us improve the manuscript. The reviewer's questions and remarks have been answered in the text below, where reviewer comments are marked in black and our responses in this blue color.

Volcanic eruptions represent one of the largest source of natural variability of our climate system. Thus, the construction of long-term database of stratospheric aerosol optical depth is highly important to constrain global climate models. In this study, Friberg et al. (2018) used the CALIPSO space-born lidar to derive the time evolution of stratospheric AOD between 2005 and 2016. They proposed two new techniques to correct the effect of particle attenuation on retrieved optical parameters (backscatter and extinction) and remove Polar Stratospheric Clouds. After selecting a definition of the tropopause based on Potential Vorticity, they show time series of stratospheric AOD and discuss the influence of several volcanic eruptions. Overall, the paper is interesting, and the technique developed to correct the effect of particle attenuation is relatively well explained. But there are several points which would need serious considerations before its publication in ACP.

1. Purpose of this work. The purpose of this study is not clear. Do the authors intend to create a new aerosol dataset from the CALIPSO data? If yes, do they have a plan for archiving the data and make them publicly available? If this is the case, is there any established collaboration with the CALIPSO team to work on this dataset or it is an independent endeavor?

The purpose of this work is to investigate the stratospheric aerosol in a period when several volcanic eruptions affected the stratosphere. Compared to previous studies we have put emphasis on understanding the aerosol load in relation to important transport features of the stratosphere. We have also included a larger part of the stratosphere compared with previous studies, approaching a complete coverage of the stratospheric aerosol of the period. We highly appreciate the work of the CALIPSO team, and would be interested in collaboration just as we have been in the past (Andersson et al., 2015).

2. Lack of validation. This study does not make use of additional datasets to compare/ validate the retrieved AOD. Several satellite datasets (e.g. OSIRIS, OMPS) are available and could provide a source of validation data but are not used. What about In situ data from the CARIBIC program such as aerosol size distribution in the Lowermost stratosphere? Why not to use those data to infer lidar ratio using Mie Calculations?

CALIOP data are validated already in for example publications by Vernier, and used in the Science paper of Solomon et al. (2011). Our methods to retrieve the AOD are not significantly different from Vernier's and that used in our previous CALIOP publication in Nature Communications 2015 (Andersson et al., 2015). We have also validated that we retrieve the same scattering and AOD values (at 20-30 km) as in Vernier et al. (2011). We will compare our AODs with those in Rieger et al. (2015), Vernier et al. (2011), and Thomason et al. (2018), and add a more thorough description about the lidar ratio in the manuscript.

Our intension is to discuss the volcanic impact from the global observations of CALIOP, and to include the entire stratosphere. Furthermore, a paper on comparison between CALIOP and CARIBIC is in preparation by one of the co-authors.

Our paper extends the study by Andersson et al. (2015) highlighting the significance of the LMS, which has been neglected until very recently, and the overlying stratosphere has to our knowledge not been separated in individual layers before.

3. Retrieving AOD from CALIPSO. The authors propose an approach to correct the particle attenuation effect which is especially important after a significant volcanic eruption. A major issue with the proposed technique is its dependency to the type of volcanic eruptions. The major assumption of the correction technique is to assume that the Upper Troposphere is clear of volcanic aerosol, but this is not always the case as shown after the Kasatochi eruption. Any techniques applied for this purpose should be independent from volcanoes and therefore could only be achieved by the iterative approach developed in Hostetler et al. (2006). The overall impact on the corrected AOD is relatively small (impact between 4-7 %).

It is true that it depends on the type of eruption and cannot be used directly for all types of eruptions. Kasatochi is a special case. In-situ observations by CARIBIC shows that volcanic elevations of the stratospheric aerosol load generally have small impact on the UT aerosol concentrations. Thus, the proposed method can be used directly for eruptions which injections mainly reached above the UT, and by our method we risk doing a small underestimation of the true volcanic elevations.

We believe that the method described in Hostetler et al. (2006)/Young et al. (2005) is good for correcting for attenuation in detected volcanic layers, but that it is evident that the method cannot detect the volcanic aerosol once it has mixed with the background. Hence, the two methods have different weaknesses.

The computed changes are relatively small in perspective to the total stratospheric AOD for the period studied here, but could become much more important for stronger eruptions. The attenuation depends on the overlying aerosol column, and is thus particularly important for the representation of the lowest layer of the stratosphere, i.e. the LMS.

The authors never discussed thoughtfully the other sources of uncertainties that could have bigger influences (e.g. calibration of the lidar, lidar ratio conversion factor). We agree that these uncertainties are important and that the attenuation corrections made in the manuscript are small in comparison to e.g. that in the lidar ratio, and we decided to include a description in the methods section according to the suggestion of the reviewer. Our corrections make a significant difference for the retrieved aerosol scattering, especially in the LMS.

For example, they rapidly mentioned that the lidar ratio values of 50sr used to convert backscatter into extinction agree with Prata et al. (2017). This is not correct, Prata et al. (2017) found a mean lidar ratio of 69 sr for the Cordon plume, 66 for Kasatochi and 63 for Sarychev. This would increase the volcanic AOD during volcanically influenced periods by 30-40 %. The lidar ratio assumption is therefore one of the main source of uncertainty for AOD retrieved from CALIPSO but poorly discussed here.

We agree with the reviewer that the motivation for using the factor 50 sr was discussed too shortly.

The assumption of value on the lidar ratio is indeed a large uncertainty for the retrievals by CALIOP. The figures by Prata et al. (2017) are higher than those used in our manuscript, but according to the uncertainties reported in Prata et al. (2017) the factor 50 sr is not significantly different from the values reported in their paper, but we are on the lower side of their estimations. They reported *means and standard deviations* for the lidar ratios of volcanic layers to be *69±13 sr* (Puyehue-Cordón Caulle, Jun 2011), *66±19 sr* (Kasatochi, Aug 2008) and *63±14 sr* (Sarychev).

There are differences in methodology in our manuscript and that of the Prata et al. (2017) paper. They found relatively fresh volcanic aerosol plumes in the stratosphere and computed the AOD from that, while we use zonal means, i.e. mixtures of layers of volcanic aerosol and the stratospheric background aerosol. Also, the values reported in Prata et al. 2017 are based on fresh aerosol. Aging of the aerosol and mixing with background changes the particle size distribution. Thus, the lidar ratio is not necessarily equal for the fresh volcanic aerosol, and aerosol that has been aged and mixed with the background. Most studies that dealt with stratospheric aerosol show lower lidar ratios than those reported in Prata et al., 2017. In a most recent study Thomason et al. (2018) find the CALIOP lidar ratio to have a value of 53 sr. We therefore believe that using the values reported in Jäger and Deshler (2003) is well-suited for our study.

We have decided to include a more thorough discussion on the uncertainties in the lidar ratio. We will also point out (in the manuscript) that we used a conservative value of the lidar ratio, along with the comparison to other AOD data-sets (Rieger et al. (2015), Vernier et al. (2011), and Thomason et al. (2018)).

4. English language. This is an overall issue which could be difficult to address without a native English speaker person. However, the level of English in the paper is relatively poor and would need to be improved. I recommend the co-authors of the paper to take part of this effort to improve the English.

We are aware that we as non-native speakers are in a difficult position language-wise. Reviewer #1 wrote that "…Overall the *writing is quite good, but there are a few awkward places…*", and also provided a thorough list of suggested changes. We have corrected many language-mistakes by taking into account the many points made by Reviewer #1, and gone through the text with a critical eye. We believe that these changes, as well as all other changes, have improved the manuscript.

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