We would like to start by thanking the reviewer for the many constructive comments that help 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.

This contribution to our understanding of stratospheric aerosol optical depth (AOD) focuses on the CALIOP backscatter data set after it is cleared of clouds. A significant fraction of the paper is devoted to a procedure to correct CALIOP measurements beneath volcanic clouds when the laser is attenuated by the volcanic cloud. The procedure is relatively straight forward, although I found the discussion of how the threshold is set to instigate the procedure, and the relevance of Figure 3 to that procedure, confusing. I don't have specific suggestions to help here, but perhaps the authors could have a close read of this part to see if it may be improved.

Then the procedure is applied and the CALIOP data set presented over the 14 year period. The results are quite interesting and I appreciate the separation of the stratosphere into three layers associated with their relevance to cross tropopause transport. This separation led to some nice generalizations concerning the volcanic eruptions which occurred over the 14 year period and how these eruptions influenced AOD and hence the cooling associated with stratospheric aerosol.

The paper is interesting, especially once the data are discussed, and should be published. There are a few things that need to be taken care of before that happens.

Aside from the detailed comments below the authors should add labels a), b), . . . to all the figure panels. The authors are very good at explaining what each of the a), b), . . . panels are in the captions, and refer to Figure Xa in the text. But none of the panels in the figures have labels. We apologize for overlooking this, and will include the indexes to all subfigures.

Overall the writing is quite good, but there are a few awkward places. I have tried to flag these. One general comment the phrase "in order" is never necessary and can be deleted with no change in the sentence meaning. Thank you for pointing that out. We have included the suggested changes, and make further language corrections adjusting to comment #4 by the second reviewer.

1.9-11. Why switch between CALIOP and CALIPSO in the abstract when neither are defined. CALIOP is the instrument and should suffice in both places. We agree with the reviewer. We will use CALIOP throughout the manuscript, and add the definition in the abstract.

1.19-20. Awkward sentence, beginning with "of which". Try. Trends in the abundance of aerosol particles are an important component of the climate system, although their influence on climate is still highly uncertain. . . We have used the reviewer's suggestion.

2.10-14. Change forming to contributing. If OCS is known to form the Junge layer (which it isn't) then why the discussion about so2? The reviewer is right, and we have changed accordingly.

2.21 LMS? – We forgot to define the lowermost stratosphere. Thank you for pointing that out.

5.20 residing . . . We changed accordingly.

6.9 becomes . . . We changed accordingly.

7.29-31. This is confusing. Why would you expect aerosol scattering to compare with extinction? There is at least a factor of 50 between them. In Fig. 3 it is much more. The AS is noted in a range of 1e-8 to 1e-7, while the extinctions range from 0.02-0.1. The units are not of importance for the comparison. It is simply done to highlight that the Level 2 extinction data do not follow that of the volcanic aerosol. We understand that it is not obvious for the reader, and therefore added a sentence to clarify this.

Fig. 3. There are no labels a), b), . . . Why is there a big 0 between the panels at the bottom? We do not see this. Can it be that the reviewer refers to the initially submitted version of the manuscript (from December, 2017)? We added the subfigure indices between that version and the current (version submitted on January 25, 2018)

8.1 Where is Fig. 2b? There are no labels on Figure 2 either. Why strangely? We apologize for missing this. Subfigure indexing is added in the new version (May, 2018).

11.2 first 8 months . . . We changed this.

11.3. Are these the averages, maxima, value after 8 months, or? They are the average, and we have now added that info in the text.

11.28-29, "A feature appearing in the southern tropics in April has been identified as smoke from bush fires in February 2009 in Victoria, Australia (Vernier et al., 2011)." Is this the feature near 20 km, if so it is worth mentioning the altitude as this may be surprising for some. Best to be clear. It is the feature around 20 km altitude. We agree with the reviewer and have clarified this in the manuscript.

Figure 6. Perhaps the vertical solid lines represent the tropical eruptions? The figure caption is confusing. Please add short labels to the eruptions, which are listed in Table 1, so the reader can easily see the effects from Sarychev, Calbuco, and Kasatochi, without having to search them out. Similar labels would be appreciated in Figures, 5, 7-9. The indexing was incorrect. Thank you for pointing that out. We change this, and add indexing in the figures.

13.18-19. What PV levels are considered the LMS. This is needed to understand the statement, "the aerosol signal is strong also in the lowest LMS layer of Fig. 6a". What is the lowest LMS layer? All of the PV-levels in the figure pertain to the LMS. The lowest LMS layer is 1.5-2 PVU. We will clarify this in the manuscript.

13.25. The discussion previous to this conclusion has been descriptive of Figure 6, but it's not clear why the last half of this sentence holds. The volcanoes affected all the PV layers investigated more or less equally. It would be nice to see a PV level that wasn't affected, then maybe there would be a justification for the choice.

The value of 1.5 PVU is the lowest dynamic tropopause commonly used. We find volcanism to impact all the way down to this lowest (stratospheric) PV-level (and in the case of Kasatochi there is an impact also below this PV-level). Therefore, we choose the 1.5 PVU to represent the lower altitude limit where volcanism impacts the stratospheric aerosol. This will be clarified in the manuscript.

14.6-7. This conclusion about the source of the springtime increase in aerosol scattering is much too strong based on the evidence given. What do “These observations” refer to, to the work here or that by Martinsson? Perhaps they “suggest”, but “indicate”, no. Much more work would be required to come to such a definite conclusion. We agree with the reviewer and have changed to “suggest”.

Figure 8. Why don’t the sum of the three lines add up to the total? There is usually at least a difference of 0.002 in AOD between the sum of the three layers and the total. We have controlled this and do not find any discrepancy, neither in the codes out-print, nor in the figure in the manuscript. We find that the three lines (representing the three layers) in each subfigure adds up to the total (red lines). We would be grateful if the reviewer could point out specific coordinates as an example of the stated discrepancy.

14.26-29. This is a bit surprising. Can some more detail be provided? Using Figure 6 the spring time maxima in 2007 and 2008 indicate SR in the lowest layers equivalent to the SR after Nabro, which shows a large AOD. Is it the fact that the impact of the dust layers, or whatever is causing this, so narrow that the impact on AOD is small?

The seasonal increase in AOD from dust (and other tropospheric sources), are estimated to constitute *less than 5%* of that of the AOD increase from Nabro. The dust signals are rapidly decreasing in strength in the first 2 km above the tropopause (1.5-5.5 PVU, the ExTL). Thus, they constitute a small fraction of the total stratospheric AOD, since they are contained within such a small fraction of the stratospheric air-mass. The picture is the opposite for the Nabro aerosol. The sulfate concentrations are increasing from the tropopause into the stratosphere, and this effect is larger for tropical eruptions than for the local eruptions in midlatitudes. In midlatitudes the subsidence of air (Nabro’s aerosol) from the 380 K isentrope through the LMS to the tropopause takes several months, during which the stratospheric air gradually mixes with tropospheric air which decreases the sulfate concentrations and induces a gradient of sulfate from the tropopause throughout the LMS. The sulfur gradient and the weak Nabro aerosol signals around the tropopause are both evident in Figure 5a. We have estimated that *the Nabro aerosol in the ExTL constituted less than 6%* of the total stratospheric AOD during the period of 1-8 months after its eruption.

This may be counterintuitive from the appearance of figure 6. We will therefore add discussion on this in section where the figure is presented.

15.3. The small peak near the equator in the upper layer can hardly be considered to be “high AOD”. It is only slightly higher than in the mid latitudes. The reviewer is right. We have reformulated our statement.

15.26-31. It would be helpful to indicate here that Figure 7 is being discussed. This is the figure I used to follow the discussion. Thanks for pointing that out. We have added a reference to Figure 7.

16.4. “A small sudden increase in the AOD is observed in the southern extratropics approximately one and a half years after the Kelut eruption.” At what level? Which figure is being referred to? This is indeed confusing. We have added a more descriptive text.

16.8-9. “The aerosol in the upper layer is eventually transported out to the next lower layer at midlatitudes, i.e the one located between the 380-470 K isentropes.” It would be nice to know what figure is being used to make this claim. Figure 7 for example does not support this statement.

The AOD in Figure 7b indicates this. For example, the AOD in the northern midlatitudes of the layer 380-470 K was higher in year 2007 and beginning of 2008 compared to the background (year 2013). The increased AOD in midlatitudes most likely comes from subsidence from the overlying layer. We will make changes to the text to clarify this.

19.2. “. . . forcing, and is added . . .” We will make the suggested change.