Response to Referee #1

Hereafter Kar et al. will be shortened to K19. K19 introduce a level 3 stratospheric aerosol product based on CALIOP data. This initial version was developed since the production of the version 4 CALIOP level 1B and 2 data sets. K19 summarize the CALIOP product history and point out the major advance in version 4 that enables a stratospheric L3 product that extends completely through the Junge layer (calibration based on measurements between 36-39 km). They describe how the employ previously documented CALIOP cloud and PSC masks to isolate aerosols, and an additional screen based on a depolarization ratio threshold for creating a separate background aerosol L3 data set and a background+plume data set. The manuscript is well composed and written. The L3 algorithm is logical and described adequately. K19 offer it as a first version and acknowledge some key areas that may justify refinements. This manuscript is appropriate for AMT. It represents a useful new contribution to the resources that atmospheric and climate scientists need for large-scale studies of the stratosphere. I would recommend publication after K19 satisfactorily address the following issues, one of which I classify as major. Next I characterize this concern, followed by some minor concerns. Finally a list of technical items to address.

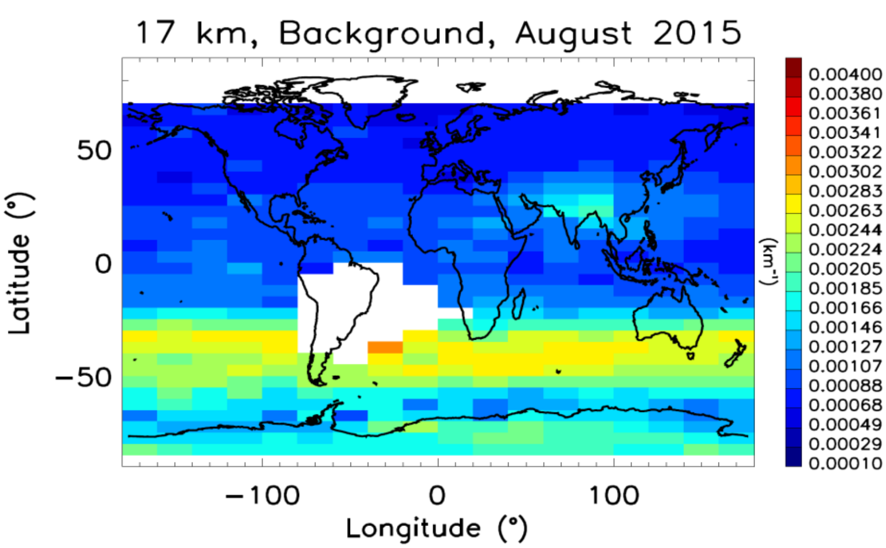
Thanks very much for a careful reading of the manuscript and for your useful suggestions.

Major concern: It is apparent from several figures and K19's discussion of them that there may be a significant bias in the L3 products (both background and all-aerosol). My attention was drawn to the residual PSC signature in Figures 4, 8, 9. K19 acknowledge this residual and attribute the signature to "particles in the process of becoming PSC." The residual itself is a dominant feature in all the figure panels where PSC presence is expected. This undoubtedly reveals an incomplete masking of weak but meaningful scattering. In the high-latitude winter realm it is straightforward to dismiss these as tenuous cloud particles, but at lower latitudes and especially the lowermost stratosphere, any residual that may survive cloud screening cannot be simply classified as aerosol. Given that the L3 aerosol algorithm is globally based on the L2 merged layer data, any tenuous cloud (analogous to the acknowledged PSC residual) will be in the L3 dataset and considered aerosol. Just like PSCs, cirrus clouds in their formative and sublimation phase will naturally present a scene that will contain scatterers outside the layer-detection algorithm's thresholds. An ad hoc inspection of L1B quicklooks reveals this to be fairly common. Hence to the extent that a particular scene is subject to thin, patchy, formative or sublimating cirrus, the L3 background and "all aerosol" data sets are at risk for false-positive aerosol detections. This vulnerability reaches its greatest likelihood in regimes of high-frequency, high-altitude cloud. The most prevalent of these is the Asian summer monsoon region, one of the cloudiest on Earth for high cirrus. To the extent that the PSC analogy is accurate for non-polar high-cloud regimes, the L3 background and all-aerosol data may be cloud biased like the PSC signatures alluded to here. If this concern is well founded, it is essentially impossible to confidently argue that extinction enhancements such as those displayed in Figure 10 in the Asian monsoon sector are aerosols and not tenuous clouds. K19 are encouraged to consider the revelations they showed with respect to PSC residual and assess the applicability of that analogy to the potential for cloud contamination in the lowermost stratosphere, especially where/when cirrus cloud occurrence frequency is high. It might also be instructive to compare Figure 10 with an identical rendering based on the L3 background realization. Ideally the stratospheric background should have no imprint of clouds or aerosol perturbations. The amount of similarity between a background rendering of Figure 10 and the manuscript's rendering would be informative as to how well the L3 algorithm is performing in cirrus-cloudy areas.

This concern was also informed by Figure 3, which shows a layer-like peak in background scattering ratio at the same altitude as the all-aerosol data set in the Sarychev Peak-influenced stratosphere. It led me to wonder what the blue background plot would look like for the same period and geographic cell but for a year without a known volcanic plume. If there is a significant reduction in peak scattering and a monotonic decrease with altitude, it would be suggestive of extra-background aerosols getting into the background data set. Akin to the cloud-detection-threshold conundrum, stratospheric aerosol features absent a feature-detection, yet visible to the eye in quicklook images, are common. To the extent that this is true and quantifiable, L3 background aerosol abundance will be high biased, especially when there are large/widespread tenuous plumes. This may be manifested in Figure 4a, which shows hints of the Nabro plume in the same place as the stronger Nabro feature in the less-screened realizations.

Your concern about cloud contamination is a perennial problem with all satellite instruments attempting to retrieve aerosol in the lower stratosphere below about 20 km. We tried our best to remove cloud contamination by using several different filters. As mentioned in the manuscript, the first filter removes the layers as detected by the CALIPSO layer detection module and then classified as "cloud"s by the Cloud-Aerosol-Discrimination (CAD) module. Clearly, the efficiency of removal of cloud layers in this dataset depends upon the efficiency of both these algorithms. In particular the CAD algorithm uses the optical properties of the layers, specifically the volume depolarization ratio and attenuated color ratio with both measurements becoming noisy at increasing altitudes above the local tropopause. The second filter removes the PSCs as identified by the separate PSC Mask product and again depends upon the robustness of the PSC product. Note that we have used v1.0 of the PSC product which has since been updated into V1.5 and we will use the latter in the future versions of the stratospheric aerosol product. For this version of the product and for the sake of uniformity, we decided to use the clouds (PSCs or otherwise) as detected by the available CALIPSO layer products. Note that in the polar regions, particularly over Antarctica, layers detected as "clouds" by the CAD algorithm and independently as PSCs have been removed. As has been clearly stated in the manuscript, scattering from particles below the threshold of detection for both the regular layers as well as the PSC layers will remain in the product. In particular, in winter over Antarctica, it may not be easy to define the point when the background aerosol transitions into a detectable PSC.

As you mentioned and as is well-known, it is very difficult to remove the tenuous cirrus clouds which occur within a few kilometers near the tropical tropopause. Our third filter attempts to do this using a threshold on either volume depolarization ratio or attenuated color ratio. We discuss these filters further below. However, we would like to point out that presence of aerosols within the Asian summer monsoon anticyclone between ~13-18 km (the so-called Asian Tropopause Aerosol Layer, ATAL), formed from gas phase precursors or primary aerosols related to deep convection has been confirmed from balloon and aircraft observations, although some ice clouds are likely present over this same area (Tobo et al., 2007, Vernier et al., 2011, 2015, 2018, Hopfner et al., 2019). Therefore most of this plume as e.g. seen in Fig 10 (Fig. 11 in revised version) is likely aerosol.



The figure above shows the spatial distribution of the "background" aerosol mode for July 2015 at 17 km. As can be seen, the Asian summer monsoon plume as well as the signature of the Calbuco volcano between 30^{o}S and 50^{o}S appear even in the background mode. As discussed above, any signal that is below the layer detection threshold and survives the different filters mentioned above will be included in our background product.

Regarding the depolarization ratio filter adopted by K19, Vernier et al. (2009) ("V09") applied a 5% screen to profiles that are an average of 300-600 L1b profiles. The argumentation therein for 5% was based on an assessment of depol. ratio typical for L1B data. By definition the average depol. ratio in the gridded averaged data is going to be shifted low with respect to the L1B data. The probability distribution of depol. ratio in the V09 gridded data set is unreported and thus unknown, but it is likely to have a very small mean since it is composed of many clear-sky pixels as well as cloud fringes and weak-cloud pixels. It is unclear how a 5% depol. ratio in such an average distribution maps to 5% in L1B data. Consequently a depol. ratio threshold based on the gridded data may have to be much smaller than even 5%. Neither V09 nor K19 provide any testing in defense of the 5% threshold, hence any conclusions regarding aerosol abundance or cloud contamination in the L3 data set are subject to considerable uncertainty. An analogous argument applies to the color-ratio filtering described. K19 apply a color ratio screen based on gridded, averaged data but chose a threshold that is justified in relation to L1B data. Hence some amount of cloud contamination would be systemic in the L3 all-aerosol data set. K19 are encouraged to assess the veracity of my concern, and if it is valid, to take steps to quantify the biases resulting from inadequate screening.

As mentioned above and stated in the manuscript, the depolarization and the color ratio filters were primarily designed to remove the thin cirrus near the tropopause. This is a non-trivial issue if at the same time you are trying to capture the signals of volcanic ash in the stratosphere in the early part of the plume evolution which is important to characterize from the point of view of aviation safety. Firstly the aerosol/cloud discrimination depends upon the scattering ratio or the strength of the plume, and in particular there is generally a large overlap between aerosols and clouds at low scattering ratios. Clearly no single number can be optimal for all situations or at all scattering ratios. We used the cirrus filter at the gridded level to avoid the noise at the profile-by-profile basis. We used June 2011 for testing the filters because of the two representative cases, Nabro being mostly sulfate and Puyehue-Cordon Caulle (PCC) being mostly ash and we wanted to capture both of these stratospheric perturbations. In particular it was a good test case for capturing ash plumes from PCC, since the ash plumes were observed ~12 km, near the cruising altitudes of most airline flights. As explained in the text it was not possible to achieve this using only a depolarization filter to remove the cirrus clouds—indeed the volume depolarization ratio of optically thin cirrus clouds can be as low as 0.1, so even using a very low volume depolarization ratio it would not be possible to get rid of all cirrus. On the other hand, as shown in Figure 4 (Figure 3 in revised version), the attenuated color ratio filter with threshold at 0.5 at the gridded level (which may include both high and low scattering ratios), did capture the ash plumes of PCC also retaining the Nabro plumes at the same time. Lowering the threshold to, say 0.1 for the attenuated color ratio, signals from both the volcanoes essentially disappear and using a higher threshold (e.g. 0.8 which might be more appropriate for higher scattering ratios as also pointed out by referee#2) may retain much more clouds. However, for tenuous plumes, the adopted threshold of 0.5 is likely to retain some cirrus cloud at the tropics near the tropopause, which cannot be helped since we use the same threshold uniformly for all cases. We have now explained this in the text using a new Figure (Figure 4). As shown in that Figure, for such tenuous plumes, the aerosol extinction does not change significantly between the "all aerosol" and "background" modes, but the depolarization filter in the background mode removes the cirrus more efficiently. Thus it might be better to use the "background" mode for volcanically quiet years or for sulfate volcanoes, particularly after several weeks when the plumes become tenuous. In any case, as seen in the climatology of thin cirrus (Sassen et al. 2008), this will primarily impact only the tropics and in the vicinity of the tropopause (up to ~17-18km). In particular, any bias seen in comparisons with other datasets above ~ 20 km will have nothing to do with this cirrus filter and there may be other sources for such a bias.

Minor concerns:
P2, L26-30. This survey of important solar occultation instruments for aerosol measurement is missing a callout for SAM II and POAM II, III. SAM II was especially central to stratospheric aerosol and cloud research. Please consider augmenting this survey.

We have revised the relevant paragraph in the revised version as:

"Most of our current knowledge of the global distribution of stratospheric aerosols comes from satellite measurements. The earliest such measurements were carried out by the Stratospheric Aerosol Measurement II (SAM II) on board the Nimbus 7 spacecraft which provided the vertical profiles of aerosol extinction at 1 μm and were followed by the Stratospheric Aerosol and Gas Experiment (SAGE) series of instruments (Chu and McCormick, 1979, Kent and McCormick, 1984, Mauldin et al., 1985; Chu et al., 1993; Damadeo et al., 2013). The basic principle employed in these instruments is solar occultation, wherein the vertical profile of stratospheric aerosols is retrieved from measurement of sunlight as the rays pass through the atmosphere during sunrise and sunset events as observed from the orbiting spacecraft. Stratospheric aerosols have been characterized using this technique from SAGE instruments on Earth Radiation Budget Satellite (ERBS) and Meteor-3M as well as from the International Space Station (ISS). Among other space-borne instruments that use this technique are the Polar Ozone and Aerosol Measurement (POAM II, POAM III, Glaccum et al., 1996, Lucke et al., 1999) and Measurement of Aerosol Extinction in the Stratosphere and Troposphere Retrieved by Occultation (MAESTRO, McElroy et al., 2009). In addition, the Optical Spectrograph and InfraRed Imager System (OSIRIS) and the Ozone Mapping and Profiler Suite (OMPS) have used a limb scatter technique to obtain the aerosol extinction profiles (Bourassa et al., 2012, Chen et al., 2018)."

P5, L23: The L3 data set extends to 85 N and S. This is beyond CALIPSO's orbit extrema of 82 N and S. Please explain the apparent extrapolation.

Since the data are binned in 5^o in latitude, the first and the last bins imply available data averaged between 85^o and 80^o in both hemispheres.

P8, L07. Two questions regarding the tropopause boundary. Please report the tropopause data that are used. K19 say that tropospheric data are removed. How does that reconcile with Figure 6, which shows

an essentially continuous record of tropical aerosol data at 15-16 km? Isn't the tropical tropopause minimally variant and higher than 16 km? Perhaps there are enough tropical profiles where the tropopause is < 15 km. Some words of clarification are requested.

We have added the following:

"The tropopause heights were taken from the Modern-Era Retrospective analysis for Research and Applications 2 (MERRA-2) reanalyses as in all V4 products (Gelaro et al., 2017)."

Please note that Figure 6 has been revised (now Figure 7) and two new panels have been added showing the distribution at mid-high latitudes in both hemispheres. Also, the stratospheric retrievals for this first version were carried out up to one km below the local tropopause for use in UTLS studies which would explain the extinction data below 16 km. In any case the tropical distribution is now shown starting at 17 km.

P11, L10. Regarding the discussion of the June 2011 situation. K19 might consider mentioning Grimsvotn (May 2011, just a bit earlier than June) given that these aerosols are apparently evident in Fig. 4 in the northern extratropics.

Thanks for this information. We have added the following:

"Further, the high scattering ratios near 50^{o}N are likely due to the Grimsvotn volcano, which erupted in May 2011."

P12, discussion of Figure 5. The relative differences are not evident to my eyes. It might be a good idea to show a third panel, A-B. Or perhaps use another color scale to make it more evident.

Figure 5. These sample sizes are huge. Might K19 consider increasing the temporal granularity?

Per your suggestion, we have now added a third panel showing the difference in samples between the "all aerosol" and "background" modes.

We are not exactly sure what you mean by "temporal granularity". Figure 6 (previously Figure 5) shows the number of samples accumulated at an altitude of 17 km in each horizontal grid cell for the month of July 2009. Since the level 3 stratospheric aerosol data product reports monthly averages aggregated on 5° latitude × 20° longitude spatial grid, the "temporal granularity" of the product is one month.

P12, L22. ". . .particles which are in the process forming PSCs." The same might be said of PSCs in sublimation/evaporation mode. Please consider a minor rewording taking this into account.

We have deleted this sentence and revised the previous sentence as:

"Also note the high number of samples over parts of Antarctica, partly from oversampling due to orbital configuration and related to small particles below the detectability of PSCs by the PSC mask algorithm."

P13, L22-30. Discussion of lidar ratio. I did not see any attention to the differences between the lidar ratio for smoke as compared to sulfate. Please augment the discussion in that regard.

We have added the following in the text:

"Note that the lidar ratios could also be significantly different for stratospheric perturbations resulting from smoke intrusion from pyroCb events (Peterson et al., 2018, Khaykin et al., 2018). For this first version of CALIPSO stratospheric aerosol product we have used only a single lidar ratio."

Discussion of Figure 6. This is an unfair comparison between Kelud, a tropical volcano, and a set of extratropical volcanoes. Hence it doesn't seem to be of any value to compare the relative imprints of these plumes. Consider removing this discussion or providing a stronger argument.

We have deleted the relevant sentence. As already mentioned above, we have now added a couple of new panels (new Figure 7) showing the time altitude plots at mid/high latitudes in both hemispheres, per the second referee's suggestion. The discussion has been modified accordingly.

Figure 6. K19 label and point out some of the obvious features but not all. There seems to be no rationale for this, so please consider labeling the dramatic plumes in J07, J11 (between Sarychev Peak and Nabro), and J16.

We wanted to point out the outstanding features without cluttering the plot with too many labels. In any case, we have now added labels for the features you have mentioned.

P15, L09. Discussion of Black Saturday. K19 rightly acknowledge the pyrocumulonimbus source of this stratospheric plume. Dowdy et al., https://doi.org/10.1002/2017JD026577) provide a detailed characterization of the pyroconvection. Please consider citing this paper if K19 consider it important to do so.

Dowdy et al. mostly deal with lightnings associated with the Black Saturday event as well as fires ignited by the event and don't quite directly address the stratospheric aerosol plumes being discussed in Figure 6 (now Figure 7) and so we have not cited this paper.

P15, L09. ". . .reached altitudes of 16–20 km. . ." To my eye the plume reached _22 km. Am I looking at this correctly? If so, please adjust the description accordingly.

That sentence referred to the findings from the cited works. We have added the following sentence: "The signature of this event can also be identified in Figure 7b, reaching up to nearly 22 km".

P15, L10-11. "These pyrocumulonimbus events seem to be increasing in frequency. . ." If K19 have support for that statement please provide it. Perhaps there is a paper to cite?

We have deleted this sentence in the revised version.

Figure 7 and discussion thereof. Two points. 1. Please provide a tropopause line or marker. 2. This is a very interesting item to understand but it seems to be out of place with the rest of the paper. Perhaps can offer a strong motivation for including it. If not, consider removing it and making it part of future work.

P16, L13. ". . .telltale signature of smoke. . ." the differential 532 nm attenuation is as evident in sulfate plumes as smoke. If this discussion is to remain, it should be expanded to include sulfate.

Per your suggestion, we have now added the tropopause lines in all the four panels in this figure (now Figure 8). We believe it is a relevant figure in that it sets the stage for the height-latitude cross sections of scattering ratios we show in the following figure. We can designate the encircled plumes in Figure 9 as smoke only on the basis of the discussion of this figure.

The level 2 stratospheric aerosol classification algorithm classifies most of the layers as "smoke" or "ash". As mentioned in the manuscript, some of the "ash" has been misclassified because of high depolarization. However we did not mention "sulfate" anywhere.

P18, discussion of Kelud. It should be acknowledged, thanks to CALIPSO, that the injection of Kelud went to 26+km. See Kristiansen et al. (doi:10.1002/2014GL062307). That paper also shows MLS SO2 to 31 mb, so some injected material was up in the mid 20s of km. This means that there may have been no lofting at all, just time-lagged conversion. If K19 agree with this assessment, please consider modifying the discussion accordingly.

Kristiansen et al. point out that most of the ash injection was around 17 km. Also from our Figure 9 (Figure 10 in revised version), the slow ascent of the plume can be clearly seen which is consistent with the upward branch of the Brewer-Dobson circulation. Friberg et al. (2018) also support this explanation. Therefore we have not modified the discussion as such.

P18, L01. "17 km" This is inconsistent with the Fig. First panel shows aerosol to 21 km. Am I looking at this correctly?

We have modified the sentence as follows:

"The gradual lofting of the plume, with its top rising from ~21 km over the tropics in March to ~24 km in the same general location several months later, shows the signature of stratospheric dynamics in the CALIPSO stratospheric aerosol product."

Figure 13 and attendant analysis. A few questions and concerns. 1. It wasn't clear to me how K19 matched up the SAGE III data with CALIOP. It's not self evident how that would be done. Please clarify. 2. This figure and text comes in a section called "Discussion" but it is fundamentally a distinct analysis followed by discussion. It would be more logically set off in another titled section. 3. During this analysis timeframe there were background sulfates and fresher smokes from BC2017. The lidar ratios would not be a constant. How have K19 considered this situation?

This figure is the first attempt to obtain the stratospheric lidar ratio using backscatter measurements from CALIPSO and extinction data from SAGE III, using the limited amount of contemporaneous data available at this time. Hence we have done it only in a climatological sense. In other words, we have not used strictly coincident profiles from each instrument as such. We have added the following to clarify this:

"As for the comparisons presented in section 3, we have averaged the SAGE III data over each month and interpolated to the CALIPSO altitude grid and computed the lidar ratios, which were then averaged to obtain the climatological distribution shown in Figure 14."

Even though it is a new result, it fits in better in the context of the discussion on the lidar ratios. So we have decided to let this be a part of the discussion itself.

Please note that we have now replotted this figure by not using data from the biomass burning months of August through November 2017 and also all data after March 2018 to avoid contamination from the Ambae volcano. As can be seen in the revised figure, the results remain much the same despite loss of data.

Technical Matters:

In a few places Thomas Trickl's name is misspelled "Trickle."

P2, L14: "number" is a singular subject.

P5, L13: "The V4 data attenuated. . ." Delete "data."

P6, L21: ". . .the primary input files used for this product is. . ." Replace "is" with "are."

P9, L25: "The distribution. . .suggest.." Replace "suggest" with "suggests."

These points have been taken care of.

Figure 4 and other plots with latitude on x-axis: Please explain why the data in summer hemisphere don't extend as far poleward as in the winter hemisphere.

Added a sentence in the legend clarifying lack of data at high northern latitudes during nighttime in summer months.

P14, L13: "rising trend" This may suggest a nonlinear trend. Perhaps "positive trend" instead?

Done.

Figure 7. Show the tropopause.

Done.

Figure 8, bottom panels: What's causing the rainbow edge in the SH? Perhaps trim this off, or explain what is responsible for it.

Done.

P18, L1: ". . .lofting. . .plume from around 17 km. . ." The figure shows the plume starting out at _21 km, not 17 km. Please clarify.

We have revised the text as:

"The gradual lofting of the plume, with its top rising from ~21 km over the tropics in March to ~24 km in the same general location several months later, shows the signature of stratospheric dynamics in the CALIPSO stratospheric aerosol product.."

P19, L19: Regarding SAGE III ISS providing data "since March 2017. . ." The SAGE results reported herein start in June 2017. Please clarify.

Although the ASDC archives state the temporal coverage for the SAGE III aerosol data start from March 2017, the available data actually begins in June 2017 after commissioning. We have modified the sentence as: "The aerosol extinction profiles are available from the solar occultation measurements in 9 channels from 384 nm to 1544 nm starting June 2017."

References:

Friberg, J., et al., Atmos. Chem. Phys., 18, 11149, https://doi.org/10.5194/acp-18-11149-2018., 2018.

Hopfner, M., et al., Nature Geosc., https://doi.org/10.1038/s41561-019-0385-8, 2019.

Tobo, Y et al., Atmos. Res., 84, 233-241, https://doi.org/10.1016/j.atmosres. 2006.08.003, 2007.

Vernier, J.-P. et al., Geophys. Res. Lett., 38, L07804, https://doi.org/10.1029/2010GL046614, 2011.

Vernier , J.-P, et al., J. Geophys. Res. Atmos., 120, 1608, doi:10.1002/2014JD022372, 2015.

Vernier,J.-P.,et al., Bull. Am. Meteorol.Soc. **99**, 955–973, https://doi.org/10.1175/BAMS-D-17-0014.1, 2018.