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Interactive comment on "CALIPSO Level 3 Stratospheric Aerosol Product: Version 1.00 Algorithm Description and Initial Assessment" *by* Jayanta Kar et al.

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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. 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. A 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.

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.

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.

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.

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.

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.

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?

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.

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. P14, L22-24. 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. 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. 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. 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. 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?

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.

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.

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

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?

Technical Matters: In a few places Thomas Trickl's name is misspelled "Trickle." P2, L14: "number" is a singular subject. Replace "number...have" with "number...has" 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.” 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. P14, L13: “rising trend” This may suggest a nonlinear trend. Perhaps “positive trend” instead? Figure 7. Show the tropopause. Figure 8, bottom panels: What’s causing the rainbow edge in the SH? Perhaps trim this off, or explain what is responsible for it. P18, L1: “. . .lofting. . .plume from around 17 km. . .” The figure shows the plume starting out at ∼21 km, not 17 km. Please clarify. P19, L19: Regarding SAGE III ISS providing data “since March 2017. . .” The SAGE results reported herein start in June 2017. Please clarify.

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