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Interactive comment on “Evaluation of OMPS/LP Stratospheric Aerosol Extinction Product Using SAGE III/ISS Observations” *by* Zhong Chen et al.

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Reply to Reviewer 2 Z. Chen et al. zhong.chen@ssaihq.com

We thank Reviewer #2 for useful comments. Below we answer the reviewer’s concerns and make the necessary corrections to the paper and supplement.

The Stratospheric Aerosol Layer is an important component of the earth atmosphere through its impacts on climate and stratospheric ozone especially after large volcanic eruptions. Chen et al. (2019) evaluate the OMPS/LP stratospheric aerosol product using the SAGE III/ISS observations. They found a good agreement (+/-25%) between OMPS/LP and a modified version of the SAGE III V5.1 data between 20 and 28 km. OMPS/LP and SAGE III/ISS data are analyzed after a moderate volcanic eruption and

extreme fire reaching the stratosphere to highlight the contribution of those events on the stratospheric aerosol extinction. Finally, the sensivity of the aerosol retrieval to the assumed size distribution is also investigated at the end of the study. I have a number of major concerns about this paper before it can be published in AMT.

1- Introduction. The introduction does a very poor job in explaining why this work is important and why the stratospheric aerosol layer should be studied. I suggest the authors to do a literature overview of this topic to explain.

Reply: We add a paragraph to the beginning of the Introduction that reads “The Stratospheric stratospheric aerosol layer is an important component of the Eearth’s atmosphere through its impacts on climate and stratospheric ozone physico-chemistry (Vernier et al., 2011; Ridley et al., 2014; Bingen et al., 2017). The stratospheric aerosol layer was first observed by Junge in 1960 (Junge et al., 1961). Stratospheric aerosols that mainly originate from volcanic sources are from sulfur dioxide and carbonyl sulfide, which are both oxidized to sulfuric acid (Kremser et al., 2016). Recent measurements show that the background stratospheric aerosol layer is variable rather than constant, and the that changes in the stratospheric aerosol layer have caused recent warming rates (Solomon et al., 2011), indicating that it is important to monitor the stratospheric aerosol layer over the long term. The importance of possible changes in the background stratospheric aerosol layer lets to the analysis of each volcanically quiescent period (Deshler et al., 2006). For the considered period from January 1979 through the end of 2004, the variability of stratospheric aerosol layer is explored with measurements from spaceâĂŘbased instruments such as SAGE II (Thomason et al., 2008), CALIPSO (Winker et al., 2010), GOMOS/ENVISAT (Vanhellemont et al., 2010), SCIAMACHY (von Savigny et al., 2015), OSIRIS/Odin (Bourassa et al., 2007), SAGE III/ISS (Chu et al., 1998) and OMPS/LP (Loughman et al., 2018).”

2- Novelty of this study? As mentioned by the authors, a precedent paper was published last year to evaluate the new OMPS/LP aerosol product (V1.5) with the SAGE III/ISS data. This study aims to extend this analysis with one more year observations but do not further explain the scientific justifications for providing this update. What is so different between this paper and Chen et al. (2018) ? This is not justified with the publication of a new algorithm or new version of the OMPS dataset so why is it important to publish this?

Reply: The present study is a more comprehensive evaluation of the OMPS/LP aerosol product than previous one (Chen et al., 2018). In previous study, we were unable to evaluate the magnitude of the aerosol variability at the temporal scale at which this comparison is conducted. While the previous comparison used 7 months of SAGE data (V5.0), this comparison uses the latest version (V5.1) of SAGE III/ISS observations for the period from 2017 to 2019. In this study, as noted in the manuscript, the agreement and discrepancy between OMPS/LP extinction profiles and SAGE III/ISS observations are quantitatively assessed, some problems of aerosol retrievals from LP are identified and explained. Furthermore, a smoothed+interpolated method for SAGE profile is employed, which allows us minimize the uncertainty due to vertical resolution issue and to avoid ozone contamination. This evaluation will play an important role in future improvements of the LP aerosol dataset. We add the following text to the end of Introduction: "Our analysis of SAGE III/ISS data specifically addresses possible biases with OMPS/LP results arising from differences in vertical resolution and possible ozone contamination."

3- Justification for using CARMA in April 2012. An important part of the retrieval is the assumption of size parameters into the radiative transfer model to infer the aerosol extinction at 675 nm from OMPS/LP. The description of the algorithm (section2) provides the basis to understand how the extinction is inferred. A gamma size distribution is used to fit size distribution from the CARMA aerosol module running with GEOS model in April 2012. I have several questions associated with this approach: - Why do you use a Gama function to fit the model data? Bi-Lognormal distributions have been commonly used to fit stratospheric aerosol data such as those observed by the University of Wyoming for more than 30 years (Deshler et al., 2003) - It's rather strange to

use one month of model data as an input to constrain a retrieval algorithm. Moreover, the caveat is that the satellite output data will not able to be used by modelers using CARMA-GEOS since they are not independent to each other.

Reply: We realized that "using CARMA in April 2012" here is miscommunicated. It should be "the LP started observations from April 2012". We have corrected this wording by removing "and observations in April 2012" in the abstract. The reason why we use a Gama function to fit the CARMA model data has been intensively explained by Chen et al., (2018). While bimodal lognormal distributions is commonly used for the in situ instruments (Deshler et al., 2003; Deshler, 2008), most satellite instruments, such as OSIRIS, SCIAMACHY and OMPS/LP v1.0, use a unimodal lognormal distribution (Rault, et al., 2013; Damadeo et al., 2013; Rieger et al., 2014; von Savigny et al., 2015; Malinina et al., 2018). Chen et al. (2018) fitted four bi-modal lognormal distributions to the same underlying OPC measurements measured by the University of Wyoming. The resulting size distributions gave very similar values of Angstrom exponent, but significantly different phase functions due to the lack of information in the OPC data gap region (between 0.01μm - 0.1μm). The fitting results indicated that the abundance of smaller particles can significantly affect the phase function, and therefore affect our LP retrievals.

In the Introduction, we add a paragraph capturing these concerns. "We use the CARMA data to take advantage of CARMA's large range of particle size information, and a gamma size distribution represents a significantly better fit to the CARMA data than a lognormal distribution. In a recent study, Nyaku et al. (2019) confirms that the CARMA model agrees with the Wyoming OPC measurements." We hope this provides a justification for the use of the gamma function.

4- Modification/improvement of the SAGE III/ISS official product. In order to correct an apparent issue with the 675 nm extinction coefficient from SAGE III/ISS due to interference with ozone, the authors developed a new algorithm to interpolate the 675 nm channel data using 449 and 756 nm. Without providing further validation of this technique, the authors acknowledge that the new retrieved 675 nm coefficient from SAGE III is used for comparison and to some extent validate OMPS-LP. I think the approach is questionable here: Without further validation of the new retrieved 675nm extinction coefficient from SAGE III/ISS, you assume that it will be your new reference to compare and validate OMPS-LP. I think OMPS-LP should first be validated/compared with the official product from SAGE III/ISS at 675 nm before transforming the SAGE III data.

Reply: We did compare LP data with the official product from SAGE III/ISS at 675 nm in the beginning of this work and shared our initial results with SAGE science team. As the SAGE science team pointed out, bias in ozone result in a bias in the official aerosol product from SAGE III/ISS at 675 nm. To avoid ozone contamination, we developed a new algorithm to interpolate the 675 nm channel data using 449 and 756 nm per their suggestion. Our results show that the differences between the original and interpolated profiles below 27 km appear to be minimal. The SAGE team started an ozone validation paper (currently under review in JGR:A) where they brought this up in the context of how apparent biases in the aerosol spectrum likely originate from how ozone is retrieved and the impact on the ozone product (though the cause is ozone and the effect is aerosol). This paper does discuss the retrieval algorithm a little more. We now provide this reference: Wang, H.J., R. Damadeo, D. Flittner, N. Kramarova, G. Taha, S. Davis, A.M. Thompson, S. Strahan, Y. Wang, L. Froidevaux, D. Degenstein, A. Bourassa, W. Steinbrecht, K. Walker, R. Querel, T. Leblanc, S. Godin-Beekmann, D. Hurst, and E.J. Hall, E.: Validation of SAGE III/ISS Solar Ozone Data with Correlative Satellite and Ground Based Measurements, JGR Atmos., 2020, in review.

Comments: 1) P1-l16: “has been flying”. I believe that this expression could be improved in a scientific publication

Reply: We have modified the sentence as “ . . . has been taking limb-scattered measurements from April, 2012–present.”

2) P1-l29: “high degree of correlation”. Quantify here.

Reply: We quantified this by changing this sentence to the following: “and the differences between the two instruments vary from 0% to ±25% depending on altitude, latitude and time.”

3) P1-L32: “systematically lower: : :” You are not measuring the same air masses so the different between the two instruments are expected to be high at the tropopause or below.

Reply: Agreed. The tropopause is a very dynamical and complicated area. We have added text in the end of Section 3.2: “Furthermore, the large different between the two instruments are expected at lower altitudes near and below the tropopause because of larger variability in the transport of air masses.” and Section 4.3: “Near or below the tropopause, big disagreements between the two instruments can also be expected due to the variability of the transport of air masses.”

4) P1-L34: “altitude dependent...” Not only altitude but also latitude.

Reply: Here we emphasized the fact the actual aerosol size distribution is altitude dependent. We prefer to keep unchanged.

5) P2-L3: “cloud contamination”: That is of the main issue, which is poorly discussed in this paper.

Reply: In Section 3.2, we have added the following text: “Cirrus cloud contamination could be another issue in the lower stratosphere below about 19 km. Clouds appear as discontinuities of the limb radiance vertical profiles, which will tend to bias the retrieved result. Although most clouds are detected and filtered from the LP retrievals, it is not always possible to completely eliminate cloud contamination.”

6) P2-L8: “ (Ridley et al., 2014) Â′ n. There is a very poor review of the available literature on this topic. This should be improved.

Reply: We have added more references. Please see Reply to Comment 1.

7) P2-26: "has become operational..." What does it mean here?

Reply: The sentence has been modified to be "The version 1.5 OMPS/LP aerosol products are now being processed routinely"

8) P2-L27: "..A more comprehensive..": Does it justify another publication on OMPS-LP?

Reply: We added "more" here.

9) P4-L10: "The SAGE III/ISS developed..": Something is missing between SAGE III/ISS and the verb. It does not read well.

Reply: We add missing "which".

10) P5-L10: "Cloud height rejected: : :": How is cloud top height inferred from OMPS-LP? \

Reply: This has been made clearer with: "The current cloud detection algorithm (Chen et al., 2016) detects cloud top height from the OMPS/LP measurements using the spectral dependence of the vertical gradient of radiances at 675 and 868 nm. Cloud top height is identified when the gradient difference increases above 0.15. All LP data below the cloud top height were rejected because extinction changes abruptly at cloud top."

11) P6-L19: "Figure 11..": You should remove a reference to a figure that you do not explain at this stage of the paper.

Reply: We have removed "(see Figure 11)".

12) P7/Figure7. Figure 7 does not really highlight nicely the emergence of new stratospheric aerosol layers before and after the Ambae eruption and the fire in Canada. I would rather suggest producing an anomaly plot before and after each event.

Reply: This is a nice suggestion. We replot data in Figure 7 by adding anomaly results. The paragraph in the end of Section 4.2 has been rewritten to be "Figure 7 highlights the emergence of new stratospheric aerosol layers before and after the fire event in Canada in late 2017. Both instruments clearly captured the stratospheric aerosol perturbation triggered by the reported PyroCb. We chose July 6, 2017 as the "before" case (Figure 7a), based on the results shown in Figure 5, and then calculated the difference for LP and SAGE separately using November 9, 2017 as the "after" case (Figure 7b) to show the anomaly results. As expected, Figure 7c shows large positive extinction anomalies of up to 140 % below 22 km. The comparison also shows good agreement between LP retrieved extinction profiles and observations from SAGE III within $\pm$20 % at 18-28 km."

13) P7-10. How sure are you that the corresponding increase in extinction is associated with this eruption? Provide reference or further analysis to make your point.

Reply: We provide here three references: "Vernier et al., 2011; Kremser et al., 2016; Bingen et al., 2017."

14) P7-L23. "were produced.." use a better verb than "produce" here.

Reply: This sentence has been rewritten as the following: "The occurrence of PyroCbs triggered by intense wildfires in British Columbia, Canada were reported on August 12, 2017".

15) P8-L8. "..for the main aerosol layer.." What do you mean by "main layer", the Junge layer? The stratospheric aerosol reservoir in the tropics? Be more accurate so that the reader can understand what you're talking about.

Reply: We dropped "for the main aerosol layer" as it not accurate.

16) P8-L27-28: "The results: : :" I do not understand this sentence. Please rephrase and improve.

Reply: The sentence as rewritten as: "that when taking into account the altitude dependency of stratospheric aerosol properties, the LP algorithm performance improves at most levels.”

17) P9-L18: “are easily associated..” It does not read well in English. Please improve.

Reply: We replaced “easily” by “possibly”

18) P9-L19: You need to include references here.

Reply: Reply: We added five references: Schuster et al., 2006; Kremser et al., 2016; Rieger et al., 2018; Malinina et al., 2019.

19) P12-L12: “..broken clouds..” What do you mean by broken clouds, cirrus clouds?

Reply: “ broken clouds” is replaced by “thin cirrus clouds”.

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