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Atmospheric Measurement Techniques Discussions

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 1 Z. Chen et al. zhong.chen@ssaihq.com

We thank Reviewer #1 for reading our paper in detail and providing useful comments and suggestions. Below we answer the reviewer's concerns and make the necessary corrections to the paper.

General Comments: The authors propose a similar study as in a previous paper (Chen et al., 2018) based on a much larger temporal range and enriched datasets. We could thus expect a more in-depth analysis of the behaviour found in the LP dataset. The result as appears in Section 4 is particularly disappointing in this respect: the analysis is superficial, poorly grounded, and sometimes reduced to truisms. Some statements are

potentially misleading and might propagate wrong information if they are further cited without too much care in future publications. An appropriate uncertainty assessment should play a central role in such an evaluation, but there is in this paper no estimate or use of the uncertainties on the measurements considered in the intercomparisons, except for the standard deviation of the binning. The term "uncertainty" is used several times, but as a vague concept without serious analysis. This should be clearly improved before a possible publication.

Reply: We thank Reviewer #1 for a careful review of this paper. We believe that the present study is a more in-depth analysis of the behavior found in the LP dataset. The central scope of the paper is the evaluation of the LP algorithm performance for background aerosol situations. 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. The measurement uncertainties which discussed in details in our previous papers (Loughman et al., 2018; Kramarova et al., 2018) are estimated to be < 1%. Since the small measurement uncertainties have weak impact on the intercomparisons, we focus on the retrieval uncertainties which play a central role in the evaluation of the algorithm performance. We believe we have appropriately assessed the retrieval uncertainties in Section 4. We now add text to mention the measurement uncertainties (see Reply to Comment on L. 30, p.6-L. 2, p.7). In any cases, we should have made some statements much more clear in our manuscript and have now done so. We answer the points one-by-one below.

Specific comments L. 26-30, p.1: Aren't these two sentences telling the same thing?

Reply: The first sentence highlights the results of statistical analysis. The seconded sentence told us that LP can capture the variability of stratospheric aerosol layer well. We rewritten the second sentence as "Comparisons of extinction time series show that the both instruments capture the variability of stratospheric aerosol layer well. The differences between the two instruments vary from 0% to ±25% depending on altitude, latitude and time."

L. 26-28, p.1: Do the authors mean: "In this altitude range, the slope parameter (. . .)? This precision might be useful in view of what is say from l. 31.

Reply: Yes, we have added "In this altitude range" to the beginning of the sentence.

L. 31-32, p.1: It would be useful to quantify the biases using the results mentioned in Section 4.3.

Reply: We have added "with significant biases up to $\pm$13%".

L. 2-3, p.2: And what about the influence of the viewing configuration?

Reply: We have added "the influence of the viewing configuration".

L. 21-25, p.3: Following Figure 1, the AE of the fitted ASD (=2.08) roughly correspond to the values found by SAGE II between 20 and 25 km in the post-2000 period, which is characterized by a load volcanic background. It should be noted that in almost all other cases (including the 1989-1990 period, also characterized by a very low aerosol content), SAGE II values are significantly lower. Hence, we can expect that this choice biases the results obtained in the case of medium to high volcanic load. This should be mentioned.

Reply: We have modified the sentence: "... during the period 2000-2005, which is characterized by a low volcanic background ... Other time periods known to have low aerosol loading (e.g. 1989-1990) show lower values of AE in the SAGE II dataset. Thus, the reference ASD adopted here for LP retrievals may produce a bias in extinction values derived during medium or high volcanic aerosol loading."

L. 27-30, p.3: This variability also and more significantly depends on the aerosol load, in the stratosphere, especially if a large time period is considered as on Figure 1!

Reply: We have added text "This variability also and more significantly depends on the aerosol load, in the stratosphere, especially if a large time period is considered as in Figure 1"

L. 2-3, p.4: This sentence requires a reference.

Reply: We have included a reference: "(Chahine, 1970)".

L. 5-6, p.4: It would be useful to specify why the authors foresee such a limitation on the SZA.

Reply: OMPS LP only measures scattered sunlight (which limits observations to SZA < 90 degrees), and the radiative transfer algorithm used for LP retrievals becomes less accurate at very high SZA, so we cut off retrievals at SZA = 88 degrees to limit that possible source of error. We modified sentence: "For the LP aerosol product, retrievals are only performed for daytime observations (solar zenith angle SZA < 88°)".

L. 3-4, p.5: What do the authors mean by "using SAGE samples that correspond to the OMPS/LP 1 km altitude grid"?

Reply: This sentence has been removed as it is not essential.

L. 5-7, p.5: In this work as in Chen et al. (2018), the fact that the datasets are zonally averaged over the whole longitude range leads to neglecting the signature of local events such as moderate volcanic eruptions. Such kind of comparison is thus not sufficient to assess the ability of the OMPS retrieval to quantify correctly extinction in case of medium to large volcanic eruptions, because their signature is diluted in the whole considered event population. In particular, the authors cannot assess adequately the effect of the weakness of the limitation of the ASD to the case of low aerosol content (See comment on l. 21-25, p.3) when using such zonal averaging. This should be at least mentioned, and either the authors should clearly mention that they restrict the scope of their paper to background aerosol situations, or they should reproduce their study over averaging windows limited in longitude and latitudes, and centered on important volcanic eruptions that occurred during the measurement period.

Reply: This was one of the major criticisms raised by the reviewer in the overall remarks through the review. We agree that the use of zonal averaging over the whole longitude range leads to neglecting the signature of local events. As indicated in Chen et al. (2018) and this manuscript, the LP retrieval is performed using background layer size distribution parameters, and assumed ASD is appropriate for OMPS/LP aerosol extinction retrievals in most of the stratosphere for background aerosol. We believe that our comparison is sufficient to assess the ability of the OMPS retrieval under background aerosol conditions when using such latitude zonal mean.

We clarify this by changing this sentence to read "The daily averaged data in the same latitude bin and on the same day were used for the comparison with an assumption that the variation with longitudinal bands is much smaller than the variation with latitudinal bands. For background aerosol situations, the longitudinal variation of stratospheric aerosol could be small because of efficient mixing in the zonal direction and strong horizontal transport prevailing in the region (Sunilkumar et al., 2011). In cases of medium to large volcanic eruptions, however, longitudinal differences could be large and a restriction should be placed on longitude to capture the signature of potential longitudinal variation."

We added text to the Abstract: "...were available to evaluate our ability to characterize background aerosol conditions"

We also added two sentences to the Introduction to emphasize this. "The assumed ASD is designed to represent the long-term background stratospheric aerosol loading" and "the central scope of the paper is the evaluation of the LP algorithm performance for background aerosol situations".

L. 7-10, p.5: Does it mean that the dataset corresponding to the side slits is actually not validated? The authors should specify if this is the case or not (with some reference if relevant), and discuss the impact of this limited intercomparison exercise on the validation of the OMPS dataset.

Reply: We analyzed the aerosol retrievals from left and right slits. Our internal analysis shows differences between the three slits. Those differences are relatively

small compared to the overall magnitude of the adjustments (both static and intra-orbital+seasonal) for any single slit. Only center slit results are shown for clarity, and that these results should be representative of results derived using left slit or right slit measurements. As mentioned in the manuscript, the reason why we focus on analyses of LP data from the center slit for the comparison is that the center slit has better straylight and tangent height corrections compared to the left and right slits (Moy et al., 2017; Kramarova et al., 2018). "

L. 13, p.5, L. 28, p.10: What does "MLR" mean?

Reply: This is just the short name of the product as given by the SAGE team, not an abbreviation. This is why it's in quotations. (Technically, MLR does stand for multiple linear regression because the algorithm performs it, but that's misleading because essentially all of the products derive from some form of multiple linear regression.)

L. 11-15, p.5: These references are particularly poor. In particular, Thomason and Taha (2003) only mention that "the aerosol product is produced as a residual following the removal of the effect of ozone and other species", and nothing more about the methodology. Overall, the mention about biases appears much less as a strong and well-founded statement in Thomason and Taha (2003) than what seems in the present paper. The authors should provide another reference with more solid grounds to support the discussion? Reply: The 2003 reference is removed as suggested. This whole concept of biases came up from internal investigations into the data, none of which is published yet. As such, the "private communication" is appropriate. 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.

L. 17, p5: The authors should specify the type of interpolation used here. Is it just a linear interpolation?

Reply: We have added "using a log-linear interpolation".

Caption of Figure 3: The authors should be more specific than "below 21 km". What is the lower altitude they consider?

Reply: We replaced "above 27 km below 21 km" by "above 19 km" in consisting with text.

Figure 3 and L. 1 p.6: It is quite striking how LP and the smoothed+interpolated SAGE profile (supposed to be particularly reliable with removed influence of ozone) are similar above 19.5 km, and much more disagree below this altitude. Which influence should be seen here? Clouds, other aerosol types (e.g. dust)? Or again, thicker aerosol particles as expected at such altitude (See e.g. Deshler et al., J. Geophys. Res, 108,(D5), 4167, 2003; and Bingen et al., J. Geophys. Res., 109, D06201, 2004)? The link should also be made with the discussion in Section 4.3. Reply: We admit we are unclear what the viewer is referring to by "thicker aerosol particles", since no that term is presented in the two references suggested by the reviewer. Perhaps the reviewer is referring to "thick aerosol layers" or "larger particle size"?

We now use the term "thick aerosol layers" and have added the following text: "The figure shows how LP and the smoothed+interpolated SAGE III profile are similar above 19.5 km, and have more disagreement below this altitude. In the upper troposphere and lower stratosphere (UTLS), particularly in the tropics, the presence of thicker aerosol layers may have an impact on the retrieved extinction levels (Kremser et al. 2016)."

This event is also mentioned in the end of Section 4.3: "Another important aspect that influences the comparison below 19 km is the presence of clouds and thicker aerosol layers discussed earlier (Figure 3)."

Figure 3: What about the error bars?

Reply: We have now added the error bars in the two panels in the figure with associated text.

Figure 4: In view of the importance of the aerosol characterization in the upper troposphere and lower stratosphere (UTLS), it would be very useful and interesting to extend such kind of comparison to lower altitude, even if the agreement might be less impressive at these altitudes, as one can suspect from Figure 3.

Reply: We expanded the comparison to a lower altitude at 15.5km in Figure 5 with relevant text as suggested. The updated figure depicts time series comparisons at15.5 km for different latitude bands outside the tropics where clouds are not an issue (see Reply to Comment on L. 5-7, p.5 in details)

L. 9-12, p.6: Again, since the binned curves compared here are likely to cover a much larger extend than the one of the volcanic cloud, even if the signature of the Ambae eruption is clearly visible here, the extinction profiles are probably a not representative of the extinction in the aerosol cloud. Consequently, I don't think that anything can be concluded here about the adequation of the assumed ASD to describe medium to high volcanic aerosol cases. Hence, I find the statement that an agreement within 20% between LP and SAGE III/ISS shows that the assumed ASD is "reasonable" in this case, really premature. See also comments on L. 5-7, p.5.

Reply: This was the same criticism in Comment on L. 5-7, p.5. We have repeated that we did not mention "medium to high volcanic aerosol cases" in this section, but we have modified the text as recommended to read the following:

"For the background aerosol condition based on the June, 2017 to June 2018 period, LP and SAGE are within 20% of each other at 20.5 km and 25.5 km, indicating that the assumed ASD in LP v1.5 algorithm is adequate in this altitude range."

We also changed the section title to "Aerosol Extinction Variability".

L. 15-19, p.6: Why would the dependence in temperature and humidity affect more the agreement between both datasets than the use of the assumption of "background aerosols" at lower height (e.g. 20.5 km)? Hasn't this dependence in the atmospheric conditions a lower-order impact on the LP retrieval than the choice of ASD? The authors should also explain why they expect that this effect of "wrong assumption" on the index of refraction would affect LP more than SAGE III/ISS.

Reply: While the SAGE algorithm does not make any assumptions about aerosol microphysics, the LP algorithm assumes an aerosol refractive index which varies significantly with temperature (Russell et al, 1996) and, therefore, has a direct effect on the aerosol scattering phase function.

We have now added text: "While the SAGE algorithm does not make any assumptions about aerosol microphysics, aerosol extinction profiles from OMPS/LP suffer from uncertainties due to assumed ASD and refractive index"

L. 13-14, p.6: This statement and the references to the papers by Pumb and Bell (1982) and Thomason et al. (2008) is not sufficiently grounded. The time period here is hardly longer than 2 year and is totally insufficient to assess any QBO effect. The only thing Plumb and Bell are 'referring to in their 1982 paper about the altitudes around 30 km is, to the best of my knowledge, that this height roughly corresponds to the maximum zonal wind amplitude of the QBO. And Thomason et al. (2008) mention in only one sentence, without any further discussion, that they observe significant aerosol variability with a period similar to the QBO at 30 km. As a conclusion, nothing convincing in the cited works seems to support the statement made here. If the authors find anything in the behaviour observed at the 5 considered latitude bands likely to reflect any QBO influence, this should be discuss appropriately. Otherwise, the authors should re-

move this sentence. And both citations should be removed unless the authors are able to formulate some arguments showing that they are really relevant in this particular context.

Reply: We have removed the two citations as requested and revised the sentence as: "The large temporal variability present in both datasets at 30.5 km is suggestive of a quasi-biennial oscillation (QBO) signal, although there is a shift in phase between latitude bands."

L. 21-23, p.6: This sentence is particularly empty. What do the authors mean by "very similar"? The very good agreement found at 20.5 km in Figure 4 is much less obvious in Figure 5; the disagreement found at 30.5 km might be more important in Figure 4 than in Figure 5, although a clear lack of data in the SAGE III/ISS doesn't allow any conclusion in some cases. And the situation at 25.5 km might show a kind of mix of data gaps for SAGE III/ISS and increased disagreement in Figure 5. Furthermore, observing that "aerosol extinctions are highly variable in altitude and time" is particularly uninformative: what is "highly variable"? One could argue that, as shown on Figure 5, the curves are quite flat – which is obviously a matter of choice of Y-scale.

Reply: We removed this sentence as it is uninformative. We also removed the comparison at 30 km from Figure 5 as it is less important than in Figure 4.

L. 24, p.6: The fact that LP sees seasonal variations not seen by SAGE III/ISS and that this is interpreted as a weakness of the retrieval caused by "ASD errors and limitations" is a serious issue. Do I understand well that this concerns plots in the range 35âŮęS-55âŮęS at 20.5 km? The amplitude of winter minima found here is particularly important (about 25%?) and is not reflected in any error bar, what is a worrying issue.
Reply: We thank the reviewer for pointing out this. To address this issue, we replot data in Figure 5 by applying scattering angle filter and add a new section (4.6 limitations of Wavelength). The paragraph in Section 4.1 has been rewritten to be "Figure 5 shows the time series comparison between OMPS/LP and SAGE III/ISS measurements at

15.5 km (left panel), 20.5 km (middle panel) and 25.5 km (right panel) for different latitude bands outside the tropics where clouds are not an issue. The blue and black dots in Figure 5 represent the LP extinction calculated at scattering angles (SA) < 145° and > 145°, respectively. Again, the highly variable nature of the stratospheric aerosols with time and latitude is well represented by the two instruments. Canadian pyro-cumulonimbus (PyroCb) was most probably responsible for increasing aerosol extinction values at 15.5 and 20.5 km in the 35°N-55°N latitude bands in late 2017, and the effect of Mt. Ambae can be seen in the 35°S –55 °S time series in late 2018. Good agreement is found between both instruments at 20.5 and 25.5 km, although there are some negative biases in the SH. In contrast to the results for altitude at 30.5 km (right panel in Figure 4), the LP values at 15.5 km are systematically smaller than the ones from SAGE III. A discussion of this systematic difference is given in Section 4.3. A notable feature in Figure 5 is that the comparisons in the Northern Hemisphere (NH) are generally better than in Southern Hemisphere (SH). In the southern mid-latitudes (35°S-55°S), the LP retrievals show significant seasonal variations at 20.5 and 25.5 km that are not seen by SAGE. The obvious seasonal variability in the differences, with the amplitudes of winter minima was observed to be about 25% at 20.5 km, and as much as 200% at 15.5 km. The presence of erroneous seasonal variations in the OMPS/LP dataset is mostly caused by limitation of wavelength at 675 nm when observing in backscatter condition at extreme large scattering angles. The results lead us to recommend filtering LP data below 20 km with SA greater than 145°. The limitations of the LP retrievals at 675 nm will be addressed in Section 4.6."

L. 25, p.6: What do the authors mean by "limitation of 675"Âa?ËŸ

Reply: That meant limitation of the limb radiances at 675nm. We have added a new section (4.6 limitations of Wavelength) to address it.

L. 25-27, p.6: Could the authors verify their statement about the influence of Canadian PyroCb from 2D maps? Otherwise, they should qualify: "was most probably responsible (. . .)".

Reply: We have added "most probably" in the sentence as requested.

L. 27-29, p.6: The reduced data coverage in the case of SAGE III/ISS doesn't allow any relevant conclusion about a maximum limit of the disagreement between both datasets in the sense that the most prominent patterns found by LP are not observed by SAGE III/ISS. The authors should thus avoid such a quantitative, possibly misleading estimate of the agreement between both datasets.

Reply: We have removed 'up to about a factor of 4' from the sentence as requested.

L. 30, p.6-L. 2, p.7: The sentence "For SAGE, . . ., for OMPS/LP" seems to be an explanation of the statement "Under low aerosol condition". However, straylight contamination and sensitivity to small aerosol particles are two independent concepts.

Reply: We have modified the text to make this clearer: "Additionally, uncertainties in LP measurements are assumed to be 1% (Kramarova et al., 2018) and the primary source of error in LP radiance measurements is the straylight error which increases with altitude. At higher altitudes where LP radiance is small, the straylight error becomes most significant."

L. 8-9, p.7: A typical daily latitudinal coverage in a given bin (if it contains any measurement) is about 0.3âŮę for SAGE III/ISS, which is very small compared to the bin latitude resolution of 5âŮę. The SAGE III/ISS are regularly spread over the whole longitudinal dimension, so that few points concern the region of the eruption. Did the authors check that the sampling provided by LP and mainly SAGE III/ISS, with respect to the spread of the volcanic plume, is adequate to conduct a relevant intercomparison? Otherwise, they should repeat their comparison by using a more adequate choice of bins (e.g. a more focussed region, possibly during more days). This is very important in order to distinguish the part of algorithm performance and the part of mismatch in the differences observed here. And these aspect should be at least discussed adequately.

Reply: This isn't entirely accurate. Daily SAGE latitudinal sampling widths vary by latitude. In the tropics, the spread can be large (up to 10 degrees). The spread can be as low as less than a degree at the sampling turnover point, but the latitude of that location changes seasonally (though it's always at the higher end of the latitude range). Summarily, a 5-degree bin width may actually be narrower than the SAGE sampling in the tropics and a little wider to much wider at mid-to-high latitudes. We agree your comments about using a more adequate choice of bins (e.g. a more focussed region, possibly during more days), but we think that SAGE III has enough profiles (about 12-14 SAGE profiles in each bin) within the volcanic plume to represent it, particularly in the early period where there is not much of longitudinal spread.

L. 22-24, p.7: The authors should mention the highest altitude reached by the PyroCb, in order to provide an insight about its expected impact on the aerosol profile.

Reply: We have added text: "Within two months after injection the plume can reach up to nearly 22 km."

L. 30, p7-L.2, p.8: What is the expected impact of the wrong aerosol type (sulfate instead of carbonaceous aerosols) and possibly inadequate choice ASD in this case? Is this impact expected to be stronger in the case of LP than in the case of SAGE III/ISS? This should be discussed.

Reply: Again, SAGE doesn't need to make assumptions about the type of aerosol for the retrieval of extinction. However, LP needs to assume a refractivity index which is mainly dependent on aerosol type. We added explanatory text: "Since the LP algorithm assumes a fixed refractivity index which is dependent on aerosol type, the wrong aerosol type (sulfate instead of carbonaceous aerosols) leads to an incorrect phase function, which then produces an error in the retrieved extinction. For SAGE, this assumption is not needed, so there is less impact."

L. 17-19, p.8: I guess the 1.8 values is close to the reported averaged SAGE II values at 18 km, *in the case of reduced aerosol load*. It is anyway not representative for a high aerosol burden. This should be specified.

Reply: We thought that it was clear that 1.8 values is close to the reported averaged SAGE II values at 18 km for the same stable background aerosol loading period (2000-2005), but have added the words "in the case of reduced aerosol load" to the end of this sentence."

L. 27, p.8: "the vertical variability of stratospheric aerosol properties": the authors should be more specific.

Reply: We have changed the words to "the altitude dependency of stratospheric aerosol properties" for clarity.

L. 31, p.8: "(. . .) to see if an ASD error exists": the authors should reword this strange sentence and specify what they mean.

Reply: Thanks for pointing out this poor wording. We have corrected this wording, removing "to see if an ASD error exists," and replaced it with "to examine if the assumed ASD is reasonable".

Title of Section 4.4: The authors announce a discussion about the correlation between the extinction and the Angstrom exponent, what seems to foretell some rigourous study with appropriate calculations of the correlation between these quantities. But the reality appears to be (apparently) that (fast) conclusions are drawn from some visual examination of the similarity between two plots. The discussion falls thus short of the expectations. The methodology looks insufficient and either it should be revised, expanded or clarified, or the authors should change the title of the section. See also comment on L. 24, p.9.

Reply: The reviewer is correct that the discussion is too short to justify a separate section, so we removed the title and treating this section as the closing paragraph of Section 4.3

L. 10, p.9: What do the authors mean by "vertically smoothed SAGE III/ISS data"?

Reply: We describe our vertical smoothing procedure in Section 3.2 (Equation (2))".

We have modified the text "... derived from the vertically smoothed SAGE III/ISS data calculated using Eq.2"

L. 11, p.9: Is the SAGE extinction at 675 nm processed as previously (interpolation using 2 close spectral channels)? This should be specified.

Reply: We interpolate to get a replacement 675 nm extinction data set for SAGE III using the method described in Section 3.2. Now we are saying that we need extinction at 520 nm, which we obtain using a different interpolation method which is clearly explained in the text.

L. 14-17, p.9: The formulation is confusing. Do I understand well that the authors interpolate the aerosol extinction at 520 nm? If the interpolation makes use of a second order polynomial, why do they need 4 channels? Or are they rather fitting such polynomial? Is this approach more accurate than just deriving directly the AE using a linear fit of the aerosol extinction on a log-log scale?

Reply: We can clarify slightly by changing the beginning of the sentence to: "However, to avoid potential bias, the aerosol extinction is first interpolated to 520 nm using a second order polynomial ..." The shape appears mostly quadratic (or perhaps quartic) but it is definitely not linear across the large spectral range.

L. 17-19, p.9: This sentence is confusing and should be more accurate. The values above 2.08 found above 25 km height are not associated to the Mt. Ambae eruption, although they constitute the majority of the category of values > 2.08.

Reply: This can be further clarified to: "AE values above the baseline are possibly associated with the volcanic eruption of Mt. Ambae below 25 km (note transport from the QBO) and aerosol evaporation at higher altitudes (particularly in the tropics)."

L. 19-20, p.9: AE below the baseline (=2.08 following L. 17, p.9) are all values plotted in blue colours in Figure 11a. It is very hard to believe that all these values are easily associated to clouds. What do the authors mean? The authors should also specify

where they see the influence of PyroCb.

Reply: This can be clarified: "AE values are typically below the baseline through the mid stratosphere in non-volcanic conditions, though the smallest values in the upper troposphere are possibly associated with clouds (and a minor influence from the PyroCb in the lower stratosphere at the northern extent of the plotted data in late 2017)."

L. 21-22, p.9: I guess the authors draw this conclusion from Figure 11b. It might be useful to specify it for the sake of clarity. Furthermore, this negative bias, following Figure 11b, doesn't cover the whole stratosphere and the authors should be more specific. Finally, it seems from Figure 11b that the vertical extend of the negative bias decreases with time. Is this due to ageing of the instrument? Or do the authors have another interpretation for this general trend?

Reply: The reviewer is misinterpreting the statement. The word "bias" in this statement is not referring to differences between OMPS and SAGE aerosol extinction. This can be clarified by changing "There also appears to be an overall bias towards an AE value slightly below the baseline throughout most of the stratosphere." to "SAGE II data suggest that the overall AE throughout most of the stratosphere is slightly below the baseline value of 2.08 used by the OMPS algorithm." Further, we would not say Figure 11b suggests any kind of trend in the difference. Rather, it is more likely that the changes seen are in-line with the phase of the QBO, though perhaps that needs to be shown or at least stated that we looked into it.

L. 22-23, p.9: Where is this statement coming from? The difference between LP AE and AE derived from SAGE is not shown anywhere. Do they mean SAGE III/ISS AE illustrated in Figure 11a? Otherwise, they need to show appropriate results to support their statement.

Reply: This is part of the foundation of this entire Section and thus illustrates where much of their confusion is coming from. OMPS/LP uses a flat value of 2.08, which is stated clearly in the text. Figure 11a uses this value as the zero on the color scale and so it shows the difference between AE from SAGE and AE used for OMPS by virtue of the color rather than by value. We added text: “Red colors in Figure 11a thus represent SAGE III/ISS AE values larger than the OMPS/LP AE value, while blue colors represent AE values smaller than the OMPS/LP value”

L. 24, p.9: There is of course a strong correlation between AE and ASD, but these quantities are still different. Furthermore, visually, the altitude range takes roughly blue colours in similar regions of both plots in Figure 11 (say, the “lower stratosphere”), but the details of the time evolution of this altitude range is different in Figs. (a) and (b) and the correlation between the negative values (the various blue tones) cannot be assessed without an adequate calculation. Therefore, saying that there is “a clear correlation” is absolutely premature (this apparent correlation might be purely fortuitous), and should be removed or clearly qualified.

Reply: We revised the text as “The similar structure between AE variations shown in Figure 11a and the extinction differences shown in Figure 11b suggest that the use of an altitude-dependent ASD in the LP retrieval is a significant component of the observed extinction differences, although other factors may also contribute.”

L. 25-27:, p.9: Where is this statement coming from? The only results shown about southern mid-latitudes are the ones in Figure 5, and I don’t see how they could lead to these results. The authors should bring the necessary developments and/or explanations to support their conclusion, or remove this sentence.

Reply: The reviewer is correct that Figure 11 only shows the tropics. We removed the sentence and replace it with the following: “Section 4.5 gives further discussion of how the assumed LP phase function and LP measurement geometry can result in latitude-dependent variations in extinction differences with SAGE III.”

L. 10, p.10: The extinction units are missing.

Reply: We have added missing “m-1”.

L. 11, p.10: What do the authors mean by “wavelength limitations”?

Reply: This is a very good point. The reviewer also mentioned this point several times through the review (see Comment on L. 25, p.6; L. 24, p.6; L. 12-15, p.10; L. 11, p.10 and Figure 13). As indicated in the manuscript, the OMPS algorithm uses a single wavelength at 675 nm which results uncertainties in the southern mid-latitudes at lower altitude. The uncertainties may be reduced by doing LP aerosol retrieval at longer wavelengths.

To address this question, we added a new section (new Section 4.5) with a new figure (Figure 14) as follows: “4.5 Wavelength Impact on OMPS/LP Aerosol Sensitivity In Figures 5 and 12, the comparison shows asymmetry between the hemispheres below 20.5 km, with much better agreement in the NH than in the SH, and OMPS/LP extinction values are significantly biased at southern mid-latitudes below 20 km due to erroneous seasonal variations in the OMPS/LP dataset. That suggests the LP measurements are more sensitive to aerosols in the NH and less sensitive to those in the SH, especially at lower altitudes. Here we shall examine the sensitivity of the LP radiances to aerosol. As mentioned before, the LP V1.5 algorithm uses OMPS/LP radiances at a single wavelength at 675 nm to retrieve extinction profile. This wavelength was selected primarily to minimize aerosol-related errors in the ozone retrieval and to reduce stray light contamination (Loughman et al., 2018). However, as indicated in Sections 4.1 and 4.4, it is difficult to retrieve reliable aerosol extinction below 20 km in the SH due to lack sensitivity to aerosol. Figure 13 shows an example of aerosol weighting functions at 675 nm for three latitudes. The aerosol weighting function, which determines how the calculated radiance (I) at a given wavelength changes with a change in aerosol extinction (k), is denoted by: (∂ lnâĄą(I))/(∂lnâĄą(k)) (3) The derivatives are calculated for all altitudes for a change at each tangent height, and each curve in the figure shows the sensitivity of the radiance at a given tangent height to extinction perturbations of a 1 km layer at a range of altitudes. It can be seen that the sensitivity to aerosol varies with latitude and altitude. The LP radiance at 675 nm is most sensitive to the aerosol extinction over the 20-30 km altitude range in the tropics and the northern mid-latitudes, but less sensitive to aerosol in the southern mid-latitudes. This behavior is consistent with the results shown in Figures 5 and 12 that the LP retrievals are in better agreement with the SAGE data in the NH than those in the SH. In the limb scattering technique, on the other hand, the aerosol signal in the limb radiance at a given tangent height is roughly proportional to the product of the aerosol extinction in that layer and the PF at the tangent point. OMPS/LP is installed in a fixed orientation relative to the S-NPP spacecraft. Therefore, southern mid-latitudes are observed at backscattering geometries whereas NH observations are carried under forward scattering conditions (see Loughman et al., 2018, Figure 2). Aerosol scattering phase function is at least fifty times smaller for OMPS limb viewing in SH than in NH. Due to the variation of the PF with latitude and season, the LP observations are most sensitive to aerosols in the NH winter and least sensitive to those in the SH. LP scattering angles typically vary between 15 and 165°. For the selected wavelength of 675 nm and the assumed ASD, the PF has much smaller values at larger scattering angles (see Chen et al., 2018, blue line in Figure 2), corresponding to the larger solar azimuth angles at southern latitudes. This leads to a smaller relative contribution of aerosol scattering with respect to Rayleigh scattering. At extreme large scattering angle (>145°) where Rayleigh scattering is high and the value of the aerosol phase function is close to its minimum, the LP radiances at 675 nm are nearly insensitive to aerosol at lower altitudes. Therefore, the SA upper limit (= 145°) should be used to filter the data. Outside of this SA range, LP extinction values will tend to bias the retrieved result, as revealed in Figure 5. This cloud explain the presence of erroneous seasonal variations in the OMPS/LP dataset. Since the sensitivity to aerosol increases at longer wavelengths (Taha et al., 2011), the uncertainties in the southern mid-latitudes at lower altitude may be reduced by using longer wavelengths. Figure 14 illustrates the sensitivity of the limb radiances to the aerosol extinction at three wavelengths for latitude at 60 °S (solar zenith angle SZA = 70°). The sensitivity to aerosol is seen to increase with increasing wavelength. The increased sensitivity of the limb radiances to aerosol at longer wavelengths is partly due

to the fact that the Mie scattering from aerosol particles does not decrease as rapidly with wavelength as the Rayleigh scattering from air molecules (Bourassa et al., 2007)."

L. 11, p.10 and Figure 13: The concept of "aerosol weighting function" is never defined in the text and should be appropriately explained. Reply: We added an explanatory paragraph in the new section (Section 4.6). Please see Reply to Comment on L. 11, p.10.

L. 12-15, p.10: Could the authors shortly explain the reason for the different sensitivity to aerosol in the northern mid-latitudes and tropical latitudes, and the southern midlatitudes?

Reply: We add text: "OMPS LP is installed in a fixed orientation relative to the S-NPP spacecraft. Therefore, southern mid-latitudes are observed at backscattering geometries whereas NH observations are carried under forward scattering conditions. Aerosol scattering phase function is at least fifty times smaller for OMPS limb viewing in SH than in NH." Also please see Reply to Comment on L. 11, p.10.

L. 4-5, p.11: See comments on L. 24, p.6 and L.24, p.9. I don't agree about the conclusion on the robustness of the measured extinction variability.

Reply: The "robust" isn't used in a rigorous manner. We have removed this strong statement.

L. 8-10, p.11: Measurement uncertainties are never discussed nor quantitatively mentioned before.

Reply: This is discussed now by adding a new sentence in the end of Section 4.3 (see Reply to Comment on L. 30, p.6-L. 2, p.7).

L. 19, p.11: "differing" is not an adequate term. What is a "differing good agreement"?

Reply: We changed the text to "differing good agreement and much larger differences above 30 km and below 19 km".

L. 4-7, p.12: These aspects are discussed in a paper by Malinina et al. (2019), amt, 3485-3502. It seems appropriate to cite this paper.

Reply: The work of Malinina et al. (2019) has been mentioned and referenced in the manuscript.

L. 7-9, p.12: "Lower sensitivity" of what? Which kind of uncertainties are the authors talking about? To which specific (and different) concepts do the authors refer by "lower sensitivity" and "reduced retrieval accuracy"? The increased extinction uncertainty above 28 km and below 19 km mentioned in L. 5, p.9 seems to refer to retrieval uncertainty, while it is not clear if the uncertainty cited in L. 15 and 18, p.10 and linked to "noise amplification" refers to instrumental noise or retrieval noise. The discussion on uncertainty assessment is clearly insufficient.

Reply: It meant lower sensitivity of LP radiances at 675 nm to aerosols. The extinction uncertainty above 28 km and below 19 km mentioned in L. 5, p.9 is to refer to retrieval uncertainty. Here we are talking about the remaining discrepancies in the comparison are possibly attributed to the inherent uncertainties associated with different measurement techniques which affect the instrument sensitivity to aerosols. The "lower sensitivity of LP radiances at 675 nm to aerosols" is now discussed in Section 4.5.

We have also rewritten the paragraph about uncertainty in the Summary and Conclusions: "Our sensitivity analysis shows that the ASD error propagates into extinction uncertainty as much as 13%. This suggests that a dynamic model for ASD is needed to accurately retrieve aerosol extinction profiles. There are three other possible causes for the discrepancies. First, most of the remaining discrepancies in the comparison are possibly attributed to the inherent uncertainties associated with the measurement techniques. The differences in the measurement techniques affect the instrument sensitivity to aerosols (Malinina et al., 2019). While SAGE III/ISS uses the solar occultation measuring technique which is self-calibrating and derives extinction directly, OMPS/LP

employs the limb scatter technique where retrieved extinction depends on instrument calibration and tangent height registration as well as the inversion algorithm and its several assumptions including the assumed aerosol microphysics. Uncertainties in these assumptions can also affect the LP extinction product. . ."

L. 10, p.12: What do the authors mean by "straylight contamination is more obvious"?

Reply: We modified the sentence as "The limb radiance is also more susceptible to additive straylight contamination at high altitudes, where absolute radiance values are smaller."

L. 11, p12: The concept of "discrepancy" looks strange.

Reply: "a larger random discrepancy" is replaced by "large difference".

L. 11-12, p.12: This sentence should be qualified. It is not clear whether the authors consider the association between the large discrepancy between LP and SAGE, and the presence of clouds as obvious (in my opinion, it is not), or as a working hypothesis. Reply: We qualified the discrepancy as "as much as 60%"

Abstract and summary/conclusions: It seems appropriate and important to explicitly mention the presence of erroneous "seasonal variations" in the OMPS-LP dataset in the abstract and in the summary and conclusions, since this behaviour risks to induce misinterpretations in future works.

Reply: Thank you, this is an important point. We add a paragraph to the Abstract that reads "On the other hand, we find erroneous seasonal variations in the OMPS/LP dataset, which usually exists below 20km in Southern Hemisphere due to the lack of sensitivity to particles when scattering angle is greater than 145°."

We also add a paragraph to the end of the Summary and Conclusion that reads "Another significant finding in this study is that the comparison shows asymmetry between the hemispheres below 20.5 km, with better agreement in the NH than in the SH, and erroneous seasonal variations at southern in the OMPS/LP dataset with an estimated uncertainty of a factor up to two. The reason for this is identified as that the LP radiances at 675 nm are nearly insensitive to aerosol at extremely large scattering angles. This problem can be solved by using longer wavelengths to retrieve aerosol extinction. In the meantime, we recommend filtering LP data below 20 km with scattering angle greater than 145°. When scattering angle exceeds this limit the LP algorithm starts to give obviously wrong results."

Technical comments General remark: The authors make use of both SAGE II and SAGE III/ISS datasets. Sometimes, they refer to "SAGE". They should be more specific.

Reply: Fixed.

General remark: I guess the correct spelling is "mid-latitude" instead of "midlatitude".

Reply: Fixed throughout the paper.

L. 22, p.3: The units should be corrected (microns instead of m).

Reply: The word document that we see is μm, not m. In the pdf version, however, it was changed. Now we reconvert word to pdf and see the same.

L. 22, p.6, L23, p.8, Caption Figure 11, l.11, p.9, etc.: "extinction" is a physical parameter and should be singular. If the authors want to use a plural form, they should use "extinction values". Please check the whole document.

Reply: Agreed. We changed "extinctions" to "extinction values" throughout the paper.

L. 30-31, p.6: The expression "The AE is quite scattered" seems improper to me. AE is a physical property.

Reply: Agreed. We changed "AE" to "AE value".

L. 30, p.6: I don't think that the formulation "smaller aerosols" is appropriate. Aerosol is a substance in suspension in the air. The authors should use the term "particle" that

can be associated to the concept of size.

Reply: "aerosols" has been changed to "particles".

L. 9, p.9: missing punctuation. Caption Figure 11: It is confusing that the labels (a) and (b) are after the corresponding part of the caption. The authors should put the labels first.

Reply: Fixed.

L. 29, p.10: The correct expression for "Chappius" is "Chappuis bands". The authors should use the correct one.

Reply: Corrected.

L. 4, p.11: Which time series? Please be specific.

Reply: We have added text "during the period of June 2017$\sim$ May 2019".

L. 6, p.11: I guess there is only one retrieval? And "Impact" seems more appropriate than "impacts".

Reply: Fixed.

L. 9, p.12: duplicated word.

Reply: Done.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-360, 2019.