

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

Anonymous Referee #1

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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

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a vague concept without serious analysis.

This should be clearly improved before a possible publication.

Specific comments

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

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

L. 2-3, p.2: And what about 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.

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 !

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

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

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

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

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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.

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.

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

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 ?

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

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

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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 .

Figure 3: What about the error bars ?

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.

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.

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.

L. 13-14, p.6: This statement and the references to the papers by Pumb and Bell (1982)

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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 remove 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.

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.

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 35°S-55°S 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.

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

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 (...)”.

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.

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.

L. 8-9, p.7: A typical daily latitudinal coverage in a given bin (if it contains any measurement) is about 0.3° for SAGE III/ISS, which is very small compared to the bin latitude resolution of 5° . 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.

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.

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.

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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.

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

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

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 rigorous 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.

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

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

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 ?

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.

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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.

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?

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.

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.

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.

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

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

L. 11, p.10 and Figure 13: The concept of “aerosol weighting function” is never defined in the text and should be appropriately explained.

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 mid-latitudes ?

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.

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

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

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.

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.

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

L. 11, p.12: The concept of “random discrepancy” looks strange.

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.

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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.

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.

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

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

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.

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

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.

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.

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

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

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

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than “impacts”.

L. 9, p.12: duplicated word.

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

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