

Reply to Reviewer 1

Z. Chen et al.

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General comments:

This is an interested and well written study on the improvement of stratospheric aerosol extinction profile retrievals from OMPS/LP limb scatter observations. Retrievals of this kind require a priori knowledge on the aerosol particle size distribution (or prior particle size retrievals/estimations), which is one of the main weaknesses of this retrieval method. The study uses a priori information on the aerosol particle size modeled with the CARMA model in combination with a gamma-function parameterization of the size distribution. In my opinion this study is a relevant contribution to the field and should be published with some modifications and additions.

Authors would like to thank Reviewer 1 for reviewing the manuscript and providing constructive comments. Our responses to the specific comments are given below in regular font.

I start with a few general comments, followed by some more specific and mainly minor comments: I'm not a modeler myself, but hear from colleagues that modeling the size distribution of stratospheric aerosols is a non-trivial task and may be affected by different (partly arbitrary) assumptions. From that perspective, one can be skeptical whether modeled size distributions are in general suitable as a basis for aerosol retrievals from scattered radiance measurements. Perhaps the model simulations were also tuned to reproduce some observational data sets? I don't think this aspect is a big issue for the current paper, though, because the paper convincingly demonstrates that the retrieval results improve with the modeled size distribution. But I suggest adding a brief discussion stating that modeled size distributions may not (or probably will not) always lead to robust retrieval results.

Authors: While we find in this study a general improvement in the quality of the OMPS LP aerosol retrievals by adopting the physically based and self-consistent CARMA-produced particle size distribution, we acknowledge here that the use of this particular model is not intended as a definitive prescription for the OMPS LP algorithms. The model is of course subject to a variety of uncertainties in its own right, in terms of formulation and implementation of its physical algorithms, and generally speaking the modeling of the stratospheric aerosol particle size distribution and composition is non-trivial and a subject of ongoing work by a number of researchers. For example, the version of the model used here does not yet include the possible impacts of volcanic eruptions on the background particle distribution, and neither does it include the impacts of non-sulfate aerosols that may be important in the UTLS (e.g., organics). We recognize that to push the approach taken here further, for example, to use a model-based climatology of aerosol properties to define altitude- and location-specific properties to be used in the OMPS LP algorithms requires at a minimum a more complete implementation of the relevant physics in the model (i.e., addition of missing species) and a thorough and independent evaluation of its capabilities and quality.

We have also added the following text in Section 3 as a cautionary note.

“Determining the particle size distribution based on model results is a challenging task. The assumptions inherent in any complex model can sometimes require arbitrary choices that influence the calculated results. So the size distribution adopted here for LP V1.5 aerosol extinction retrievals may not yield equally good results in all situations.”

Another more general point: The previous version of the OMPS/LP stratospheric aerosol data product was V1.0, the new version is V1.5. Please describe briefly, what was changed for the intermediate versions (1.1, 1.2, ..) that probably exist, too.

Authors: No intermediate data versions exist between V1.0 and V1.5. We have added the following text at the end of Section 2 to help clarify the distinction.

“The primary change introduced for the LP V1.5 aerosol retrieval algorithm is the revised particle size distribution described in Sect. 3. Other changes with less impact on the retrieved extinction values include the use of vector radiative transfer calculations and the implementation of intra-orbit tangent height adjustments as described by Moy et al. (2017). In addition, the V1.0 retrievals only allowed a factor of two change in extinction for each iteration and executed three iterations, rather than the larger values (factor of five change, four iterations) given in Loughman et al. (2018). Based on inspection of test results, we revised those parameters for the V1.5 algorithm to allow a factor of three change in extinction for each iteration and four iterations of the retrieval.”

Specific comments:

Page 2, line 17: “in (Jaross et al., 2014)” -> “in Jaross et al. (2014)” (wrong cite command used)

Authors: Fixed. Thanks.

Page 3, line 9: “described in (Rault and Loughman, 2013)” -> “described in Rault and Loughman (2013)” Similar typo in lines 11, 16 and 28 on page 3.

Authors: Fixed.

Page 3, line 29: “when the optical path along the line of sight (LOS) becomes thick”. Can the “optical path” become “thick”? I suggest replacing “thick” by “optically thick”

Authors: We have replaced “thick” by “optically thick”. Thanks.

Page 4, lines 19 and 20: please use the correct LaTeX cite command (same problem as above)

Authors: Fixed.

Page 8, line 10: wrong citation command used.

Authors: Fixed.

Caption Fig. 1, last sentence: “The V1.0 distribution has the largest $dN/d\log r$ value at $r=0.1 \text{ }\mu\text{m}$ and the smallest value at $r=0.3 \text{ }\mu\text{m}$.” I don’t understand the second part of this sentence. The V1.0 distribution does not have its smallest value at 0.3 micron. You probably mean compared to the other distributions, right? But this is not clearly stated.

Authors: We have revised the caption of Figure 1 to clarify this point.
“Among these size distributions, the V1.0 function has the largest $dN/d\log r$ value at 0.1 μm , but the smallest $dN/d\log r$ value at 0.3 μm .”

Caption Fig. 2: The GD phase function shown is the one corresponding to the GD parameters in Fig. 1, right? I suggest to mention this.

Authors: You are right. We have added a note to the caption in Fig. 1: “The phase functions derived from the V1.0 and GD are shown in Figure 2.”

Caption Fig. 5: “left/right panel” -> “left/right column” ?

Authors: We have replaced “left/right panel” with “left/right column”.

Page 9, line 25: please use correct cite command

Authors: Fixed.

Page 9, line 28: “This behavior as a function of reflectivity is further illustrated in Fig. 6.” I’m not sure, if the behavior described in the previous sentence is really illustrated in fig. 6.? How does the Fig. illustrate that the retrieval is less sensitive to aerosols at lower altitudes? Please explain. It would also be good to provide a brief, qualitative explanation for the albedo dependence of the extinction ratios.

Authors: Thank you for pointing this out. We have revised the text to read as follows:
“The relationship between the large variability in extinction ratio shown in Fig. 5 and variations in LER is further illustrated in Fig. 6. The dependence of the extinction ratio ($\beta_{a,V1.5}/\beta_{a,V1.0}$) on ρ can become non-linear at low reflectivity ($\rho < 0.2$), and the slope of the linear portion of this figure ($\rho > 0.2$) varies with latitude. The non-linear variation in extinction ratio at $\rho < 0.2$ clearly increases in magnitude when moving from 25.5 km to 20.5 km, showing the altitude dependence of the additional contribution from Rayleigh scattering.”

Page 11, line 9: please use correct cite command

Authors: Fixed.

Fig. 10: I’m not sure how to do this better, but in the current Figure a lot of the blue dots appear to be hidden by the green dots, i.e. the V1.5 LP values appear somewhat biased. Perhaps you can test plotting V1.5 on top of V1.0 and check, whether the apparent message of the Figure can be improved?

Authors: We have revised the plotting sequence as suggested for clarity. We have also revised some of the text in Sect. 5 to bring out the key message for this figure more effectively.

Page 12, line 25: I suggest writing “is generally < 10% for 19–29 km”, because there are a few points in this altitude range, where the differences are larger than 10%.

Authors: You are correct. We added “generally” in the sentence.

Fig. 13: The Figure and the caption include a standard deviation. It is not clear – at least to me – which standard deviation this is.

Authors: We clarified that in the text: “standard deviation of differences σ (defined as

$$\sqrt{\sum_{i=1}^N (\beta_{LP,i} - \beta_{SAGE,i})^2 / (N - 2)}$$

Page 14, Table A1: Is the use of “mode radius” intended, or is this rather the median radius as in the main text of the manuscript?

Authors: “Mode radius” has been changed to “Median radius”.

Caption Fig. A1: I suggest defining f_c also in the figure caption.

Authors: Done. Thank you.