Referee report to "Using OMPS-LP color ratio to extract stratospheric aerosol particle size and concentration with application to volcanic eruptions" by Yi Wang al.

In this manuscript, a simple algorithm suggested for the OSIRIS instrument by Bourassa et al. [1] is applied to OMPS-LP measurements to retrieve the median radius of the log-normal particle size distribution and the number density of the stratospheric aerosols. Authors fix the mode width of the particle size distribution to 1.6 claiming this value to be typical for stratospheric aerosols. To justify this claim authors cite papers of Deshler et al. and Bourassa et al. [3, 2]. In the former paper, the only information on the mode width is given in their Fig. 5 for two single measurements. For the fine mode, it reads 1.26 in the left panel and 1.63 in the right panel of the plot. The second cited paper deals with the merging of ozone data and does not contain any information on the aerosol particle size distribution width. Thus, the claim that the width of 1.6 represents a typical value for the stratospheric aerosols remains absolutely unjustified. Although it is widely known that changes in the distribution width affect the extinction coefficient resulting form the Mie code in a very similar way as changes in the median radius, authors make no attempt in a course of their paper to investigate how a different assumed value of the distribution width would affect their results. The presented validation of the retrieval results is absolutely insufficient for the following reasons: (i) the comparison with both LPC and SAGE III data is made for only two days although much more data is available, (ii) a good agreement with both LPC and SAGE III is seen for the median radius of about 0.1 μm while strong deviations are seen for higher values, (iii) no comparison is done for scenes with high volcanic activity, e.g. after Raikoke or Hunga-Tonga eruptions, which are analyzed later in the text. To my opinion the manuscript has to be rejected, as the used scientific method is poorly justified and no reasonable validation of the data has been done. As the quality of data is not properly assessed, it is also unclear if the results illustrating the application of the data to the volcanic eruptions are trustable. I would like to encourage authors to put more efforts in the justification of the validity of their method and validation of the results under different conditions and re-submit the manuscript thereafter.

Major comments

- Line 72: "This size distribution is consistent with in situ stratospheric aerosol measurements (Deshler 2003; Bourassa 2014)." - as mentioned above, this statement is false. The mode width is consistent with one of the two values reported by [3] and is inconsistent with the other. The second reference is inappropriate here, as it has nothing to do with situ stratospheric aerosol measurements.
- Line 74: "In the discussion below, the 'size' is the median radius of the size distribution." - Median radius is absolutely inappropriate quantity for this kind of study because it changes then changing the mode width, even if the extinction coefficients at both wavelengths remain the same.

- Line 74: “Thus, the CR should be nearly independent of the aerosol concentration and only a function of size.” - this is true for values resulting from the Mie code while the extinction coefficients retrieved from limb-scatter measurements are strongly affected by the scattering and their ratio is not independent of the number density any more. Although the authors claim later in the text that the dependence on the scattering phase function is low, no justification of this statement is provided in the manuscript.

- Line 98: “However, AE is the robust quantity retrieved by the L2 V2.1 algorithm since the AE must match the observed radiance.” - again this statement is not true as the radiance is determined not only by the extinction coefficient but also by the scattering phase function. Furthermore, depending on the algorithm, the contribution of the surface reflectance and/or aerosol amount at the reference tangent height might be relevant.

- Line 99: “Thus, if we use the L2 AE at two wavelengths, we have enough information to independently compute a size and number density consistent with the two AE values and independent of the radiative transfer model assumptions about the L2 size distribution.” - this statement is just wrong. First, authors make an assumption about the mode width. Second, the extinction coefficients at different wavelengths resulting from OMPS-LP L2 retrieval will be different for different model assumptions affecting thus the resulting sizes.

- Line 101: “This approach was also used by Bourassa et al (2008b).” - at this point authors should remark that an OSIRIS data set based on this retrieval was never provided by the University of Saskatchewan for a public use. Maybe the coauthors from the University of Saskatchewan might shortly comment why.

- Line 104: “ Rieger et al., (2018) compared retrievals between volcanically quiescent periods (small aerosols only) and post eruptions periods characterized by bimodal particle distributions. They did find a retrieval dependence on scattering angles; however the scattering angle error was minor when averaged over similar range of scattering angles.” - the reference Rieger et al., (2018) is not present in the reference list. It is unclear what authors mean as “the scattering angle error”. The dependence of the retrieved aerosol extrinction coefficients on the scattering angle is related to the phase function but this dependence is not relevant in the context of this paper. For the scattering angle dependence the shape of the phase function is relevant while its wavelength dependence is crucial then analyzing the color ratios.

- Line 108: “ Since we will compare the retrievals within the same latitudes during the same times, we believe that the biases caused by size-driven Mie phase function error will be small.” - This conclusion is made w.r.t. the dependency on the shape of the phase function, which is not relevant for this manuscript. It is wrong, however, when applied to the wavelength dependence of the phase function, which is on the contrary highly relevant for this manuscript.

- Sect. 3.1: Why only 2 comparisons are presented? There are certainly more collocations within 10 years operation time of OMPS-LP. Why no comparison is presented for periods of volcanic activity?

- Line 127: " Figs. 3 and 4 show two characteristic profiles." - as the results for median radii strongly depend on the assumed mode width of the distribution, an additional comparison for another mode width, e.g. 1.26, must be presented.

- Sect. 3.2: Comparison for just one day cannot be accepted as a validation. There are much more data available. Comparisons for periods of volcanic activity, e.g. after Raikoke and Hunga-Tonga eruptions must be presented. Same as for Sect. 3.1, an additional comparison for another mode width needs to be provided.

- Figs. 3 – 5: I do not agree with the overall rating of the agreement. In my opinion, a good agreement is seen only if the median radius from SAGE III is around 0.1 while the agreement rapidly degrades if the radius from SAGE III gets larger. Comparisons for scenes with larger particles need to be provided.

- Line 173: " The agreement validates our assertion that errors due to Mie phase function variation with size are minor and that the extinction estimates from the OMPS-LP L2 algorithm are robust." - This conclusion seems unjustified to me. As mentioned above, this statement refers to the dependency on the shape of the phase function rather than to its wavelength dependence, although only the latter is relevant for this manuscript. Furthermore, the agreement might be good if the phase function assumed in the retrieval is in a good agreement with the real one and might be worse otherwise. A scenario with larger particles must be considered.

- Conclusions: no word is said about the use of the fixed mode width of the aerosol particle size distribution.

Minor comments

- Introduction: Authors do not seem to know anything about European instruments measured aerosol characteristics, e.g. GOMOS, SCIAMACHY.

- Line 62: When talking about the NASA L2 OMPS-LP product, it would be worthwhile to mention that this product uses the Gamma distribution rather than the log-normal one to represent the particle size distribution of the stratospheric aerosols.

- Line 71: "SASKTRAN assumes a log-normal aerosol size distribution with a mode width of 1.6 for spherical sulphate aerosols" - this is a bit misleading statement. You assume the mode width of 1.6 not SASKTRAN.

- Line 152: “SAGE will thus report a lower concentration compared to OMPS” - this is not necessary true, errors in the retrieved extinction coefficients might also result in wrong median radii. Figs. 3 – 5 provide an impression that a high bias in the median radius is associated with a low bias in the number density and vice versa.

- Fig. 9: Authors should discuss that the figure shows a completely unrealistic behavior of the retrieval below 20 km. The particles are getting smaller and smaller reaching undetectable sizes in panel (d). The pronounced anti-correlation of the median radius and the number density is in accordance with Figs 3 – 5, 7, 8 a clear indication of retrieval issues.

- Line 267: “The robust quantity retrieved is, however, the extinction since it must be consistent with the observed radiance.” - this is not completely true for limb-scatter measurements as the retrieved extinction coefficients depend on the assumptions on the aerosol particle size distribution and, depending on the retrieval approach, surface reflectance and aerosol amount at the reference tangent height.

- Line 271: “Using the SAKATRAN radiative transfer model, we show that the color ratio is independent of the number density and only a function of the particle size for a log-normal function size distribution with a fixed width.” - this statement is misleading, as only the Mie code from SASKTRAN rather than the full radiative transfer modeling was used. A pure usage of the Mie code cannot show anything as this independence results per definition from the used formulas. To show an independence, full radiative transfer modeling followed by the synthetic retrievals needs to be done, which was not the case in the framework of this study.

- Line 284: “errors in the Mie/Rayleigh scattering angle” - the statement makes no sense. Most probably you are taking about the shape of the phase function, this is, however, irrelevant in the framework of this study.

Technical corrections

- Line 15 and throughout the text: “Reikoke” - should be “Raikoke”

References

Bourassa, A. E., Degenstein, D. A., and Llewellyn, E. J.: Retrieval of stratospheric aerosol size information from OSIRIS limb scattered sunlight spectra, Atmos. Chem. Phys., 8, 6375-6380, https://doi.org/10.5194/acp-8-6375-2008, 2008.

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Deshler, T., Hervig, M.E., Hofmann, D.J., Rosen, J.M. and Liley, J.B.: Thirty years of in situ stratospheric aerosol size distribution measurements from Laramie, Wyoming (41 N), using balloon-borne instruments. J. Geophys. Res. Atmos., 108(D5), 2003