Below please find our specific responses to the reviewer RC1. Overall, the reviewer comments were quite useful and pointed to issues that need to be clarified in our analysis. Below we respond to general and specific comments. We also include a few comments from CC2 where relevant.

We would like to submit a revised paper with the additional analysis as shown below.

The format is the reviewer comments in italics followed by our reply.

(RC1 general comments part 1):

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.

The reviewer may find it beneficial to consider Rieger et al. (2018), where it is demonstrated that a width variation of 1.6 (or slightly smaller) can be considered reasonable for a background simulation. Of course, we might expect wider distributions for recent volcanic eruptions as discussed below although Bernath et al. (2023) used a narrow distribution in their analysis.

Although it is widely known that changes in the distribution width affect the extinction coefficient resulting from 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.

A similar comment was made by CC2 in the main text and in CC2 comment #7 shown below.

CC2: The methodology itself is fundamentally flawed and the derived products are wholly unreliable. I present a simple model below to demonstrate this unreliability. The authors assumed that the information content of 1 extinction ratio is sufficient to derive a valid estimate of particle size, but this only holds true if the distribution width is fixed and the measurement error is sufficiently small; both are invalid assumptions. While an assumed distribution width of 1.6 is a good estimate, fixing the width to that value (or any other value) imposes an artificial constraint on the solution space and inevitably biases the inferred radii and number density results. Ultimately, we have to recognize that we know very little about the atmosphere (the width could be 1.2, or it could be 1.9; both are very realistic) and forcing the distribution width to 1 specific value is wrong.

Below is our response to both RC1 and CC2 comments.

We concur with your assertion that the distribution width may vary, but numerous in situ measurements have constrained the range of widths and models of the size distribution for ambient condition (Rieger et al., 2018). However, we disagree that the methodology is fundamentally flawed. In the context of this retrieval method, assuming a fixed distribution width is a necessary step and a common approach used in current retrieval algorithms, and, as the CC2 reviewer notes, 1.6 is a good estimate for the distribution width (in disagreement with R1).

The basic concern is that we have not assessed the error in assuming a fixed distribution width. We have performed a series of experiments using Mie code inside SASKTRAN to determine the sensitivity of the size to both color ratio and assumed distribution width.

Figure 1 shows how the size varies with color ratio and width of the distribution derived from the SASKTRAN model. Given a color ratio (CR) of 3 the size varies from 0.05 to 0.3µm over a distribution width from 1.1 to 1.8. Given a measured extinction, this size range will produce a large change in the estimated aerosol concentration. However, Rieger et al. (2018) Fig 6 shows that not all distribution widths are likely. Can we constrain the distribution width further, or estimate the propagation of uncertainty in the distribution width and the uncertainty in the color ratio into an uncertainty in size?

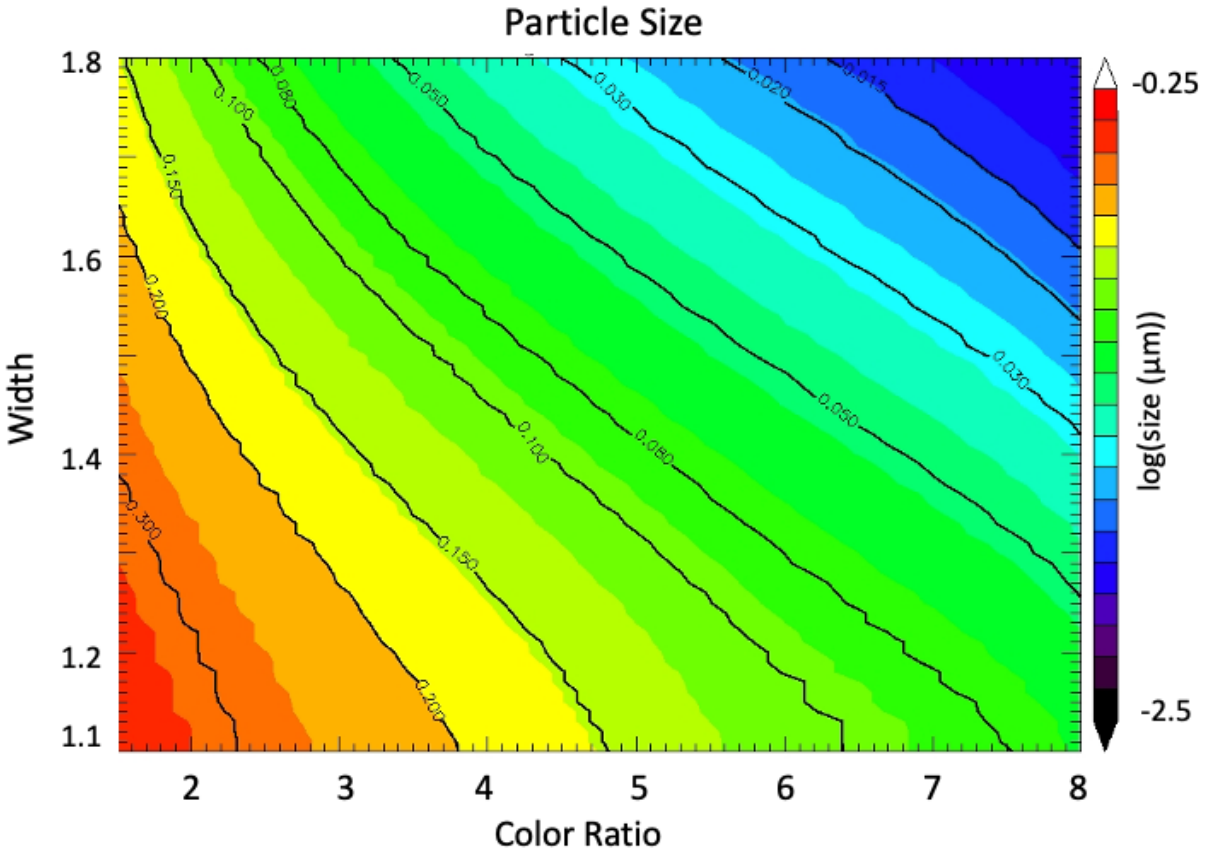

Figure 1: The size as a function of color ratio (510 nm/869 nm) and particle distribution width. Color contours are log10 of size. Black contours are size in nm.

To do this, we define the size uncertainty as the standard deviation of particle sizes within the uncertainty domain of both the color ratio (CR) and the distribution width (W). The uncertainty in CR can be estimated from Taha et al. (2021, Fig. 4). For example, the radiance uncertainty at 20 km for 869 nm is about 5% (u_{869}) and the uncertainty at 510 nm is about 20% (u_{510}) - both at the equator. The uncertainty in the color ratio is then CRu = sqrt (u_{510}^2 +u_{869}^2) or 21%. For the uncertainty in distribution width for small particles, we use Fig. 6 from Rieger et al. (2018) which gives a width uncertainty (Wu) of ~0.2. To estimate the size uncertainty, we find the standard deviation for all the points within the domain W±0.2 and CR ±21% for the color ratio and the width – see ellipse in the figure. To get the normalized uncertainty we divide by the mean particle size within the domain. We now repeat this calculation for each CR and W value in Fig. 1. Figure 2 shows the normalized size uncertainty with contours of size overlaid.

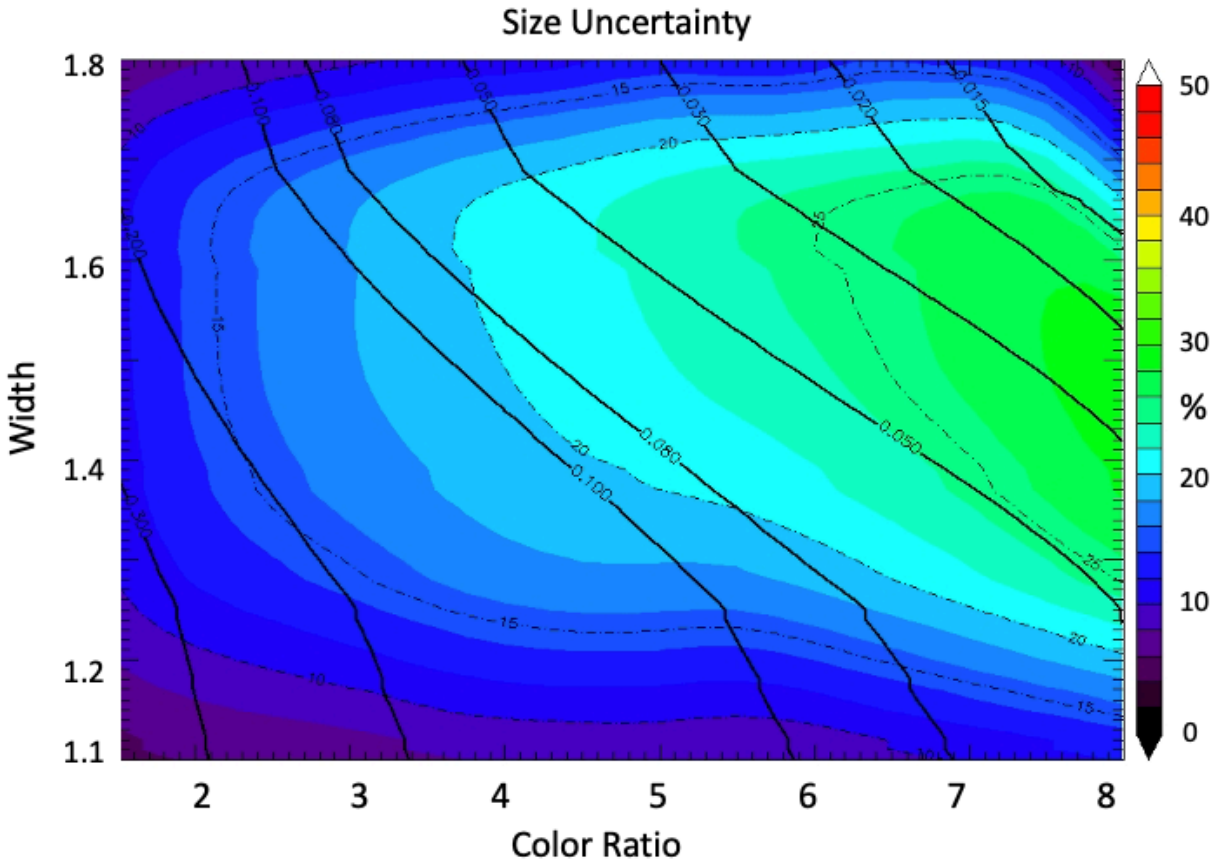

Figure 2. Size uncertainty in % as a function of distribution width and color ratio. The color contours and dashed contours are the normalized size error in % for a distribution width uncertainty ±0.2 and color ratio uncertainty of 21%. The black contours are size in nm.

We can use the color ratio uncertainty for ranges from 18 to 28 km (radiance uncertainty up to 10%-50% for 510 nm and 5%-20% for 869 nm) and the log-normal distribution width uncertainty from 0.2 to 0.4. Then for a a width value of 1.6, and averaging color ratios are between 2 and 4, our average size uncertainty is ~20%. To summarize, Figs. 1 and 2 quantify

the expected impact of CR uncertainty and distribution width uncertainty on size using values from Taha et al., (2021) and Rieger et al (2018). The size uncertainty of 20% leads to a number density uncertainty of 44% assuming that the cross section changes as the square of the radius. This example shows how the size and number density uncertainty due to the distribution width can be quantified.

We agree that the size distribution is not static and volcanic or PyroCB distributions may not resemble distributions observed under ambient conditions, and our algorithm will have higher uncertainty under those conditions. Note the Rieger et al. (2018) also provides size distribution widths for coarse mode particles and the fine mode and coarse mode distribution widths are similar as are the mean distributions (1.6). Under volcanic conditions, our Fig. 6 shows that some of the aerosol plumes are characterized by lower CRs (between 1 and 2) but most of the aerosols still have CR values between 2 and 4.

The calculation above will be included in the revised manuscript which should address RC1 and CC2 concerns. This directly responds to RC1 comment : .. *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.*

(RC1 general comments part 2):
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.

We feel that the reviewer's statement that the validation is "absolutely insufficient" and the recommendation to reject the paper is overly harsh. The OMPS LP aerosol data has already been validated (Taha et al., 2021), and our PSD estimation is consistent with corelative measurements and other studies. Exploiting OMPS-LP multiwavelength measurements to derive PSD is a much-needed work given that there is no planned solar osculation mission beyond SAGE III/ISS or ACE-FTS, whereas OMPS-LP is scheduled to fly on the JPSS-2, JPSS-3, and JPSS-4 satellites, to extend the stratospheric aerosol measurements into the next couple of decades.

(i) & (ii) As noted in our paper, SAGE III/ISS coincidences with OMPS-LP are infrequent because of the different instrument observing requirements. Nonetheless, we don't feel the need to repeat the more extensive aerosol extinction comparisons between OMPS-LP and SAGE III

performed by Taha et al. (2021). Since we are applying the same algorithm to SAGE III extinctions as we are applying to OMPS-LP, and since Taha et al. (2021) shows that the aerosol extinctions between the two instruments generally agree, it is no surprise that the sizes agree. This point, also noted by CC2, will be emphasized in the text, there is really no need to do further comparisons.

(iii) The date of Figure 4 is during the Raikoke eruption, which shows the comparisons between LPC and OMPS-LP, and LPC is only available at the mid-latitude. Figure A1 in supplementary shows that OMPS and SAGE III/ISS particle size during the Raikoke eruption agrees very well. Retrieval of Hunga-Tonga PSD from SAGE III measurements has already been published (Taha et al., 2022; Khaykin et al., 2022) among others, and repeating that work is beyond the scope of the paper.

The application of our methodology was to ambient conditions as well as volcanic eruptions. The reviewer misses the point that the technique produces consistent size and aerosol concentration between wavelengths. The existing OMPS-LP algorithm produces inconsistent aerosol concentration and size since the size is fixed. The issue of distribution width dependence is a fair point (noted above) that we will address in manuscript revision. While our suggested approach for estimating the size may have issues shortly after the eruption, we feel that the uncertainty in the size distribution is not large enough to invalidate our calculation later in the plume lifetime (after the ash has settled and SO_2-> sulfate aerosol conversion is complete. We will add material on this point in the revision.

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.

The discussion above and below provides "more effort." We will include this material in the revised version.

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.

We added Rieger et al. (2018). We note in the paper that Deshler reports their data as bimodal distribution. Our method can only extract the size from a single log-normal distribution. Thus, we will tend to generate a size in between the dominant distribution. Fig. 4 illustrates the issue, but it also shows that our size estimate generally agrees with Deshler when one mode dominates. Bourassa (2014) should have been Bourassa (2008). This citation error also noted by CC2.

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.

The median radius and the width describe the log normal distribution. Dependency on width is addressed above.

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 anymore. 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.

This is a fair point. Adjusting the size distribution will affect the cross section and the Mie phase function. To investigate the OMPS LP aerosol color ratio sensitivity to the assumed phase function, we perturb the phase function parameters by ± 10% and run the OMPS LP retrieval algorithm at a range of scattering angles observed during a single orbit, similar to Chen et al. (2018). OMPS LP V2.0 aerosol retrieval algorithm assumes a gamma aerosol size distribution using fitted parameters of α =1.8 and β =20.5 where $r_{eff}=(\alpha+2)/\beta = 0.185\mu m$, where r_{eff} is the effective radius (Chen et al., 2018), which is used to calculate the aerosol phase function (see Figure 3). For a width W=1.6, $r_m=r_e \exp(-2.5*\text{alog}(W)^2) =0.1\mu m$ where r_m is the modal radius. Fig. 4a shows that the phase function is more sensitive to β changes than α. A 10 % change of β can produce ± 10 and 15% change in 510 nm and 869 nm phase functions, respectively, while a 10 % change of α results in a ± 3 and 5% change for both wavelengths. Chen et al. (2018) noted that increasing α increases the peak of the differential size distribution while increasing β shifts the peak distribution to a larger particle radius. Figure 4b shows that the phase function perturbations produce anti-correlated but lesser changes in aerosol extinction. The changes caused by β perturbations are mostly within 5% for both wavelengths and 3% for α. The structural change in the extinction coefficient along the orbit is caused by scene reflectivity (Chen et al., 2018). The effect on extinction ratio (510/869) is shown in Figure 1c for the same variations of phase functions. The color ratio perturbations are mostly within 3% between scattering angles of 65-125° and 5% outside that range, except for the very small scattering angles, which might be caused by scene reflectivity changes. Let's pick a typical color ratio of 3 with W = 1.6 to calculate the uncertainty of retrieved particle size. The size range would be 0.096 ~ 0.103 μm for 3% change in color ratio perturbations and 0.094 ~ 0.106 μm for 5% change in color ratio perturbations. It means the 3% and 5% change in color ratio perturbations lead to < 4% and < 7% difference in retrieved aerosol particle size, respectively.
The analysis above and figures will be included in the revised text.

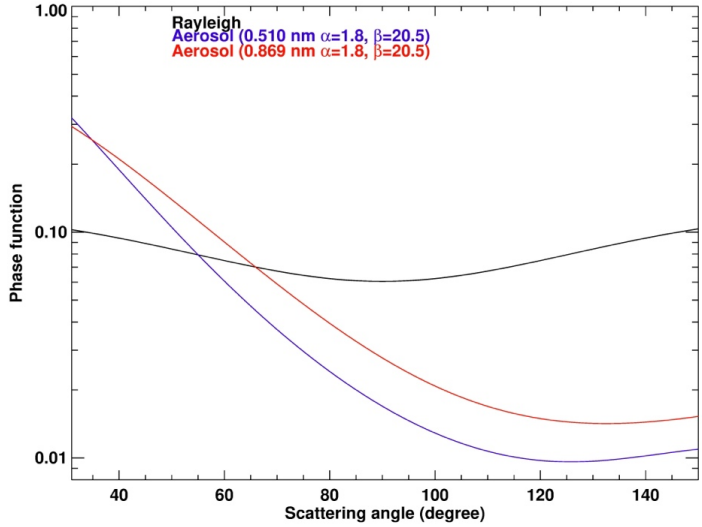

Figure 3: Plot of the aerosol phase function used for OMPS LP retrieval algorithm, which assumes gamma size distribution at 510 (blue) and 869 nm (red). Rayleigh phase function is also shown (black).

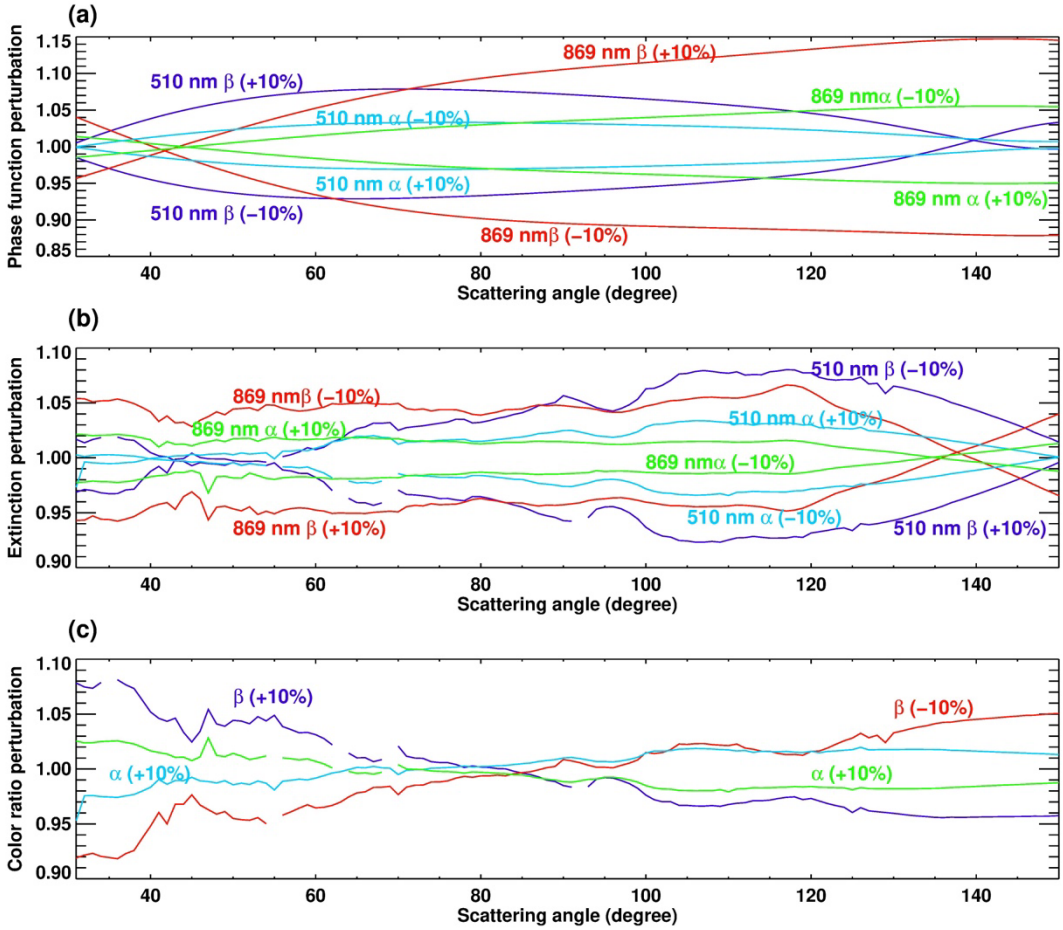

Figure 4: plot of simulated phase function (a), aerosol extinction (b), and color ratio (c) perturbations as caused by gamma parameter changes of ± 10%. Perturbations are shown relative

to OMPS LP operational retrieval at various scattering angles. Scattering angles represent the range of are in the northern hemisphere, while large scattering angles are in the southern hemisphere (Taha et al., 2021). The aerosol extinction was retrieved using a single orbit on 12 September 2016 at 20.5 km altitude.

"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."

To address this comment, we need to discuss the OMPS-LP aerosol algorithm in a little more detail (see Taha et al., 2021; Chen et al., 2018; Loughman et al., 2018). We define the aerosol scattering index Y and the normalized tangent height radiance ANR. By normalized, we mean that ANR=R(z)/R(38.5km) where R is the actual radiance. We define

$Y_m=(ANR_m-ANR_{c0})/ANR_m$

$Y_c=(ANR_c-ANR_{c0})/ANRc$

The c subscript indicates that the radiance is computed by an RTM and the m subscript is for the measurements. ANR_{c0} is the ANR with no aerosols, computed using the RTM. $ANR_c = ANR_{co}= ANR_m$ at the normalization altitude 38.5 km.

To compute the extinction, E, the Chahine scheme is used, i is the iteration number.

$E^i = E^{i-1} (Y^{i-1}{}_m/Y^{i-1}{}_c)$

Starting with aerosol climatology E^0 we compute E^1, then E^1 is fed back into the RTM to recompute $Y^1{}_c$ and since ANR_{c0} doesn't change we only need to update $ANR^1{}_c$ with the new extinction. The iteration continues until $(Y^{i-1}{}_m/Y^{i-1}{}_c)$ -> 1 and you have the extinction.

The effective scene reflectivity is calculated by matching the measured and calculated radiances at 40.5 km. The normalization at 38.5 km also reduces the uncertainty of retrieved surface reflectivity and the impact of aerosol scattering at other heights. The tangent heigh normalization is a compromise between minimizing the effect of aerosol extinction at the normalization altitude and the effect of residual straylight. This point is addressed in the three OMPS papers noted above. The algorithm is also applied separately to each wavelength and different scattering angles along the orbit.

Because the algorithm directly computes the extinction rather than the components, we refer to the extinction as the robust quantity. Technically the number density can be derived from E (l) since the cross section is fixed for each l and we know the scattering angle, but the number density will be different at each wavelength because the size (distribution) is fixed and cannot be

adjusted. Thus the algorithm would generate different number densities for each wavelength. We modified the text to clarify this point.

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.

CC2 also makes this point in comment 5. We do make an assumption about the mode width; this issue and sensitivity to mode width and size is discussed above. The issue will be discussed at length in the revised manuscript since both CC2 and RC1 have raised this point. The second point is also true, if the RTM uses a different size distribution (for example) then dANR/dE will be different and E will be different. However, we can assess the error in our calculation as we have shown above.

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.

This issue is outside the scope of this paper. The reviewer should contact the University of Saskatchewan group for more information.

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 extinction 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.

The reference to Rieger is below, and we apologize for not including it in the reference list. The rest of this comment was addressed above. As noted above for small particles (~0.1μm) the Mie scattering phase function does not vary much with particle size for a fixed scattering angle as noted above.

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.

We discussed the wavelength dependence of the phase function above. This shift in the Mie phase will produce a small error in the estimate aerosol concentration compared to the larger change in the cross section. However, the reviewer's comments will be addressed specifically in the revised 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?

There are very few comparisons measurements of OMPS-LP near the balloon launches. The launches aren't very frequent, and we select the latest two available balloon measurements to compare. These two comparisons illustrate the points we wanted to make with the intercomparison.

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.

We recalculate the radius and density using 1.26 and add to a supplementary figure as suggested. The figures are as follows. Comparing to 1.26, the selection of 1.6 as more adequate since it will agree better with the balloon.

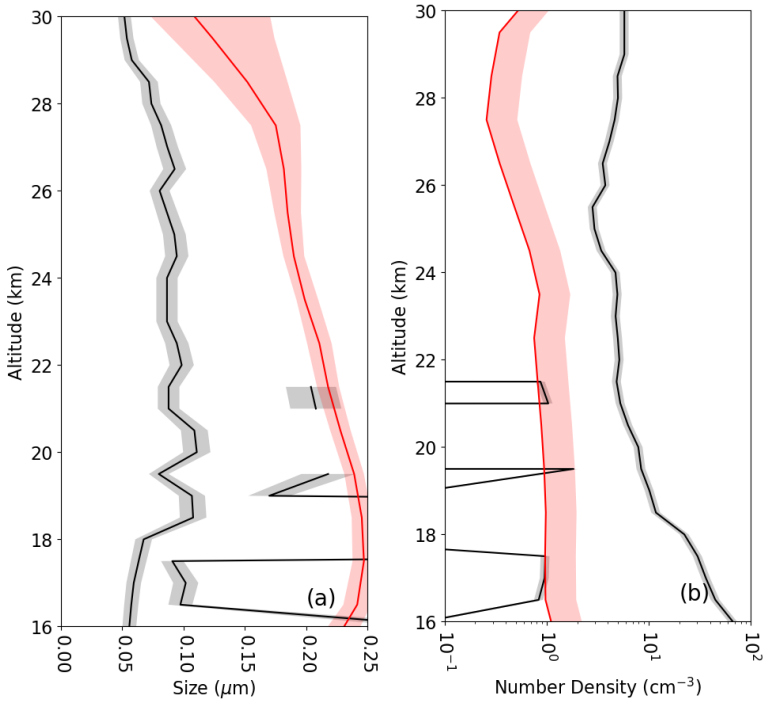

Figure 5: The same as Figure 3 in the manuscript, but the aerosol model assumes a log-normal aerosol size distribution with a mode width of 1.26 instead of 1.6.

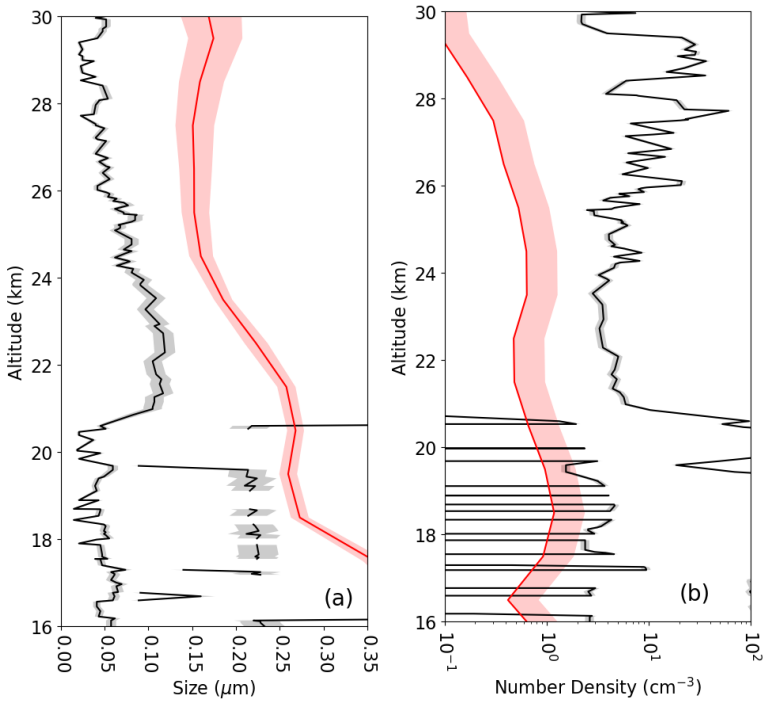

Figure 6: The same as Figure 4 in the manuscript, but the aerosol model assumes a log-normal aerosol size distribution with a mode width of 1.26 instead of 1.6.

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.

Taha et al. (2021) provides extensive OMPS-LP and SAGE extinctions comparisons. Here we apply our algorithm to the SAGE color ratio and express the results in terms of size and number density to compare with OMPS. However, the good agreement between SAGE and OMPS here is a result of the good agreement in the extinctions and discussed in Taha et al. (2021). Extensive comparisons between the two instruments are not required as they are available in Taha et al. (2021)

We have shown in Figure A1 in supplementary that OMPS and SAGE III/ISS particle size during the Raikoke eruption agrees very well. Sect 3.1 and 3.2, as well as the supplementary figure, provide adequate validation of the technique. We also note that several papers already reported on the Raikoke and Hunga-Tonga particle size that agrees well with our estimates, and none provided any validations of their PSD retrieval (Wells et al., 2022; Schoeberl et al., 2022; Legras et al., 2022; Taha et al., 2022; and Khaykin et al., 2022). Our PSD estimates for Hunga Tonga is also consistent with model predictions that the large amount of water from this eruption led to the fast conversion of SO2 into sulfate aerosol and increased the rate of aerosol growth (LeGrande et al., 2016; Zhu et al., 2022).

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.

This is a fair point. We noted that the agreement is not as good at lower and higher altitudes consistent with Taha's findings and possible explanation for the discrepancy. For example, at lower altitudes, the size distribution tends to be more bimodal and at higher altitude the SAGE and OMPS-LP S/N is lower. We also note the case mentioned by the reviewer, where SAGE shows a larger particle size than OMPS (Figure 5c) is in the tropics, and the significant difference is below 18 km is near the tropopause, which is mainly influenced by cloud contamination. Again, a comparison with larger particles is already shown in Figure A1. Also, the "A good agreement" rating is relative. For example, Malinina et al., 2018 reported a 20-30% particle size agreement between SCIAMACHY and SAGE II during background conditions, which is significantly larger than the differences reported in this study.

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.

The phase function and size distribution errors are discussed above. The largest particle we can retrieve is about 0.4 μm. Compared to a 0.1μm aerosol, the phase function difference with a 0.2μm aerosol is about 20% in the forward scatter, but the cross-section change is 400%. So Mie scattering phase shifts errors are small in comparison to shifts in the cross section that we compute.

Minor Comments

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

Thanks for your suggestions, we added relevant contents into the introduction, such as Malinina et al, (2018) and Vanhellemont et al., (2016). Note that Rieger uses SCIAMACHY data.

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.

True, although the early version of the OMPS-LP algorithm used log-normal. Log-normal is built into SASKATRAN and Deshler uses log-normal. We will include this in the revised paper.

Line 152: "SAGE will thus report a lower concentration compared to OMPS" - this

is not necessarily 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.

Good point. We will adjust the text.

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.

Yes, below 20 km the retrievals aren't realistic. Some of the problem is due to cirrus in the upper troposphere and possibly very large aerosol particles in lower stratosphere. The color ratio approaches 1 in this region and we cannot determine the particle size. We should have cut off the graph at the higher altitudes. That said, the interesting part of the figure is the evolution of the aerosol layer above 20 km.

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.

A review of the basic algorithm is presented above. The retrieved reflectance and radiance normalization at 38.5 km mostly removes the surface reflectance problems, and this altitude is high enough to produce near zero aerosol scatter (see Loughman et al., 2018). The way the algorithm operates, the active elements are the extinction and the observed radiance. The radiative transfer model is only run for the first guess, the number concentration is never recomputed (although it could be). Hence our statement 'the extinction is the robust quantity'. The reviewer is referred to the algorithm paper by Taha et al. (2021) and earlier papers referenced therein and mentioned above.

"Using the SASKATRAN 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.

Thanks for pointing it. We corrected the statement to "using the Mie code inside the SASKATRAN radiative transfer model".

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.

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