We would like to thank the reviewer for their comments on the paper. Below is our response.

1. The general methodology is not new. A very similar method was published in 1983 by Glenn Yue (DOI: 10.1364/AO.22.001639) and another method (using 2 ratios) was published in 2021 by Felix Wrana (DOI: 10.5194/amt-14-2345-2021)). Both of these papers (and numerous others in between) deal with extinction ratios from SAGE instruments, but the gist is the same.

R: Thank you for bringing these additional papers, in addition to those we cited, to our attention. We apologize for the oversight and will add them to the manuscript. While we acknowledge that the general methodology of using color ratio to retrieve particle size is not new, we believe that applying this method to OMPS-LP and focusing on recent volcanic eruptions represents a valuable contribution to the field.

2. 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.

R: 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 reviewer notes, 1.6 is a good estimate for the distribution width. In response to the longer comment by the reviewer below we have quantified the impact of the distribution width assumptions as described in response to comment 7.

3. A comment on the references: The authors cited many manuscripts that do not correspond to the text they supposedly support. For example, on lines 54–56 the authors state that their method of determining particle size is based on 4 previous publications and all of the cited papers deal with cloud identification and filtering, not determination of size. Further, the Bourassa et al. 2007 paper does not seem to fit at all. The same is true for the Bourassa 2014 paper cited on line 73. Bourassa 2014 has to deal with stratospheric ozone trends. Perhaps the authors intended to cite Bourassa 2008 instead, but even that paper does not support their text (Bourassa 2008 cites Deshler et al. 2003, but the context within which Bourassa 2008 is cited here indicates that they actually did in situ measurements, which they did not).

R: Thanks for pointing this out. We will correct Bourassa et al. (2007) to Bourassa et al. (2008b) and fix other citations.

4. Line 10: The authors claim that they demonstrate that extinction ratio is insensitive to aerosol concentration. This is nothing new and can be observed by looking at the corresponding equations.

R: We do not intend to claim this is a new finding. We will rephrase the sentence to make it clearer.

5. Line 51: As stated in this paper the OMPS-LP retrieval assumes a size distribution to obtain the extinction products. The authors then used the extinction products to infer a size distribution, which makes a cyclical process. What if the assumed size distribution used in the OMPS-LP processing was different, would that change the derived size? What is the level of correlation between the assumed size distribution and the derived particle size?

R: See information above. The OMPS-LP retrieval assumes a size distribution to compute an extinction consistent with the observed radiance. To achieve this result, the algorithm varies the concentration. Thus, the aerosol concentration varies with wavelength, which is unphysical. The fundamental retrieved quantity is the extinction. Using the two wavelength extinction ratio (color ratio), we recompute a consistent size and concentration, the only assumption is the distribution width.

6. Line 60 (all of section 2): It is unclear whether the authors accounted for the uncertainty in the OMPS-LP products. Given the content of some of their figures I assume they did, but it is never explicitly stated (see comment below regarding error propagation).

R: We will add an uncertainty range of retrieval for different distribution width values instead of just giving an error propagation.

7. Line 72: It is unclear why the authors assumed a distribution width of 1.6. Granted, this value makes for a reasonable first guess, it used in SASKTRAN, and was used by Bourassa et al 2008 (the authors cited Bourassa 2014). However, Deshler et al. 2003 in no way claims that 1.6 is the only value that should be used. The Deshler et al. 2003 paper presents a figure (Fig. 5 panel B) that contains a derived size distribution from 1 altitude (20 km) of 1 profile; this in no way supports the use of a static distribution width of 1.6. This is a key point.
(a) The authors took this value of 1.6 (collected during the "background period" at 20 km), failed to account for the natural variability of this value and made the assumption that it never changes. This is particularly a problem when the authors use the same distribution width after major eruptions.
(b) The distribution width in the atmosphere is not static. It changes with season, altitude, latitude. The width is highly variable even when the atmosphere is not substantially impacted by volcanic and/or pyroCb activity (see Fig. 1). While the assumed width of 1.6 may be reasonable, it cannot be assumed to be static.

(c) The University of Wyoming dataset reports an uncertainty in distribution width of ±20%. Therefore, even if Deshler et al. 2003 said that the width is consistently 1.6 that would still leave a range between 1.56 and 1.63. If we make some assumptions, we can model the expected behavior: assume sigma error is fixed at 20% (per Deshler et al. 2003) and the measurement error (propagated uncertainty in the extinction ratio) is only 5% (this is conservative as Taha et al. 2021 report accuracy/precision on the order of ±20%). Here (Fig. 2) we see the range of mode radii that produce extinction ratios that fall within these uncertainty bounds (everything from ≈40 nm through 190 nm). The question then is "Which mode radius is the 'real' one, or which do you pick?" Each radius is a viable solution so the uncertainty in the authors' estimate is far larger than they show. If the authors were to use a realistic uncertainty in their estimate of distribution width (e.g., along the lines of the atmospheric variability shown in Fig. 1) and were to account for the propagated measurement uncertainty then they would see the solution space expand quite rapidly. This point cannot be overstated: the distribution width is highly important and is far from static, fixing the width to 1.6 (or any other value) imposes an unjustified constraint on the solution space and introduces bias in the inferred radius estimates as well as the corresponding number density estimate. It is for this reason that I see the method as fundamentally flawed.
The model I present in Fig. 2 is overly conservative and presents a best-case scenario. The point I'm getting to is: Even under these best-case scenarios we cannot make a definitive statement about the particle size. The requisite information content is not there.

R: All remote sensing systems make assumptions about size distributions as noted above. The actual question is: how sensitive are our results to assumptions about the size distribution in the retrieval. To address this issue, we have evaluated the impact on the retrieved size by varying distribution widths along with the color ratio. Then using the estimates of the uncertainty in color ratio and the uncertainty in distribution width, we can estimate the uncertainty in size. The results are shown in the figures below, and these figures will be added to the revised manuscript.

Figure 1 shows how the size varies with color ratio and width of the distribution derived from the Saskatran 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, and 1.6 is a reasonable choice. 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?

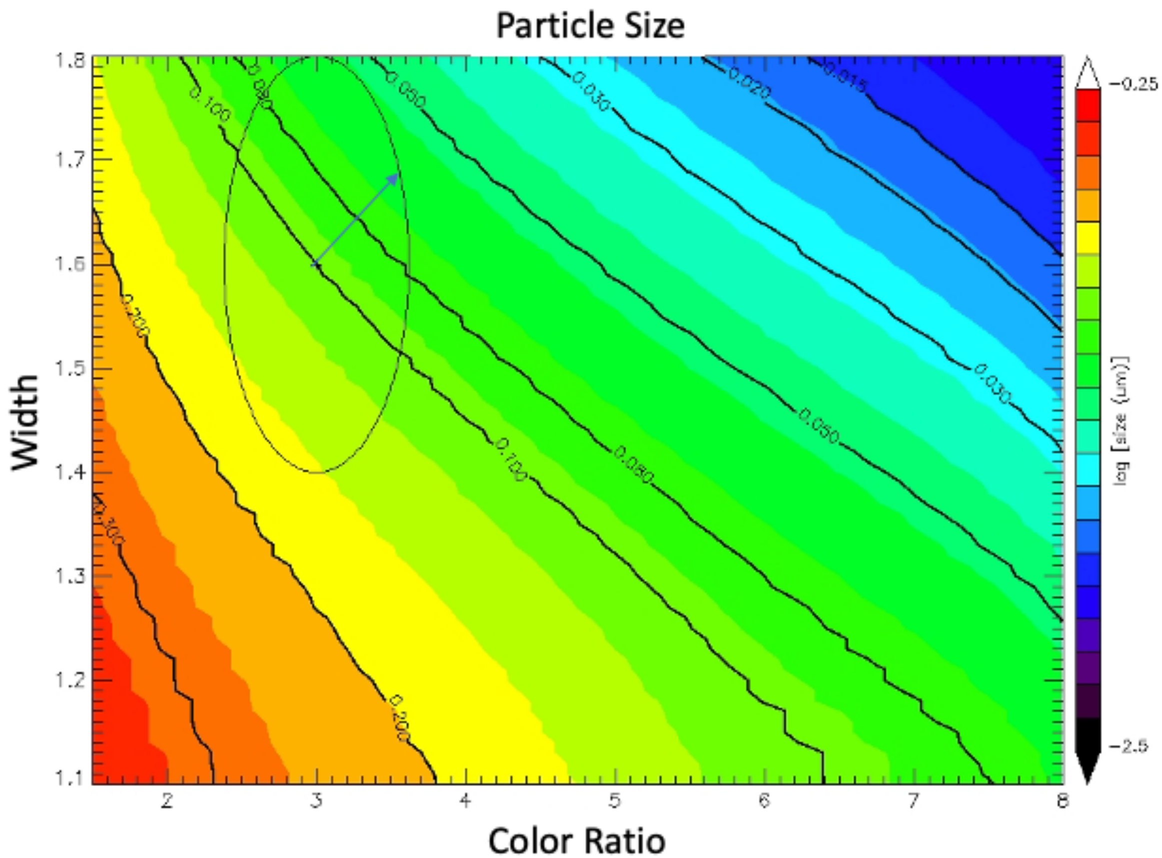

Figure 1: The size as a function of color ratio (510 nm/869 nm) and assumed particle distribution width. Color contours are $\log_{10}$ of size, black contours are size in nm. Ellipse shows an example of the domain for an uncertainty calculation for a width of 1.6 and color ratio of 3. The uncertainty is the standard deviation of the sizes within the domain.

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 879 nm is about 5% and the uncertainty at 510 nm is about 20% (both at the equator). The uncertainty in the color ratio is then CRu = sqrt ($u_{510}^2 + u_{879}^2$) 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. 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 with size overlaid.

Now we can vary the color ratio uncertainty for ranges from 18 to 28 km (radiance uncertainty up to 10%-50% for 510nm and 5%-20% for 879nm) and the distribution width uncertainty from 0.2 to 0.4. For a size value of 1.6, and averaging color ratios between 2 and 4, we find that our size uncertainty is 20%.

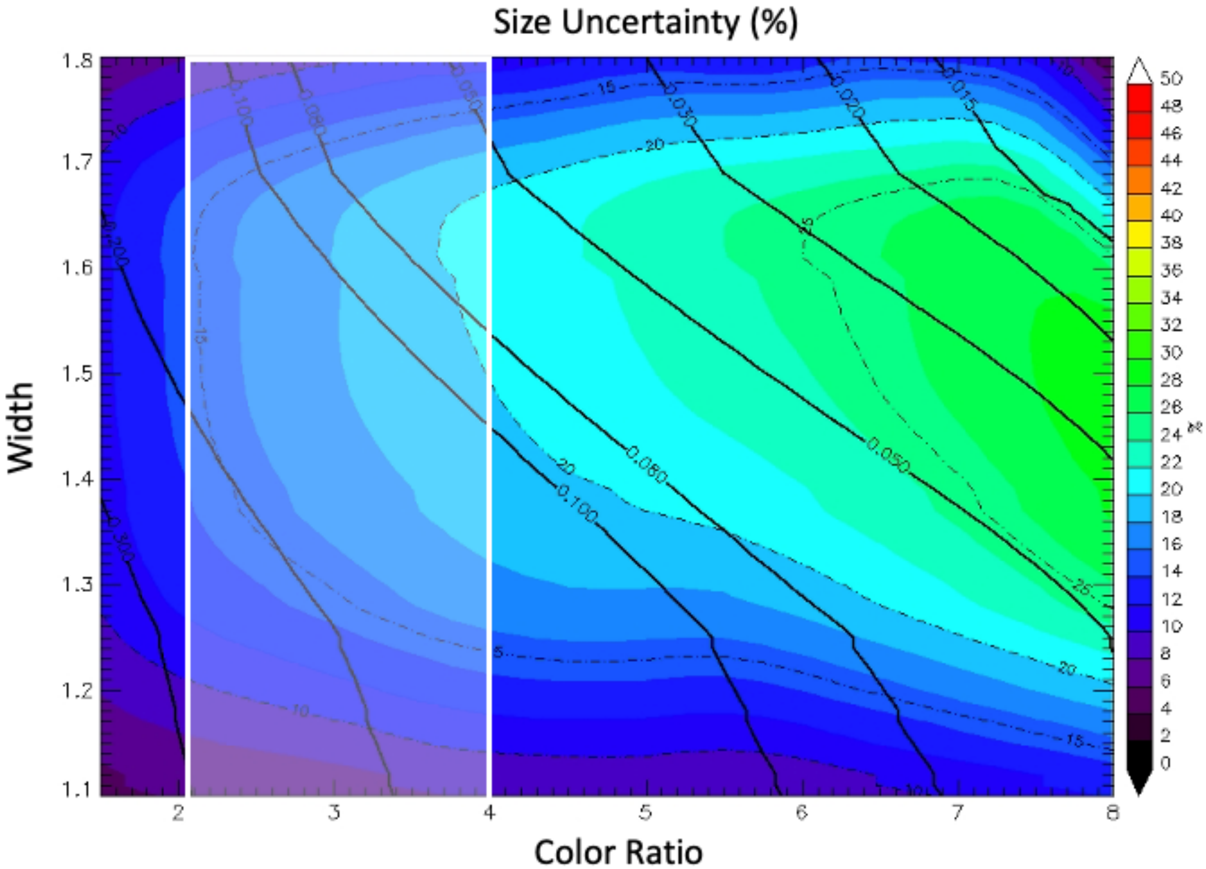

Figure 2. The color contours are the normalized error in size for a distribution width uncertainty Wu of ±0.2 and color ratio uncertainty CRu of 21%. The black contours are size in nm. White vertical lines show the typical color ratio domain for aerosols (from Fig. 6 in our paper)

To summarize, Figs. 1, 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). For 1.6 distribution width and color ratios between 2 and 4, the maximum size uncertainty is 20% (for a CRu of 42% and uncertainty in width of 0.4). This leads to a number density uncertainty of ~ 28%.

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 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 above error uncertainty analysis will be added to the revised version.

8. Line 78: The authors suggest that the "CR" is "only a function of size" and I am uncertain of what is meant by that. The CR is a function of particle size distribution parameters (both mode radius and distribution width).

R: Yes. This is a more rigorous statement by both mode radius and considering distribution. We will improve the corresponding sentence and include the dependence on distribution width described above.

9. Line 79: Could the authors please explain why the 510/869 combination was chosen instead of 510/997? The 510/997 combination would expand the "usable" range from 0.4 μm to $\approx$ 0.5μm.

R: We uses 869 nm because SAGE has the same wavelength, which simplified the validation between OMPS-LP and SAGE III/ISS. We should have made this comment earlier in the paper and we will add it to the revised version.

10. Lines 80–81: "This CR – size relationship allows us to infer the median aerosol particle radius up to $\approx$0.4 μm." This will vary depending on the distribution width.

R: Agreed, we will add the constraints to the sentence.

11. Lines 99–100: "Thus, if we use the L2 AE at two wavelengths, we have enough information to independently compute a size and number density..." This is not true as demonstrated above. Even with multiple extinction ratios you would not have enough information to definitively determine particle size. The best we can do is report a range of radii.

R: We will modify sentence, thanks for your comments. We added the range of radii based on a varying distribution width range. We are not sure we agree with the third sentence. For each extinction measured wavelength, more information is gathered on the size distribution, this is the approach used in Generalized Retrieval of Aerosol and Surface Properties (GRASP) algorithm of AERONET, for example. [Dubovik, et al., 2014]

12. Lines 103–104: In the previous paragraph the authors stated that their method was "weakly dependent on the radiative transfer model assumptions", but now they state this is a potential source of error. Could the authors please clarify?

R: We will clarify this sentence.

13. Lines 129–130: The authors stated "The uncertainty ranges of OMPS-LP retrievals are calculated from the extinction coefficients (AE), using the formula below". The context of this paragraph led me to believe that Eq. 2 was used to calculate the error in derived mode radius...but this is just an error propagation. Could you authors please clarify how this equation was used to generate the errors in their Fig. 3 & 4?

R: Eq.2 is just about the error propagation. The error of retrievals from distribution width will be added to this manuscript.

14. Section 3.2: The purpose of this section is unclear. I see 2 possibilities:
(a) Do the authors present this as corroboration of their size estimate? If so, then this fails as all this demonstrates is that the SAGE III/ISS extinction ratios are in agreement with those of OMPS-LP.
(b) Do the authors present this as validating the OMP-LP extinction ratios (i.e., since the derived radii agree with the radii derived from SAGE III/ISS then OMPS-LP and SAGE III/ISS must be reporting the same extinction ratios)? I wonder because later in this section the authors state "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." (lines 173–175). If this was their intent, then why is this needed and why does this fall within this paper (it seems a stark departure from the stated intent)? Also, didn't Taha et al. 2021 already do this validation?

R: Section 3.2 aims to assess the impact of varying scattering angles on retrievals. This analysis is also related to Reviewer comment 3, and partly evaluated the error source from that. Yes, the comparison essentially compares the color ratio between OMPS-LP and SAGE III/ISS, but the purpose is different. Additionally, it is valuable to include a section comparing the retrieved particle sizes, rather than directing readers to seek out the color ratio comparison from other sources. We will make the goals of this section clearer.

References:
Dubovik, O., Lapyonok, T., Litvinov, P., Herman, M., Fuertes, D., Ducos, F., Torres, B., Derimian, Y., Huang, X., Lopatin, A., Chaikovsky, A., Aspetsberger, M., and Federspiel, C.: GRASP: a versatile algorithm for characterizing the atmosphere, in: SPIE, vol. Newsroom, 2014.

Rieger, L. A., Malinina, E. P., Rozanov, A. V., Burrows, J. P., Bourassa, A. E., and Degenstein, D. A.: A study of the approaches used to retrieve aerosol extinction, as applied to limb observations made by OSIRIS and SCIAMACHY, Atmos. Meas. Tech., 11, 3433–3445, https://doi.org/10.5194/amt-11-3433-2018, 2018.