Reply to Reviewer 2
Z. Chen et al.
zhong.chen@ssaihq.com

We thank Reviewer 2 for reading our paper in detail and providing so much comments and suggestions. Point-by-point responses are numbered in the same order as Reviewer 2 was given in the review:

Comment 1: *This paper investigates the impact of the assumed aerosol size distribution (ASD) used in the retrieval of information from radiances measured using limb scattering, and in particular here, OMPS. The topic is important since most current satellite-borne measurements of stratospheric aerosol use this technique, OSIRIS and SCIAMACHY in addition to OMPS, and since the details of how the measurements are used to retrieve the quantities of interest are not well known outside of the retrieval community. Unfortunately, although the topic of this paper is important, it fails in many aspects. The paper begins with an unfair comparison between the currently assumed ASD for OMPS retrievals and a model distribution from a different altitude, location, and altitude. I can think of many reasons why the currently assumed ASD was a poor choice. But the fact that the assumed ASD differs from the modeled ASDs, which were never intended to mimic the currently assumed ASD, is not one of them. Yet that is what the paper does and delves into details about how these distributions differ, which patently makes no sense. Of course they are different. It is fine to use a different ASD to analyze the OMPS measurements and to show how that impacts the results, but don't begin by claiming some a priori improvement in the ASD because the new and assumed ASD differ. More detail is provided below in comment 4.7-7.9.*

Reply: We agree that the two ASDs are different in time, altitude, location, and altitude. However, one should consider these conditions in interpreting the comparison. Naturally, it is fairly common practice to compare the two ASDs that are used in the retrieval algorithm to retrieve aerosol extinction using OMPS/LP measurements under the same geophysical conditions to see how a change in ASD affects the retrieved extinction values and to determine which ASD is better. This is done by comparing the calculated ASIs from the two ASDs with the measured ASIs. These types of comparisons helped us to improve the retrieval by choosing a correct ASD.

Comment 2: *The paper would benefit from a more complete explanation of how aerosol extinction is derived from the OMPS measured radiances, leading to the variations seen in Figure 9 by altitude and latitude. A clear simple explanation of this is missing. Here is my understanding. Is it correct? As a function of latitude the OMPS measurement comes from a specific angle based on the solar-satellite geometry, Figure 5. The assumed ASD is used to calculate the phase function, but for any one measurement only a small piece of the phase function is important, and which piece is indicated by Figure 4. Now the radiative transfer equation is solved, with, for the aerosol, the only adjustable parameter the aerosol total number concentration, at least that piece of the number concentration which influences scattering at the wavelengths measured. Thus the OMPS radiances are used to determine the number concentration which has to be used with the normalized*

ASD to finally calculate aerosol extinction using Mie theory. If this is correct, something along these lines needs to be added to the paper. If it is not correct, a more correct explanation needs to be added. Presently, the authors are asking a lot of readers not intimately knowledgeable about the fine details of analyzing limb scattering measurements.

Reply: The following explanatory text was now added:
'We retrieve aerosol extinction profiles at 675 nm from OMPS/LP radiance measurements. We first calculate the cross-section and phase function from the resulting Gamma aerosol size distribution using Mie theory, then run the radiative transfer model within the aerosol retrieval code with the new cross-section and phase function to determine how the OMPS/LP aerosol retrieval changes with this new ASD relative to Pueschel ASD in the V1 (Loughman et al., 2018).'

Comment 3: *The paper lacks clarifying details. Here are some issues with further explanation below. When extinction or extinction ratios are calculated what wavelengths are used, Figures 7, 8, 9, 10? Figure captions lack information. It is not clear how units are included in an ASD described by a Gamma distribution. What is meant by more Rayleigh-like? The explanation of why the ASD extinction ratio has a correlation with reflectivity in the southern hemisphere is insufficient.*

Reply: OMPS aerosol extinctions retrievals are retrieved using wavelength at 675 nm, as indicated. We have added a note to the caption and text reminding the reader that the OMPS/LP aerosol retrieval is performed at 675 nm (see Reply to Comment 2 above). Eq. 2 presented a normalized differential size distribution. We now added N_0 with text "*n(r)* is the size distribution function ($cm^{-3}\mu m^{-1}$), N_0 is the total number density of aerosols (cm^{-3})'. We also added the units of β (1/μm) here and in the Table 2. More Rayleigh-like means that comparing with CARMA phase function, Pueschel phase function is closer to Rayleigh phase function at larger Θ. Regarding the comment on the correlation between extinction and reflectivity in the southern hemisphere, it shows that this relationship is rather complex. However, we accept the comment made by Reviewer 2 (see Reply to Comment 19 below).

Comment 4: *Finally the first statement, 15.2-5, in the conclusions section is not correct nor acceptable. Where has it been shown, "… that P(Θ) is very sensitive to the assumed aerosol particle number density near a particle radius of 0.1 micron"? Which figures? Where is the phase function shown as a function of particle size, or how this dependence figures into the impact on calculated radiances and extinctions? What the authors have shown is that if an assumed ASD, based on model results (for a time period, altitude, and location different than the previously assumed ASD), is used in place of the previously assumed ASD, then there will be differences in the calculated radiances and extinctions. But to then extend this difference to a condemnation of in situ measurements for poorly characterizing particles near 0.1 μm does not follow. This last statement may be true or false, but the results here, which use one ASD from in situ measurements, ignoring the 1000s-10,000s of other in situ ASD measurements available from aircraft and balloon, provide no answer. What the results here do show is that if an ASD from measurements* *two months after Pinatubo, at 16.5 km, are used for the assumed OMPS ASD, then the results are not as good as results using a more climatological ASD from 20 and 25 km. But this conclusion seems on face value to be quite obvious and not requiring all this work to prove. It seems what this paper is really about is the sensitivity of spectral extinction and radiances of the OMPS limb profiler to the assumed ASD. This can be done by choosing two quite widely divergent ASDs to compare, which is more or less what is done here, but without stating this fact and reading too much into the differences in ASD.*

Reply: We stand by this statement. Our findings include that data near a particle radius of 0.1 μm is impotent for deriving accurate phase function, especially for using a bimodal lognormal size distribution. As an example, Figure 3 compares the two ASDs and highlights the differences at 0.1 μm and radii greater than 0.3 μm. Figure 4 shows the phase function as a function of scatter angle for different ASDs which have different peak values near 0.1 μm shown in Figure 3. We have added an appendix, and Figures A1-A2 in it speak to the point of this comment.

We don't think looking at the individual contributions is as helpful here. We expect a broad range of particle sizes, and Figures A1-A2 in Appendix A speak to the concern that motivated this comment.

Figure R2. Mie phase functions for different values of the size parameter χ derived with a refractive index of 1.33. Observe the increasing asymmetry and complexity of the phase functions with increasing χ (Petty, 2006).

We definitely do not condemn in situ measurements for poorly characterizing particles near 0.1 μm does not follow. However, almost all OPC measurements have limitations below 0.1 µm such that the aerosol size distribution from 0.01 – 0.1 µm is poorly measured. In fact, the Wyoming in situ data have been updated recently and the minimum size measured is now claimed to be 0.094 μm (Deshler, private communication, 2018). We now added text and Appendix A below in support of this point: 'Additionally, most OPC measurements have limitations below 0.1 µm such that the aerosol size distribution from 0.01 - 0.1 µm is poorly measured. The lack of information in the OPC data gap region would result in uncertainty in calculating phase function (see Appendix A).'

Regarding the comment on the ASD and ASI comparisons, please see Reply to Comment 1. We would emphasize that these types of comparisons yield scientifically important insights. What we have shown is that ASIs from the Gamma/CARMA ASD agree better with the OMPD/LP measurements than the ASIs from the Pueschel ASD, not "*the CARMA modeled ASD does not agree with Pueschel et al.'s measured ASD",* as repeated by Referee 2. Based on this comment of the reviewer we realized, that there might be a misunderstanding of the general approach of our study, or it is possible to mix ASI up with ASD.

Comment 5: *3.9-11. This is an odd choice of an aerosol size distribution (ASD) to characterize the stratosphere, since this ASD would have been heavily influenced by the eruption of Pinatubo in June 1991. At least some words should be added to justify the* *choice. I am confident that there are many other ER-2 ASDs available in a less perturbed stratosphere. Note the values of Angstrom exponent (AE) and extinction in Figure 1 for the time period selected for the ASD in Table 1. Thus imposing a restriction of AE on the ER-2 measurements also seems artificial, and not reflective of the measurements or the time period.*

Reply: We agree that there are many other bimodal lognormal size distributions resulting from ER-2 samples. We specifically wanted a bi-modal distribution with a "fine mode" + a "coarse mode", and tuned the coarse mode fraction to give the desired Angstrom coefficient = 2 (which of course nearly made the coarse mode vanish). Now we've tried something else that seems to explain the ASI better. We added text "Our main motivation for using this Pueschel bimodal size distribution arose from the existing OPC dataset, which generally features a bimodal size distribution at the altitudes where the stratospheric aerosol extinction is greatest. But the problem of how to specify this more complex distribution is a serious concern. Our initial hope was that requiring the resulting Angstrom exponent to = 2 would minimize the importance of the 5 size parameter settings, but that is unfortunately not true in all cases."

Comment 6: *Table 1. What is fc?*

Reply: We added text to Table 1 "fc is the coarse mode fraction, which is the ratio of the number of particles of the coarse mode to the total number of particles for a bimodal lognormal distribution (Loughman et al., 2018)."

Comment 7: *Figure 2. Needs more explanation and a better figure caption. What do the lines in Fig. 2a) represent? Are these just connecting the dots? Why not show the differential Gamma distribution (GD) for comparison to the model results? Which of the distributions is shown in Fig. 2b), or is a single GD with a single set of α and β used for both altitudes? If the latter is the case then do the distributions only differ by a total number concentration? In line with the disparity between the time period and altitudes chosen for the ASDs to compare, how would the Pueschel ASD appear in Figure 2b), also normalized to 1 at the smallest sizes? Why are there so many fewer model points in red in Figure 2b) compared to the model points in Figure 2a)?*

Reply: We have added 'The lines in (a) are just connecting the circles.' Since we use ASD at 20km in the retrieval, Fig. 2b showed a Gamma function fit to the cumulative number density just for 20km only (for the purpose of comparison, we now added a GD fit for 25km as Reviewer 1 suggested). We do not use a single GD for both altitudes. So the latter is not the case. We added text: 'The cumulative CARMA data (circles) are chosen in consisting with the OPC in situ measurements which has a gap ranging from 0.01 µm to 0.1 µm (Kovilakam and Deshler, 2015).'

Comment 8: *Eq. 2. I don't understand the units in this equation? The n(r) suggests a differential ASD in standard usage. The only units on the right appear in r^(α-1) and β^α, so the units are m^-1, which is correct for a normalized differential distribution, but then there must be an No appearing in Eq. 2. In short how does the GD provide a number* *concentration (mˆ-3) as implied in Eq. 5 or a differential number concentration (mˆ-4) as implied in Eq. 2?*

Reply: Eq. 2 presented a normalized differential size distribution. We now added N_0 with text "*n(r)* is the size distribution function ($cm^{-3}\mu m^{-1}$), N_0 is the total number density of aerosols (cm^{-3})'. We also added the units of β (1/μm) here and in the Table 2. Please see Reply to Comment 3 above.

Comment 9: *Eq. 3. What is the upper limit of the integral? There is a problem with the equation editor, so that it looks like the integral is from 0 to 0.*

Reply: The document that we see does the integral in equation 3 with respect to r from 0 to ∞, not from 0 to 0. In the pdf version on the website, the same thing appears. Now we replaced $\int_0^\infty$ with $\int\limits_0^\infty$. Is it ok?

Comment 10: *6.11. I believe the authors mean, ... using a Levenberg-Marquardt nonlinear least squares regression algorithm, rather than "by".*

Reply: We replaced 'by' with 'using'.

Comment 11: *6.20-21. "CARAM data" and "GD distribution"?*
Reply: Reviewer 1 also noted this. The "GD distribution" has been changed to "Gamma distribution ".

Comment 12: *4.9-7.9 and Figures 3 and 4. This entire discussion beginning with the introduction of the CARMA modeled ASD needs to be changed. What the authors have shown with the present discussion is that the CARMA modeled ASD does not agree with Pueschel et al.'s measured ASD. Why should they be similar? Pueschel's measurements were made at 16.5 km in August 1991 at 36 N and 121 W, approximately 2 months following Pinatubo. The CARMA results are from a three year summertime climatology at 20 and 25 km at 41 N and 105 W. Of course these two ASDs are different. The text here is comparing apples and oranges and claiming they are different. Well yes they are different, but we knew that. If the authors really want to make the case that GD fits to CARMA are better than lognormal fits to measurements, then let them either compare CARMA with the dates and altitudes of the Pueschel results, or compare CARMA with measurements over Laramie, which they claim are the reasons they produced CARMA results at that position. Or better yet just compare GD and lognormal fits to the same CARMA data.*

Reply: The comment on ASD comparison repeats the previous one. Please see Replies to Comments 1 and 4 above. Regarding comparing GD and lognormal fits to the same CARMA data, we made the following two figures as Reviewer 1 suggested.

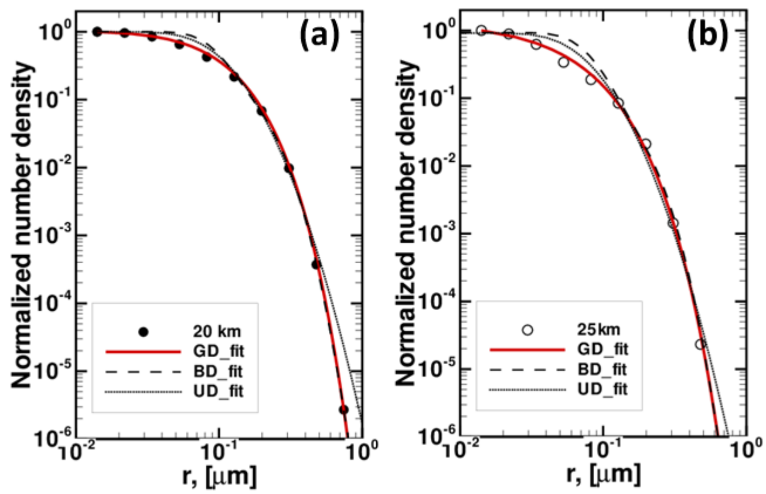

Figure R1. Gamma size distribution (GD), unimodal normal distribution (UD) and bimodal lognormal size distribution (BD) fits to the cumulative number density (N>r) for 20 km (a) and 25km (b). For the purpose of comparison, the cumulative CARMA data between 0.02 - 0.1 μm are also shown.

Comment 12: *7.9-7.10. More Rayleigh-like? What is the basis for this statement? A Rayleigh phase functions varies from 0.07 to 0.11. Pueschel's phase function varies from 0.83-0.02 and is closer to either of the CARMA phase functions than Rayleigh.*

Reply: The baseline for this statement is Rayleigh phase function. Please see Reply to Comment 3 above.

Comment 13: *9.5-6. Why would we expect that a single quantity, AE, would be enough information to determine two fitting parameters?*

Reply: We removed the unnecessary text 'Note that AE by itself does not provide information to determine both α and β, and hence r_{eff}'.

Comment 14: *Figure 7 and 10.1-3. What is meant by extinction ratio and phase function ratio? 11.1. I assume the ratios of aerosol extinctions are the ratio of 525/1020 nm, but this should be stated somewhere and it would make sense to include this information on the figures, or at least in the figure captions. Or is this the ratio of extinctions at some unspecified wavelength for the two ASDs? Or is this a ratio of ratios? Some clarification is needed.*

11.3 and Figure 8. How is the ratio of phase functions calculated? How can there be a single phase function by latitude, since the phase function is angularly dependent? Since a ratio is shown why use the inverse? The phase function ratios in Figure 7 are all near 1. Why now the shift from 20/25 to 20.5/25.5 km?

Now I think I understand what is being done. Perhaps Fig. 5 could be modified to add a second panel to show that for any latitude there is only a single value, or perhaps a small angular range, of the phase function that applies, depending on the season. Then when the ratio of phase functions are discussed it will be clear what ratios are being used. It would be very helpful to show the variation of phase function with latitude, conflating Figures 4 and 5, for the two ASDs.

Reply: There might be a misunderstanding about the ratios. Figure 7 (and Figure 6) relate the Gamma parameters to more physical quantities and the impact of a particular change (α,β±10%) on the retrieval. So the extinction ratio and phase function ratio shown in Figure 7 are the ratios of the CARMA perturbations that result from adjusting Gamma parameters by ±10% to the CARMA baseline model that is calculated using the fitted Gamma parameters (α=1.8, β=20), while Figures 8 and 9 show the impact of the two ASDs (CARMA and Pueschel) on the retrieved extinction. The labels and legends in Figures 8 and 9 clearly represent the ratio of CARMA to Pueschel. For the purpose of comparison, we use the inversed phase function ratio in Figure 8 and 9 since the change in phase function and the change in extinction are roughly anti-correlated (see Figure 7). The shift from 20/25 to 20.5/25.5 km due to we calculate P(Θ) using the assumed ASD at 20 km and we retrieve extinction using OMPS measurements at 20.5 and 25.5 km. We show phase function as a function of latitude based on the relationships shown in Figures 4 and 5. Please also note that in Figure 8, we show daily zonal mean data in 5° latitude bands.

Comment 15: *11.7. Now multiple scattering is brought in which has not so far not been mentioned. This seems rather cavalier, since no further mention is made of multiple scattering.*

Reply: The review states earlier that "only a small piece of the phase function is important" (***Comment 2***). This only makes sense if multiple scattering (MS) plays a small role. And our Figure 10 clearly shows how the phase function influence declines as ρ increases (adding more diffuse radiation to the atmosphere). Please refer Loughman et al. (2018) for details.

Comment 16: *Duo?*
Reply: Reviewer 1 also noted this typo. It is fixed.

Comment 17: *Figure 9. Again! What wavelength extinctions are being shown? The limitations on the piece of the phase function used by latitude for any measurement helps immensely to explain why there is very little variation in the ASD extinction ratio between 40 and 80 N in Figure 9.*

Reply: The OMPS/LP aerosol retrieval is performed using wavelength at 675 nm (see Replies to Comments 2 and 3). Your explanation may be correct.

Comment 18: *12.7. Right, once the extinction calculation is understood, it is clear that a lower value of the phase function will lead to larger number concentrations for the same limb scattering measurement, thus larger extinction.*

Reply: That is correct.

Comment 19: *12.9-20 and Figure 10. Why is the ASD extinction ratio correlated with reflectivity in the southern hemisphere? The authors do not explain this, they sort of* *imply that reflectivity has a larger variation in the southern hemisphere, but this is not the case. The reflectivity variation is similar in both hemispheres. It really comes down, again, to the conflation of Figures 4 and 5, illustrating how the southern hemisphere is so much more sensitive to the choice of ASD, than the northern hemisphere, due to the larger differences in backscatter compared to forward scatter.*

Reply: It is true that the reflectivity variation is similar in both hemispheres. We agree with you and added text: 'The conflation of Figures 4 and 5 illustrates that how the southern hemisphere is so much more sensitive to the choice of ASD, than the northern hemisphere, due to the larger differences in back scatter compared to forward scatter'.

Comment 20: *Fig. 11 and 14.1-6. Panels are also shown for 745 and 869 nm. Why aren't these in the figure caption and mentioned in the text? What is the explanation for the larger residuals for the Pueschel ASD in the northern hemisphere? This seems surprising given the tight extinction ratio, Figure 9, and the similarity of the phase functions, Figure 4, for the two ASDs in the northern hemisphere, and since ASI is proportional to E*P(Θ).*

Reply: Thanks for pointing this out. We now added "745 and 869 nm" to the caption and text. We also added explanatory text 'The larger residuals for the Pueschel ASD in the northern hemisphere suggest that CARMA ASD is better than Pueschel ASD for OMPS/LP measurements'.

Appendix A. Fitting Aerosol Size Distributions to OPC data

One of the longest and most comprehensive records of local stratospheric aerosol conditions is from the University of Wyoming's optical particle counters (OPC) carried on weather balloons at Laramie, Wyoming, USA (41°N) at altitudes up to 30km. The instrument measures the number of aerosol particles in several size bins, ranging from 0.15 to 2 μm. In most cases, bimodal lognormal size distributions (BD) is used to fit OPC data if there are enough sizes measured. For background stratospheric conditions, however, OPC data often does not provide sufficient information for a robust BD fit. An example of this problem is illustrated in Figure A1, which shows four deferent BD fits to the same OPC concentration data of April 12, 2000 for an altitude at 20 km (Kovilakam and Deshler, 2015). Since the five parameters of a BD are interdependent, the fits were constrained by using different values of f_c. The fitted parameters as well as the calculated AE and r_{eff} are given in Table A1.

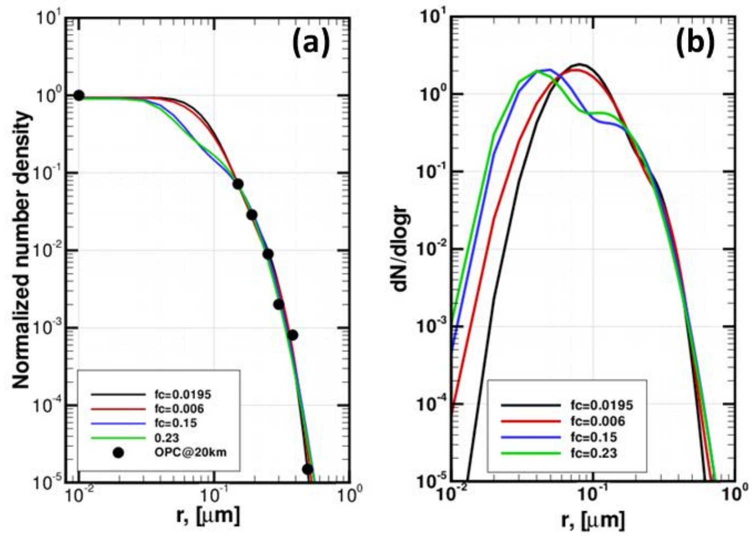

Figure A1. Estimated bimodal lognormal cumulative distributions (a) and differential distributions (b) for nonvolcanic (20000412 implies 12 April 2000) OPC measurement at 20 km (Kovilakam and Deshler, 2015). Measurements on the left panel are shown as black dots.

Table A1. Four BD fits to OPC data measured at Laramie Wyoming on April 12, 2010 at 20km.

	ASD_1	ASD_2	ASD_3	ASD_4
Coarse mode fraction, f_c	0.0195	0.006	0.15	0.23
Mode radius, r_i (μm)	0.080,0.238	0.075,0.280	0.046,0.140	0.040,0.120
Mode Width, σ_i	1.45,1.25	1.56,1.21	1.45,1.43	1.43,1.47
Angström exponent, AE	2.45	2.40	2.40	2.40
Effective radius, r_{eff} (μm)	0.1332	0.1335	0.1437	0.1470

The four fits have similar Angström exponents but differed from each other significantly in radius range between 0.01 μm to 0.1 μm. As a consequence, the differences between the ASDs near 0.1 μm lead to significant changes in P(Θ) as shown in Figure A2. It can be seen that P(Θ) is quite sensitive to the value of dN/dlog10r at around 0.1μm when Θ> 90°. The larger this value, more Rayleigh-like the P(Θ).

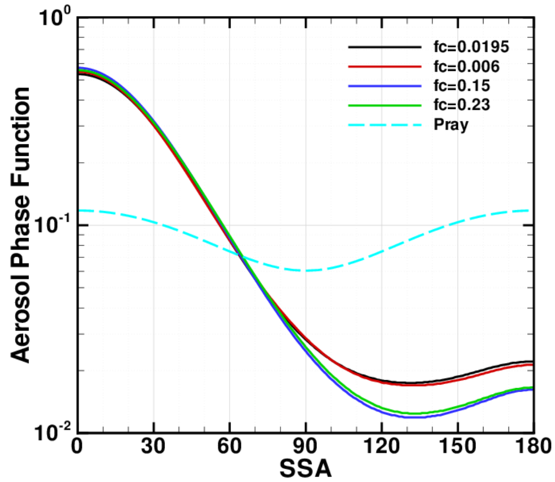

Figure A2. Aerosol phase functions at 675nm as a function of single scattering angle for the four ASDs listed in Table A1.