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Interactive comment on "Impact of aerosol size distribution on extinction and spectral dependence of radiances measured by the OMPS Limb profiler instrument" *by* Zhong Chen et al.

Anonymous Referee #2

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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.

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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.

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.

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.

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.

Here are more specific questions, comments, and corrections for this paper by page and line number.

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.

Table 1. What is fc?

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)?

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 rˆ(α-1) and βˆα, so the units are mˆ-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?

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.

6.5. This is a nice result to see.

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

6.20-21. "CARAM data" and "GD distribution"?

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.

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.

7.14-15. This seems a little surprising since extinction is the loss of light in the forward direction.

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

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.

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. Duo?

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.

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.

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.

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).

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