Reply to Reviewer 3
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General: Article compares the retrieval results using two different assumed aerosol distributions with OMPS/LP limb scatter radiance profiles. Comparisons are shown to be more consistent with the spectral dependence of OMPS/LP data as well as correlative aerosol extinction data from SAGE III/ISS. This information is worthy of publication.
We appreciate the constructive comments offered. Point-by-point responses are numbered in the same order as the comments given in the review.

However, the contents of this manuscript seem similar to another submission by the same author to AMTD (doi.org/10.5194/amt-2018-4). For example, both papers compare the degree to which two aerosol size distributions (bi-modal vs. gamma) match OMPS/LP radiance spectra. And, three figures in amt-2018-221 are "from amt-2018- 4". What is new here? So, this document should do a much better job of clearly conveying the information that is unique to this manuscript. Otherwise, I do not support publication
Authors: A previous version of this paper was submitted to this journal as manuscript amt-2018-4. We prepared responses to the original reviews and returned the revised manuscript to the journal. The associate editor recommended further revision, and stated "… you might think about re-writing the manuscript (possibly including the comparison with SAGE data and the new OPC data) and submitting anew to AMTD". The current manuscript (amt-2018-221) has been substantially revised in accordance with this editorial guidance. Specifically, we expanded on amt-2018-4 to provide extensive comparisons with SAGE III/ISS data that were not available at the time that manuscript was submitted. The present journal editor for this manuscript also recommended that "you cite the old submission in the new submission". We have added a brief explanation of the relationship between this manuscript and amt-2018-4 at the end of Sect. 1 to clarify the situation for the reader.

Specific comments:
1. Page 3, line 20: need to italicize I0
Authors: Fixed.

2. Section 2, especially page 4 is confusing. Are you describing V1.0 or V1.5? If the only difference between V1.0 and V1.5 is the phase function, then you need to clearly state that. As is, page 4, line 5 reads as though the use of Chahine's method is unique to V1.5 and was not used for V1.0. Furthermore, line 16 describes the number of iterations used for V1.0. How does that compare to V1.5?
Authors: We have added text in Sect. 2 to clarify that the primary difference between the V1.0 and V1.5 algorithms is the particle size distribution. We also address other changes, including the number of iterations and the maximum change in extinction allowed for each iteration.

3. Page 5, line 27, "(with some probably large uncertainty)" is conjecture that serves no purpose. Suggest deleting it or supplying evidence to prove your claim.

Authors: You are right. We deleted "(with some probably large uncertainty)".

4. Page 5, 29: suggest ". . .well, the. . ."
Authors: Done. Thank you.

5. Page 6, why not use the CARMA bins directly?
Authors: The reviewer is correct that the aerosol optical properties can be calculated based on a RTM for each bin of the size distribution. However, the Mie calculation in the current LP aerosol code requires an analytic aerosol mode, rather than bin data. We use an analytical model of aerosol particle size distribution which deals with the ASD as a mean of size spectrum to accurately fit a cumulative distribution function (CDF) on the binned data using Deshler's method (Deshler et al., 1993, 2003).

6. Page 7, line 5: What values of rmin and rmax are used here?
Authors: We updated the text to provide the values: "r_{min} = 0.01 μm to r_{max} = 3 μm."

7. Page 7, line 15: Was the solution for the BD case determined using the method given earlier in line 6? Appendix X, indicates there is sufficient freedom to fit a wide range of size distributions.
Authors: Yes, it was the same.

8. Page 7, line 16: Fig. 1 leads me to believe that point at 0.015 um is used in the fit, but the text states that values "between 0.01 um and 0.1 um" were not used. Need to correct the figure caption or the text in the body.
Authors: You are correct. It was a typo in the text. It should be between 0.02 μm and 0.1 μm". We corrected the typo in our new revision.

9. Page 7, line 24: I must be missing something, but I do not see how for the bimodal ASD has "the smallest dN/dlogr value at r= 0.3 um. On the figure the lowest dN/dlogr is at r = 0.015um and 1um. Similar comment for the caption in Fig. 2.
Authors: We have revised the caption of Figure 1 to clarify this point.
"Among these size distributions, the V1.0 function has the largest $dN/d\log r$ value at 0.1 μm, but the smallest $dN/d\log r$ value at 0.3 μm.".

10. Page 11, line 13: The doi for the SAGE III/ISS V5 data is: 10.5067/ISS/SAGEIII/SOLAR_BINARY_L2-V5.0 for the binary, and 10.5067/ISS/SAGEIII/SOLAR_HDF4_L2-V5.0 for the HDF version.
Authors: Thank you for the information. We replaced "available through NASA's Atmospheric Science Data Center" with "the doi for the SAGE III/ISS V5 data is: 10.5067/ISS/SAGEIII/SOLAR_BINARY_L2-V5.0 for the binary, and 10.5067/ISS/SAGEIII/SOLAR_HDF4_L2-V5.0 for the HDF version."

11. Page 14, line 16: suggest ". . .all having similar Angstrom exponents. . ."
Authors: Fixed, thank you.

12. Figure 2: What data do you have to make the claim at the end of the paragraph?

We have modified text in Appendix A to clarify this statement:
"ASD_1 (black) and ASD_2 (red) have larger values of *dN/dlogr* around r = 0.1 μm shown in Fig. A1. Larger values of P(Θ) derive from the two ASDs in this range are therefore closer to a Rayleigh scattering behavior."