Interactive comment on "A comparison of lognormal and gamma size distributions for characterizing the stratospheric aerosol phase function from OPC measurements" *by* Ernest Nyaku et al.

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In the present paper, authors try to answer the question which shape of the aerosol size distribution (ASD) is it better to use for stratospheric aerosols. In the paper, two shapes of the stratospheric ASD were taken into consideration, namely, uni-modal lognormal (UMLN) and gamma-distribution. Both distributions were fitted to the data from Optical Particle Counters (OPC) and the CARMA model. The quality of the fits was compared using the χ^2 criterion. Based on this comparison, it was concluded that gamma-distribution provides more realistic aerosol phase function (APF) than UMLN

distribution. The latter application is particularly important for the aerosol extinction retrievals from the limb scatter instruments. While the research itself is thoroughly conducted and convinces the reader that gamma-distribution fits better than UMLN OPC and CARMA model data, the part about the use of gamma-distribution in the limb scatter retrievals is completely missing. There is a long discussion in the manuscript about the importance of the APF for limb scatter instruments (which is absolutely true), and there are nice studies showing the APF from the gamma-distributions. However, the authors did not show any application of the improved APF in the retrievals. Based on this major issue, the following can be suggested:

- authors include some additional study, where the improvement of the limb retrievals with the corrected APF is shown;

- or authors revise the manuscript in a way that they, for example, leave the recommendation to fit OPC data with gamma-distribution rather than with UMLN during the background aerosol loading.

While both revisions will be sufficient to publish the manuscript in AMT, I would suggest going with the first one. Otherwise, the purpose for the APF discussion should be justified differently.

Response: A parallel study by (Chen et al. 2018) have compared retrieved aerosol extinctions using the OMPS/LP V1.0 (bimodal lognormal), V1.5 (gamma distribution) derived from the CARMA model output to the extinction profile derived from SAGE III (on the International Space Station). The results show an improvement in the V1.5 extinctions to within 10% at altitudes 19-29 km. The authors of the paper are including this information and referencing the above paper.

Specific Comments:

1- P.2, L.1: Maybe it would be good to add to the cited works the newer studies? E.g., Ivy, D. J., Solomon, S., Kinnison, D., Mills, M. J., Schmidt, A., and Neely, R. R.: The influence of the Calbuco eruption on the 2015 Antarctic ozone hole in a fully coupled chemistry-climate model, Geophysical Research Letters, 44, 2556–2561, 2017

Response: The citations have been updated to include (ivy et al. 2017). Also the effect on the ozone hole enhancement by the presence of volcanic aerosols associated with Calbuco has been mentioned in the same paragraph.

2- P.2, L.27: Here it is important to mention such sources of stratospheric aerosols as wildfires smoke (see for example Khaykin et al. (2018), https://doi.org/10.1002/2017GL076763) and SO_2 from Asian pollution (e.g., Randel et al. (2010), DOI:10.1126/science.1182274).

Response: Other sources of stratospheric aerosols from wildfire smoke and SO_2 from Asian pollution have been mentioned and the Khaykin et al. (2018) and Randel et al. (2010) have been cited.

3- P.2, L. 28 and 33: Is there a difference between $P_a(\Theta))$ and APF? If there is, then it should be better highlighted. If there is not, then just one abbreviation should be used throughout the manuscript.

Response: There is no difference between $P_a(\Theta)$ and APF. Only $P_a(\Theta)$ will be used to subsequently represent the Stratospheric Aerosol Phase function.

4- P.3, L.70: It would be nice to mention here, and in Table 1 SCIAMACHY aerosol extinction algorithm V1.4 (see Rieger et al. (2018)).

Response: SCIAMACHY has been mentioned in both places.

5- P.3, Eq.(1): The above-mentioned products provide aerosol extinction at one wavelength, so the Eq. (1) can not be used for them to calculate Ångström exponent, because the second extinction coefficient is missing. However, the Eq. (1) is generally absolutely correct and can be used to calculate Ångstöm exponent using the ASD and

Mie theory. It would be better to add the sentence before, that the formula is correct for the general case. Otherwise, the reader gets the impression that Ångström exponent is computed from the products

Response: The sentence has been revised to include that the extinction of the two wavelengths are derived using the ASD and Mie theory. "The figures also display for each fit the Ångström exponent (AE) that was computed using Equation (1), where λ_1 and λ_2 are 525 nm and 1020 nm respectively.

6- P.4, L.103: Firstly, for all three publications cited here UMLN was used. Secondly, they all used certain assumptions (simply because spaceborne measurements do not provide enough pieces of information). I think it should be mentioned here

Response: The cited publications have been updated to include Loughman et al.(2018), which used BMLN aerosol size information for the extinction retrievals. Also it has been mentioned that space-borne measurement do no provide enough pieces of information.

7- P.6. L.172: I think it should be explained why the particles in size range between 0.05 and 0.1 μm are so important in this study. Smaller particles also scatter solar radiation, and the next sentence says that OPC measurements include particles with radii greater than 0.01 μm. Therefore, the importance of this particular size range should be justified.

Response: In a case study (see Figure A1 of Chen et al., 2018), four bimodal lognormal size distributions were fitted to the same data set which did not have a measurement between 0.05 μm and 0.1 μm. The differences observed in the resulting phase functions were due to the differences of the fits at that radius range because all four fits captured the larger bins very well. This shows the importance of aerosol particles within the radius range 0.05 μm and 0.1 μm.

8- P.12, L.258-261: It is hard to understand the purpose of the whole Section 3.2 and

its main message. Is the purpose to show that gamma-distribution is less sensitive to the particles smaller than 0.1 μm? Then it is a good result for OPC fit, and it should be highlighted. However, for the limb instruments, this fit might be relatively useless then Coarse resolution of the data on particles smaller then 0.1 μm does not mean that there are no particles of this size and that they will not influence the "real" distribution. Or is there a misunderstanding of the Section?

Response: This section tests the sensitivity of the two unimodal distributions to determine which distribution would accurately predict the amount of particles within the particles radius range of 0.01μm and 0.1μm during the period when there are no measurements within this particle radius range (no small bin (nsb) or OPC_{nsb}) and when there is at least a measurement within that range (OPC).

Also the conclusion has been rephrased to read:
"The conclusion drawn from this comparison is that the phase functions calculated with the gamma distributions with and without the small bin are comparable to each other to within 10% as compered to those of the UMLN distribution. This signifies that the gamma distribution is relatively insensitive to the addition of an intermediary bin between 0.05 μm and 0.1 μm, whereas the UMLN distribution is quite sensitive to this additional information."

9- P.14, L.272: Firstly, it is better to use μm instead of the nm here, because it might confuse the reader. Secondly, I assume that the bins are not equally distributed over the presented size range and that there is information on small particles. Were there attempts to fit gamma-distribution to the "raw" output of CARMA model to see how this distribution behaves with more information on the particles smaller than 0.1 μm? Or this question is irrelevant because the purpose of Section 3.2 was wrongly interpreted?

Response: First, nm has been converted to μm. Secondly, The CARMA model "raw" outputs were used because they provide enough information on smaller particles

smaller than 0.1 μm, and for this study these model outputs were subsetted into the OPC measurement bins to find out which of the two uni modal distributions was the best fit to this model output. The conclusion drawn from section 3.2 is to use the gamma distribution to fit OPC_{nsb} data. This section also shows that the gamma distribution is the best fit to the CARMA model outputs. A table showing the distribution of the aerosol size bins used in the CARMA model has been added.

In our next study we plan to fit the gamma distribution to all the "raw" outputs of the CARMA model.

10- P.15, L.303-305: If I understand correctly, CARMA is planned to be used for OMPS retrieval,which should be explicitly mentioned.

Response: The plan to use phase functions derived from the CARMA model outputs in OMPS retrievals has been stated in the manuscript.

11- P.18, L.334-349: As it was said in the general comments, the part about the space borne instruments is absolutely missing. Thus, it should be either removed and reformulated for OPC measurements, or some real studies using limb instruments should be done

Response: A parallel study by (Chen et al. 2018) have compared retrieved aerosol extinctions using the OMPS/LP V1.0 (bimodal lognormal), V1.5 (gamma distribution) derived from the CARMA model output to the extinction profile derived from SAGE III (on the International Space Station). The results show an improvement in the V1.5 extinctions to within 10% at altitudes 19-29 km. The authors of the paper are including this information and referencing the above paper.

Technical corrections:

1- P.1, L.1-2: The first sentence in the abstract leaves an impression that OPC provided

measurements only from 2008-2017, which is not true. See e.g., Deshler et al. 2003.

Response: it has been clarified in the abstract that this is a subset of the total data since measurements have been taking place since 1971 (Deshler et al. 2003).

2- P.3, L.28: There is not much sense to shorten "solar occultation" to "SO" since it is used just once. If the authors want to save some space, it is better to shorten "Figure"to "Fig." and "Equation" to "Eq.".

Response: Noted

3- P.4, L.98-99: The citation here should be done as "Deepak and Box (1982) or Hinds(1982)".
Response: Noted

4- P.4, L.101: Sparc better spelled as SPARC.
Response: Noted

5- P.6, L.151: Here I think is a typo, and 6 data points were meant.
Response: Because we are using the coarse mode fraction (CMF), which is the ratio of the coarse mode concentration to the total, the number of parameters reduces from 6 to 5.

6- P.8, L.212: Maybe "percentile" should not be in italics?
Response: Noted.

7- P.14, L.282: Maybe leave χ^2 here instead "chi-squares"?
Response: Noted

8- P.18, L.308: I think citations should be listed chronologically.
Response: Noted