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Atmospheric Measurement Techniques Discussions

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.

Anonymous Referee #2

Received and published: 29 May 2019

General comment:

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

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Discussion paper

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.

Specific comments:

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

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chemistry-climate model, Geophysical Research Letters, 44, 2556–2561, 2017.

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).

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.

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)).

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.

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.

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.

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.

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

Technical corrections:

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.
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.".
P.4, L.98-99: The citation here should be done as "Deepak and Box (1982) or Hinds (1982)".
P.4, L.101: Sparc better spelled as SPARC.
P.6, L.151: Here I think is a typo, and 6 data points were meant.
P.8, L.212: Maybe "percentile" should not be in italics?

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P.14, L.282: Maybe leave χ^2 here instead "chi-squares"?
P.18, L.308: I think citations should be listed chronologically.

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