General comment:

In this paper, the authors study which aerosol particle size (ASD) distribution fits better OPC measurements and/or CARMA model output. Here, two ASDs were taken into consideration, namely, unimodal lognormal (UMLN) and gamma distribution. The authors also look at how the aerosol phase function ($P_a(\Theta)$) changes with different ASDs and particle size range taken into consideration.

While, in general, this revision of the manuscript got significant improvements in comparison to the previous version, the authors still did not clarify all the points from the previous review. Because of that, I think, another revision of the manuscript should be done. Even though the conclusions were reformulated, they are not yet sufficient to be published. If I understood this paper correctly, its take-home message is "you should know where your ASD came from". This message does not need 21 pages of studies.

Reading this revision of the manuscript, I got an impression that the authors tried to say that UMLN is a good fit for OPC measurements, while CARMA model output is better fitted with gamma distribution. However, both of the distributions are correct depending on the source of information one uses, and there is no truth, which of them should be used in limb retrievals. This is a very important result for the limb community, which looks for the last 15 years for the solution of the aerosol extinction coefficient dependency on $P_a(\Theta)$ problem, and in particular, tries different ASDs in order to improve products. If I am correct in my understanding, then the conclusions should be reformulated this way, providing numerical proof. The authors quantify the changes in the $P_a(\Theta)$; however, they do not explain how this change in $P_a(\Theta)$ influences the resulting limb extinctions, which in my opinion is absolutely essential. Again, if the authors do not want to deal with limb retrievals, that is fine, but then the paper should be revised to remove a long introduction about limb instruments. If the authors want to leave the discussion on how this study is important for the limb community, then they should show it differently. The suggestions on how it could be done can be found in the specific comments.

Another general comment, the literature review is, in my opinion, too long. Additionally, some citations were wrong. You can find suggestions on how to resolve these problems in the specific comments.

Specific comments:

P.3, L.19: To be fair, Bingen et al. (2004) retrieved three parameters of ASD based on SAGE II data. Even though the results might not be absolutely gorgeous, the work is hard to ignore.

P.3, L.28-30: Since here there is a discussion about the limb stratospheric aerosol products and their dependency on $P_a(\Theta)$, I think, it would be worth to mention new OSIRIS v7 product (Rieger et al., 2019). The University of Saskatchewan group minimizes the $P_a(\Theta)$ dependency by using a multiwavelength approach.

P.3, L.34: Generally, in my opinion, the AE discussion is unnecessarily long, it does not play any role further in the paper. I can even justify the Equation (1) and Table 1, since AE is shown on the Figures, and Table 1 gives an overview of ASD used for different products. However, the information about the AE>2 is absolutely unnecessary and in addition, not absolutely correct. The cited paper by Schuster et al. (2006) was based on AERONET data, and its authors talked about tropospheric aerosols of different origin, where the term "small" could be applied, because there is a reference what is "small" (urban aerosol) and what is "large" (marine aerosol). However, those terms are not so easy to translate to the stratosphere, where the division to "small and large" is blurry. I suggest removing this sentence.

P.4, Caption of table 1: Honestly, I find (Nyaku, 2016) citation here absolutely unnecessary. The formula for Ångtröm exponent is given in Equation (1), and providing information which wavelengths were used for its calculation is more than sufficient.

P.4, Table 1: SCIAMACHY ASD parameters are wrong; those are V1.1 parameters (von Savigny et al, 2015). For V1.4 (Rieger et al., 2018), the ASD is the same as in OSIRIS retrievals. So, I suggest just to put SCIAMACHY in the same line as OSIRIS.

P.5, L.20: I might have missed something, but I have not found any ASD parameters retrieval information in Loughman et al. (2018).

P.7, L.4: Again, I might have missed something, but to define a BMLN mathematically, 6 parameters are needed, namely, 2 median radii, 2 sigmas, number density of the fine mode and number density of the coarse mode (2 number densities) or a coarse mode number density or a total number density and the CMF. In the cited paper (Malinina et al., 2018), the authors also mention 6 independent pieces of information. So, I suggest either to clear why you think 5 are enough and remove the citation, or change it to 6 data points.

P.1, L.22 vs P.14, caption of Figure 5: In the beginning of the manuscript, it is said that a refractive index used in this study is $1.45{+}0i$, in the caption for Fig. 5 it is written that X was calculated with a refractive index of 1.33. I haven't found any information in the paper, why it is so. Is there a reason for a refractive index to be 1.33? If not, in order to be consistent, I suggest replotting the $P(\Theta)$ with a refractive index of $1.45{+}0i$.

P.18, L.24: I think, it is important to put here that in the study of Chen et al. (2018) the comparison was between collocated zonal mean profiles.

P21. L. 30 - P.22, L.6: I do not think it is the right conclusion which deserves 21 pages of manuscript. The authors base their conclusions on Chen et al. (2018) studies, which is fine if the conclusions were deeper. Otherwise, it shows that the study of Chen et al. (2018) was useful, no this one. To give current paper scientific significance, other conclusions should be drawn. For example, in section 3.3, the authors talk about the percentage differences in $P_a(\Theta)$. However, to understand how this percentage differences in $P_a(\Theta)$ influence limb measurements, some reference should be given. Basically, if the changes in $P_a(\Theta)$ with the "better" or "worse" fit distribution are in terms of the UMLN within 0.01 μm change of r_m, then the conclusion should be, that the shape of the distribution does not play any role for the limb instruments. The $P_a(\Theta)$ changes not significantly for aerosol extinction retrievals, and everyone can use whatever distribution they want since the uncertainties in the resulting product will be the same anyway. If I understand the paper right, that is what the authors tried to say, or at least, their thoughts went this direction. This is a good and very important result; OSIRIS and SCIAMACHY teams can use their distribution, OMPS team can use theirs. There is no truth which one is right, and some other way of minimizing $P_a(\Theta)$ dependency should be found. However, this should be clearly formulated, not in a way it is done now. If the change in the phase function is comparable, for example, to the change in 0.10 μm of r_m in UMLN, then I do not understand the current conclusion. The phrase "it is imperative to one to have a knowledge about the nature of the measurements from which parameters of any distribution are provided" does not deserve 22 pages of paper and should be more clearly formulated. Again, the authors write that "The overall implication of this study is to show the importance of the nature of $P_a(\Theta)$ used in the retrieval if the stratospheric aerosol extinction from limb scattering measurements", however, the limb scattering part is missing.

Technical corrections:

P.2, L.26: "..., which measures extinction directly" is not technically correct. Maybe, it is better to reformulate it to "..., which derives extinction directly" or something similar?

P.6, Equation 2: I don't think the multiplication signs are necessary here.

P.6, L.17: Maybe, it is better to write "logarithm" instead of "log".

P.8, L.12: I think, it is easier to put this link into references; here, the link is a bit out of place.

Figures 3, 6, 9, 10: There is a first "A" missing in SCIAMACHY on those Figures.

P. 14, L. 12-13: If you work in Latex, it would be nice to put a tilde in between 0.3 and μm.

P. 1, L. 23-24: It would be nice to remove space after "Station" and a bracket sign.

P. 20, caption of Figure 10: Maybe, it is better to put "panel on the left" and "panel on the right" instead of "figure"?

P. 22, L. 7: Again, maybe it is better to put the link into the references or split it somehow?

References:

Rieger, L. A., Zawada, D., J,Bourassa, A. E., and Degenstein, D. A. (2019). A multiwavelength retrieval approach for improved OSIRIS aerosol extinction

retrievals. Journal of Geophysical Research: Atmospheres, 124, 7286–7307. https://doi.org/10.1029/2018JD029897
von Savigny, C., Ernst, F., Rozanov, A., Hommel, R., Eichmann, K.-U., Rozanov, V., Burrows, J. P., and Thomason, L. W.: Improved stratospheric aerosol extinction profiles from SCIAMACHY: validation and sample results, Atmos. Meas. Tech., 8, 5223–5235, https://doi.org/10.5194/amt-8-5223-2015, 2015.