Dear reviewers and editor:

We would like to thank you for taking the time to review our manuscript and suggest such valuable information in order to improve its quality. Even the most trivial suggestion helped in the progress of the text's value.

A set of minor changes and typos has been corrected throughout the document. Following this introduction, you may find a detailed answer to every Reviewer#1's comment. All of the listed changes and the ones suggested by the reviewers can be seen in the new version of the manuscript, marked in red. Additionally, a few more references have been added to the manuscript.

Reviewer 1

1) *There are many ESA internal references which seem to be not publicly available. All reference must be available somehow. You might discuss with ESA to submit these documents as supplement or ask if they can make it publicly available. I just googled some of them, an many documents can be downloaded, like for example the "Aeolus-Sensor-and-Product-Description ", which is freely available here:*
https://earth.esa.int/eogateway/documents/20142/37627/Aeolus-Sensor-and-Product-Description.pdf
Please add links to all ESA documents and or make them available to the reader in another way.

Following reviewer#1's request, the following links have been added to the manuscript's references section:
- European Space Agency, ESA: ADM-Aeolus Science Report, ESA SP-1311, https://earth.esa.int/pi/esa?id=3409&sideExpandedNavigationBoxId=Aos&cmd=image&topSelectedNavigationNodeId=AOS&targetIFramePage=/web/guest/pi-community/apply-for-data/ao-s&ts=1496439496255&type=file&colorTheme=03&sideNavigationType=AO&table=aotarget (last accessed on 21/11/2021) 2008.
- Flamant, P. H., Lever, V., Martinet, P., Flament, T., Cuesta, J., Dabas, A., Olivier, M., and Huber, D.: ADM-Aeolus L2A Algorithm Theoretical Baseline Document Particle spin-off products, ESA, reference: AE-TN-IPSL-GS-001, https://earth.esa.int/eogateway/documents/20142/37627/Aeolus-L2A-Algorithm-Theoretical-Baseline-Document (last accessed on 21/11/2021), 2020.
- Ingmann, P., and Straume, A. G.: ADM-Aeolus Mission Requirements Document, ESA, reference: AE-RP-ESA-SY-001 EOP-SM/2047, https://earth.esa.int/eogateway/documents/20142/1564626/Aeolus-Mission-Requirements.pdf (last accessed on 21/11/2021), 2016.
- Reitebuch, O., Huber D., and Nikolaus, I.: ADM-Aeolus Algorithm Theoretical Basis Document ATBD Level 1B Products, https://earth.esa.int/eogateway/documents/20142/37627/Aeolus-L1B-Algorithm-ATBD.pdf (last accessed on 21/11/2021), 2018.

- Straume, A. G., Schuettemeyer, D., Von Bismarck, J., Kanitz, T., and Fehr, T.: Aeolus Scientific Calibration and Validation Implementation Plan, https://earth.esa.int/eogateway/documents/20142/1564626/Aeolus-Scientific-CAL-VAL-Implementation-Plan.pdf (last accessed on 21/11/2021), 2019.

2) *With respect to the topic above, would like to mention that a description of the L2A retrievals is currently in discussion in the same special issue. Even though not yet accepted, it might be worth to cite:*
https://amt.copernicus.org/preprints/amt-2021-181/

Following reviewer#1's comment, the mentioned reference has been added to and cited in the manuscript.

3) *I am still struggling with the quantity you named BIAS. For me, a bias is a systematic error. But what you calculate is the Difference between ground-based lidar and Aeolus for some cases. As the reason of these differences could be also due to atmospheric nature (horizontal heterogeneity etc), I would prefer not to use this wording. Thus, I recommend to rename this parameter simply to (absolute) difference or something similar, but not bias, please.*

As the reviewer#1 points out, the use of bias for the quantity computed in this work is not rigorous. Strictly speaking, the term bias of an estimator in statistics refers to the difference between the mean value and the numerical value of the parameter it estimates. Therefore, we agree with the reviewer that the use of bias in the previous version of the manuscript is somewhat ambiguous leading to a misunderstanding, and therefore we propose to replace it by 'difference (Δ)' through the manuscript.

4) *In my opinion, Figure 15 is not useful. It is known, that Aeolus uncertainty estimates (even though not perfect yet) handle only statistical uncertainties. Thus, it is not meaningful to compare them to the absolute differences (BIAS) derived from the comparison.*

Following reviewer#1 comment, former Figure 15 and the associated discussion has been removed from the manuscript.

5) *Reviewer 1, comment 2:*
R1: "…It is not very clear why the authors prefer to use the literature values instead of spectral conversion factor retrieved from measurements."
Authors: "…Considering all these arguments, we preferred to use the spectral conversion factor derived from the literature in order to base our results on measurements of different stations (i.e. different environmental conditions and aerosol types), which themselves have been reviewed through peer review process."
Me: I understand your arguments and can agree on your conclusions, but why to show "your own" conversion factors then, if they are not used? This is confusing and has nothing to do with the topic! Please always have in mind: "Illustrations should only be shown if they are necessary for the understanding of the paper, not because they have been created. " Therefore, I recommend to remove Fig. 2a and b, and only show Fig. 2c. The comparison of different depolarization ratio at different wavelength is not the topic of your paper but could be followed in another study.

Following this reviewer#1 comment and in order to make Section 4.1 easier to understand, we have decided to remove Figures 2b and 2c. We want to make it clear that we have used the literature-derived spectral conversion factor $K_\delta = 0.82 \pm 0.02$ from Figure 2a (now renamed as Figure 2).

6) *Reviewer 1, comment 4:*
   "In section 4.3 Case studies, the authors stated "Sun -photometer measurements are taken into account for the sake of completeness aerosol typing, through the study of the aerosol optical depth at 675 nm (AOD 675)". Why was the 657 nm wavelength chosen? Why not choose the AOD values in the UV region as 340 or 380 nm, instead?"
   Me: In my opinion your response is not sufficient. As reviewer 1 noted, you could have used a wavelength much closer to Aeolus (440 nm). And I don't think that 675 nm is most used in the community and is not the closed one to the wavelengths of your lidars. So I also do not see the point why to use that wavelength. Anyhow, as the AOD is only of minor importance for your work, it is not crucial, but you might consider to change this.

Following reviewer#1's suggestion, we have replaced AOD675 values with AOD440 ones in the discussion of the Sun-photometer measurements, which is closer to the Aeolus wavelength and still with low uncertainty (~0.01) (Eck et al., 1999) .

7) *Reviewer 1, comments 5:*
   Authors:" Regarding Barcelona station location just in the coastline, as the satellite overpasses the station at a close distance we can assume that both instruments (ground-based and space-borne lidars) detect the same air masses. Consequently, both instruments will register the same effects that the geographical layout might produce, so no special considerations have to be taken into account in the statistical analysis."
   Again I think your response is not sufficient. In my opinion you cannot assume that "both instruments detect the same airmasses" given the complex location of Barcelona and the very coarse resolution of Aeolus with 8 km in the horizontal. Rather, I would assume, that the coarse horizontal resolution of Aeolus and the horizontal heterogeneity in this area is one of the main reasons for the discrepancies found. Therefore, I think, you should discuss this issue in the paper with some sentences. Then, your newly added sentences in the conclusion would also fit much better, as it holds not only for the vertical but also for the horizontal domain.

We agree with reviewer#1 and therefore we have decided to include the following sentence at the end of the Conclusions section:

"However, as it can be noted from the results, Aeolus vertical and horizontal resolution is too coarse (especially compared to other satellites) for a detailed characterization of the nuances of the atmospheric optical properties, especially in regions such as Barcelona, where the coarse horizontal resolution might cover very different orographic features, from a mountainous system to the Mediterranean sea. Thus, Aeolus provides valuable information in the detection and characterization of significant aerosol and cloud layers".

8) *Reviewer 1, comment 9:*
   "9) Page 15 - lines 474-475 - "With the implementation of the quality flags (Figure 12c and 12 d), all of the sets range from 0 Mm-1sr -1 .". Please, consider correct this sentence. All the sets range from 0 to which value?
   Authors: The quoted sentence has been restated and completed as: With the implementation of the quality flags (Figure 13c and 13d), all of the sets range from 0 Mm-1sr-1 onwards. Actually, the maximum values mentioned are still flagged as valid, 86 Mm-1 sr-1 and 68 Mm-1 sr-1 in the case of the SCA and SCAmb, respectively."

> *Me: the sentence is still awkward. I guess what you simply mean is that with the implementation of the QA/QC flags, no negative values are existent anymore?*

Following this reviewer#1 comment the mentioned sentence has been stated as:

"With the implementation of the quality flags (Figures 13c and 13d) any of the sets present negative backscatter coefficients".

> 9) *Reviewer 1, comment 10:*
> *Authors: "Aeolus backscatter coefficient uncertainties (known as Aeolus error estimates) are addressed through the biases between satellite and ground-based measurements (as presented in Section 3.3)."*
> *It is known, that the Aeolus uncertainties (even though not full developed) handle only statistical errors. So how can this be addressed through "your" BIAS?--> makes Fig. 15 obsolete.*

We agree with this reviewer#1 comment. Because of the current status of the Aeolus uncertainties representing only the statistical errors, Figure 15 has been removed in the new version of the manuscript.

> 10) *Line 428: Why are values up to 85 Mm^-1 sr^-1 unrealistic? Please explain or give reference!*

Following this reviewer#1's comment we have included some references to indicate that a backscatter coefficient of 86 Mm^{-1} sr^{-1} can be considered unrealistic. Consequently, the next sentences has been included in the manuscript:

"The mentioned large values (86 Mm^{-1} sr^{-1} in the case of the SCA and 68 Mm^{-1} sr^{-1} in the case of the SCAmb) can be considered as unrealistic when compared to particle backscatter coefficients detected during extreme events in the Iberian Peninsula (e.g. Guerrero-Rascado et al., 2009; Preißler et al., 2011; Cazorla et al., 2017; Fernández et al., 2019)".

> 11) *Line558: I would use "Nevertheless" or "Even though" instead of "Thus"*

Done.

> 12) *Line 935: Is the WMO report available somewhere? DOI? Internet page?*

Following this comment of reviewer#1, the web link of the mentioned report has been added to its reference:

"World Meteorological Organization, WMO: Proceedings of the third WMO Workshop on the impact of various observing systems on numerical weather prediction, WMO, https://library.wmo.int/doc_num.php?explnum_id=5409 (last access on 21/11/2021), 2004".

> 13) *Table 2 caption: Please write at least in the caption the full name of delta_par_linear. For example: "The linear particle depolarization ratio delta_par_linear at 355 nm…." And please indicate the UNIT, so that one at least knows that the unit is not [%].*

According to this comment from reviewer#1, the full name and the units of the variable has been written in the caption of Table 2 as follows:

"Table 2. Linear particle depolarization ratios (δ_{linear}^{part}) at 355 and 532 nm (with the corresponding standard deviation) obtained from the literature for dust, marine and mixed anthropogenic aerosol types. It should be notice that δ_{linear}^{part} is a dimensionless variable".

14) Fig. 1, Caption: in spring 2021, the orbit has changed, can you indicate that here: E.g. "Distribution of Aeolus overpasses during the studying period from .. too..."

Following this reviewer#1 request we have specified the considered period in Figure 1 as:

"Figure 1. (a) Distribution of Aeolus overpasses over Europe during the considered period (B10, from July 2019 to December 2019 and from 20th April 2020 to 6th October 2020). (b) Location of the stations in Évora, Granada and Barcelona and the associated overpasses during the case studies analyzed in Section 4.2.. Source: ESA Aeolus online dissemination (aeolus-ds.eo.esa.int)."

15) Figure 2b Caption: Please indicate were the measurements were taken.

According to this reviewer#1 comment the region where the measurements were taken is now included in Table 2.

16) Figure 3. Caption: Please remove "Daily"

Following reviewer#1 comment, the word "daily" has been omitted from the caption of Figure 3, and for consistency also from Figures 6 and 9 captions.

17) Fig 13 caption: Caption does not describe what is seen. Please write: Frequency disruption plot of ... derived from.... Also, please make the caption more concise.

This reviewer#1 suggestion is taken into account and the caption of Figure 13 has been restated as:

"Figure 13. Histograms for the dataset, considering the three stations, of (a) Aeolus SCA co-polar backscatter coefficient without the implementation of quality flags; (b) Aeolus SCAmb co-polar backscatter coefficient without the implementation of quality flags. (c) is the same as (a) but considering quality flags. (d) is the same as (b) but considering quality flags".

18) Fig. 14, caption: What is meant with "combined database"? Please write more exactly.

This reviewer#1 suggestion is taken into account and the caption of Figure 14 has been restated as:

"Figure 14. (a) $\beta^{part}_{Aeolus\ like,355}$ and $\beta_{Aeolus\ SCA}$ of the dataset, considering the three stations, with no quality flags applied. (b) $\beta^{part}_{Aeolus\ like,355}$ and $\beta_{Aeolus\ SCAmb}$ of the dataset, considering the three stations, with no quality flags applied. (c) is the same as (a) but considering quality flags. (d) is the same as (b) but considering quality flags. The values of each dataset have been adjusted to a linear model with null intercept (red line)".

19) Figure 15: See comment on Bias and Aeolus uncertainty above. In my opinion these plots are not useful.

Following this comment and previous comments of reviewer#1, former Figure 15 and its related discussion has been omitted from the manuscript.

20) Caption Fig. 16: Replace measures by measurements.

Done. For the sake of consistency the same change has been made to Figures 15, 16 and 17 (former Figures 16, 17 and 18).

References

Eck, T. F., Holben, B. N., Reid, J. S., Dubovik, O., Smirnov, A., O'Neill, N. T., Slutsker, I., and Kinne, S.: Wavelength dependence of the optical depth of biomass burning, urban, and desert dust aerosols, J. Geophys. Res., 104(D24), 31333– 31349, https://doi.org/10.1029/1999JD900923, 1999.