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Comment on amt-2025-462

Anonymous Referee #1

Referee comment on "Cross validations of the Aeolus aerosol products and new developments with airborne high spectral resolution lidar measurements above the Tropical Atlantic during JATAC " by Dimitri Trapon et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/egusphere-2025-462-RC1, 2025

A Review of "Cross validations of the Aeolus aerosol products and new developments with airborne high spectral resolution lidar measurements above the Tropical Atlantic during JATAC" by Dimitri Trapon et al.

This paper provides the most robust validation efforts of the Aeolus aerosol data products to date. The paper is well written, clear, and provides results that are important for the interpretation of Aeolus aerosol data products. It deserves to be published after a few minor revisions that I believe will strengthen the paper.

My 4 main comments are:

1) Importance of Aeolus aerosol products: The lidar and wind communities view Aeolus as a wind lidar first and foremost (as they should). The aerosol community typically uses CALIOP for aerosol lidar data. Why should either community use the Aeolus aerosol data products? Some in the aerosol community may be hesitant to use Aeolus products due to the coarse resolutions and low sensitivity to faint aerosols. However, there are good reasons to use these products. As one example, simultaneous aerosol and wind lidar profiles can inform aerosol transport/advection. This is especially true for dust in the SAL. I recommend the authors address this early in the Introduction with a dedicated paragraph.
The value of Aeolus aerosol profiling comes with the ability to separate the contributions from molecules and particles contrary to elastic backscatter lidars such as CALIOP/CALIPSO. Aeolus therefore measures both the particles backscatter and extinction coefficients independently, leading to no ambiguities in aerosol and clouds optical properties, and without using a-priori hypothesis on extinction-to-backscatter ratio. We emphasised this point in the introduction, and we added a sentence about the Aeolus capacity to provide simultaneous aerosol and wind lidar profiles.

2) Converting HALO data to 355 nm: This is probably my biggest concern reading the paper, given it is so important to the entire results section. I understand why the authors used the median backscatter and extinction Angstrom exponents from PollyXT as they are the most applicable. However, the statistics reported in Section 4.1 have a very wide range. This made me wonder several things: (1) what do the histograms of these Angstrom exponents look like? (2)

how sensitive is the 355 nm conversion to these Angstrom exponents? (3) Would there be less range/variability if the authors only used the PollyXT data from those specific dates to convert the HALO from 532 to 355 nm? Then when I look at Figures 4a and 4b, I see that there is much better agreement between the PollyXT and MLEsub backscatter and extinction profiles than the HALO 355 nm and MLEsub, especially between 2.5 and 3.5 km in the backscatter. I highly recommend the following: (1) Instead of using the entire month of September for the PollyXT Angstrom exponent calculations, try using just that specific day or +/- 1-2 days around that day (if you need more data). See if that reduces the range/variability of the values. (2) Provide the histograms of the Angstrom exponents for whatever set of PollyXT data you use for the conversion in an Appendix. (3) Add error bars to the yellow HALO 355 nm profiles in Figures 4a/b (and Figures A4 and A5), similar to what you have for the purple MLEsub profiles, that show use the 25th and 75th percentiles. (4) Add some discussion in Section 5.2 about what these uncertainties in the HALO 355 nm conversion mean for the cross-section comparisons. Note: the MLEsub and PollyXT 355 nm data show very good agreement (Fig 4, A4, A5), especially for the backscatter. This is a should give users confidence in using the Aeolus MLE aerosol data!

The Angstrom exponents' median values of the monthly profiles were initially selected as the number of valid profiles per day vary, and because it can be considered well representative for the conditions observed for September 2022, i.e., with occurrence of Saharan Air Layer (SAL) in the troposphere and the close-to-zero reported values being in accordance with the literature for mineral dust.

As recommended, the Angstrom exponents' median values of the daily profiles for 09, 15 and 16 September 2022 have been derived from the PollyXT measurements and used to reprocess the 532 nm to 355 nm conversion of the HALO atmospheric products. A new co-author who derived the Angstrom exponents values has been added (i.e., Moritz Haarig from TROPOS). This results in a much better agreement between the MLEsub and HALO parallel 355 nm backscatter profiles in Figs. 4b below, A4b and A5b; the HALO and PollyXT parallel 355 nm backscatter profiles being now really close (i.e., offset less than ≈ 0.1 $Mm^{-1}sr^{-1}$ for high aerosol loads and inner SAL around 3 km altitude on 16 September 2022). One should also note a better agreement in the Marine Boundary Layer (MBL). It has to be noted that the use of Angstrom exponents daily median instead of monthly median results in a minor impact onto the metrics for the inter-comparison between Aeolus and HALO converted and re-gridded products (e.g., decrease of 0.01 for RMSE with MLEsub backscatter coefficient on 09 September 2022).

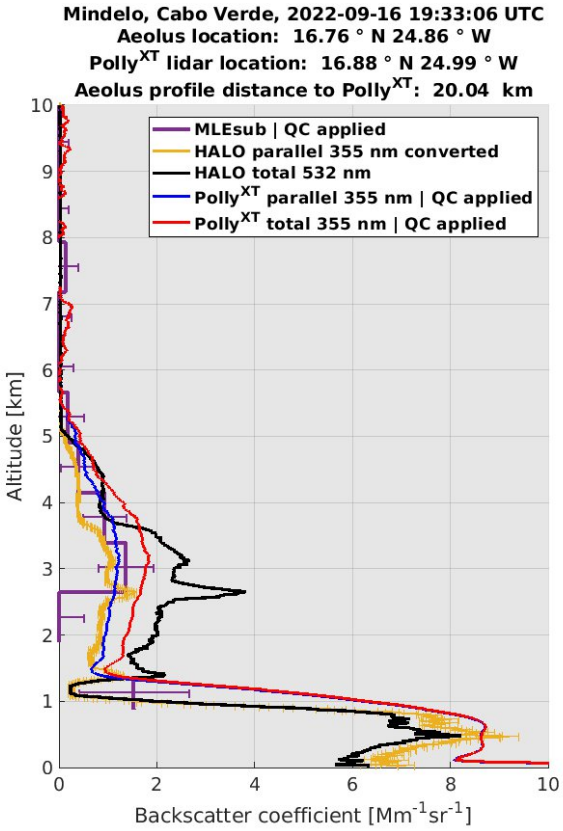

Figure 4b. Parallel backscatter coefficient for particles at 355 nm derived with Aeolus MLEsub algorithm (i.e., violet), PollyXT (i.e., blue) and HALO (i.e., dark yellow) on 16 September 2022.

The section 4.1 and the Figs. 4b-c, A4b-c, A5b-c have been revised accordingly. The Table 1 as the Figs. 5, 6, 7, 8, 9, 10, A4, A5, A6, A7, A8 have also been updated.
The errors bars have been added to the yellow HALO 355 profiles in Figs. 4b-c, A4b-c and A5b-c. A statement introducing the uncertainties induced by the wavelength conversion for HALO and the deviations per Aeolus range bin via the re-gridding has been added in section 5.1.

It has to be mentioned that the statistical metrics presented in the pre-print correspond to the Pearson Correlation Coefficient (PCC) and the unsystematic Root Mean Square Error (RMSEu) instead of R^2 and RMSE. Nevertheless, as we think that the use of R^2 and RMSE is preferable (i.e., the focus here being more the assessment of the deviations between HALO and Aeolus using RMSE than the characterization of the dispersion from the regression line with RMSEu, and the R^2 being often used as complement) the metrics have been corrected. This does not invalidate the observations and conclusions.

3) Cloud contamination in aerosol retrievals: On lines 248-249, you say “Invalid HALO measurements that correspond to localized conditions, e.g., below dense clouds or within the PBL, and reported as NaN values in the ready-to-use products are not taken into account.” Does this just mean those bins aren’t included in the re-gridding? What about scenes with thin cumulus clouds? How are conditions handled when half of the HALO obs in an Aeolus bin are NaN or in thin/small cloud and half are not (in a dust plume)? For example, how are the clouds (red dots) on the left side of the high-resolution HALO backscatter curtain (Figure 5, top left)

handled when re-gridding? Please add some text that describes how these situations are handed and describe the consequences of including high resolution cloud bins in coarse resolution re-gridded data.
The HALO bins with NaN values are not included in the re-gridding, hence a gap filling below dense clouds (e.g., Fig. 9a). One could expect a less representative cross-comparison between HALO and Aeolus for such regions if compared to fully valid bin. This statement has been added to the paragraph in section 4.2. The re-gridding with scattered or broken clouds in regions highlighted with HALO cloud_top_height product in Figs. 5, 7 and 9 results in averaging dilution for cloud echos as the Aeolus range bin may encompass surrounding lower backscatter signal (e.g., Fig. 3b at altitude 6 km and HALO profiles 86 to 114). The averaging dilution is expected to be more pronounced with thicker range bin and with coarser resolution at observation level (i.e., Basic Repeat Cycle BRC). The authors then do recommend to use the Aeolus measurement level (i.e., sub-BRC) products when studying clouds. The point has been stressed into section 5.2.1 and within the conclusion.

4) Conclusion & relating to Aeolus 2: There are a few things that could be added or reworded to really help Aeolus aerosol data users and drive home the needed capabilities of the Aeolus-2 follow-on.

a. Lines 396-400: The sentence that starts “The study reveals that the agreement between the Aeolus and reference measurements can be improved when applying scatter ratio threshold…” This is a key finding of the study. Can you be more explicit to Aeolus data users as to how to filter the data to get the highest quality (only use backscatter values above X.X and scattering ratios below X.X) ?
The highest quality of Aeolus L2A data is obtained when applying the pre-defined Quality Check (QC) flags based on Signal-to-Noise Ratio (SNR) and error estimates as described in the L2A User Guide v2.2. When focusing on high aerosol load only, the use of a scattering ratio threshold may help to discard the background noise and the cloud contribution. The corresponding sentence has been modified.
The recommendation about the lower limit for the backscatter detection is given in the last paragraph of the conclusion.

b. Lines 415-417: The sentence that starts with “The present study provides further evidence of HSRL benefits for aerosol atmospheric profiling…” should be rewritten. It comes across to the reader as if Aeolus demonstrates HSRL capabilities for aerosol typing but given the results of this study and the lack of polarization, I don’t think that is what the authors are trying to say. I suggest changing this sentence to something like “The present study demonstrates the independent HSRL measurement of aerosol properties such as particle extinction and backscatter coefficients, even for missions initially designed for winds such as Aeolus. The collocation of the Aeolus data with the HALO polarization measurements demonstrates the aerosol typing capabilities and provides further evidence of the need for Aeolus-2 to have polarization capabilities.”
CORRECTED

c. Lines 427-429: The sentence “Vertical sampling is a limitation to bear in mind for new missions such as EarthCARE and Aeolus-2…”. I think this is another key takeaway from this paper and something that should be considered for Aeolus-2. The comments raised in #3 (cloud

contamination in aerosol retrievals) are a great example of the importance of the higher resolutions.
This takeaway message has been strengthened in parallel to the additions for comment 3).

d. I think this paper can provide great examples of why polarization capabilities and finer resolutions are needed for Aeolus 2. I highly recommend a sentence at the beginning of last paragraph of the conclusion that explicitly states
This takeway message has been strengthened in parallel to the additions for comments 3) and 4)c.

Other minor issues to be addressed are:

1) That vs which: The word “which” should be changed to “that” in lines 11, 367, 376
CORRECTED

2) Allows to: There are several places where the authors use the phrase “allows to”, which reads awkwardly to me. I suggest change this to “enables” or “allows for estimates of”. This occurs in lines 15, 44, 53, 103, 253, 264, 430
CORRECTED

3) Line 35: MLE acronym is used but not defined in the line above.
CORRECTED

4) Lines 59. Delete “It is also important to point that” as it is unnecessary.
CORRECTED

5) Line 111: It says “Baseline 16 does not include QC flags for SCA particulate attenuated backscatter which then contains non-physical values (i.e., negative) because of non-perfect cross-talk correction. It was decided to flag the negative values for the study”. I recommend adding some discussion as to what it means to flag this data and instructions for potential data users. It would be even better if you could provide a figure in an Appendix that demonstrates the importance of flagging these negative values.
Negative values for SCA particulate attenuated backscatter (up to ~ 40 %) are distributed in low SNR regions with aerosol-free conditions. The information has been added to the paragraph in section 2.1.

6) Line 120: HALO has already been spelled out and the acronym defined, so I would use the acronym here. The same thing for line 139.
CORRECTED

7) Figure 2: The text in the key isn't readable (too small). Are the Aeolus overpasses/tracks exactly the same every Thurs, Fri, and Sat? If not, please provide the exact dates of interest. If no Saturday overpasses are used in the paper, I recommend removing it from the figure.
The Aeolus overpasses selected for the study only correspond to Thursday (i.e., 15 September 2022) and Friday (i.e., 09 and 16 September 2022). The Saturday overpasses (i.e., Fig. 4c) have then been removed from the figure 4.

8) Line 128: CPEX-CV has already been spelled out and the acronym defined, so I would use the acronym here.
CORRECTED

9) Line 140: There should be a comma before which. Same thing for line 145, 178, 296, 307, 332, 430
CORRECTED

10) Line 181: The phrase “It is proposed” makes it sound like you are asking to do the conversion this way. I suggest changing this phrase to “It is necessary”
CORRECTED

11) Line 217: The authors say “This constant is added to the αmol,355 in Eq. (11)”. Do they mean that the constant has been added to the equation or do they mean that value was used for computing αmol,355? The way this is stated is confusing to me.
The constant has been added to the αmol,355 derived in Eq. (11) when calculating the HALO 355 nm attenuated backscatter coefficient for particles in Eq. (12). The sentence has been corrected.

12) Line 232: The phrase “from an over-sampled ≈ 15 m resolution” refers to the vertical resolution, correct? If so, please add the word “vertical” before resolution.
Yes, CORRECTED

13) Line 259: The authors say “Figure 4a illustrates how the Aeolus MLEsub βpart up to 18 km altitude helps assessing the particle-free conditions above the DC-8 flying at ≈ 10.7 km altitude for the 16 September 2022 case.” I don't understand how Figure 4a illustrates this? I don't see any backscatter signal that looks like a layer above 10 km. Maybe you mean to say that the Aeolus MLEsub βpart up to 18 km altitude confirms the assumption of particle-free conditions above the DC-8?
The Aeolus MLEsub βpart up to 18 km altitude in Figs. 4a, A4a, A5a confirm the particle-free conditions above the DC-8 as no backscatter signal corresponding to aerosol layers or clouds can be observed above 10 km altitude (i.e., with close-to-zero values in dark blue color code). The sentence has been corrected.

14) Line 266: the sentence “The HALO parallel 355 nm profile agrees with Aeolus MLEsub (i.e., violet).” Is too vague. The 2 profiles agree above 4 km, but below 4 km the HALO backscatter and extinction is 25-50% higher by my eye.
CORRECTED

15) Line 281: The authors use the phrase “look solid”. I suggest a more formal phrase, such as “are robust”. The word solid is used also in line 337 and 390
CORRECTED

16) Lines 390-391: I don’t consider these scenes to be heterogeneous, especially considering profiles with clouds have been screened from the statistics analysis shown in Figures 5-10. I recommend changing the phrase “a solid agreement between the aerosol retrievals is shown for heterogeneous scenes with complex atmospheric conditions” to something like “good agreement

between the aerosol retrievals is shown for homogeneous, optically thick aerosol layers.”
CORRECTED