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 Reviewer#1's comments. All of the changes can be seen in the new version of the manuscript, marked in red.

Reviewer#1

General comments

1) *However, the language is not well phrased, which makes the analysis and results very difficult to understand. A large portion of the text therefore needs to be rewritten to be more precise and clear. Also, some further analysis is needed to strengthen the scientific analysis and interpretation of the results. The reviewer therefore recommends that the paper is only accepted upon revisions, according to the comments provided here.*

   The authors understand the issue raised by Reviewer#1. Thus, the text has been accordingly rewritten in order to improve its comprehension. Additionally, further discussion of the results was performed to ease their interpretation. The Reviewer#1 valuable suggestions have been implemented and a more detailed description can be found in the following comments.

Specific Comments

2) *The manuscript has many acronyms and Aeolus specific abbreviations and expressions are used early in the paper (including in the abstract) without being fully explained or spelled out. This makes in particular the abstract but also the early sections difficult to read. This would need to be revised and improved.*

   Following this Reviewer#1 comment, a deep inspection of acronyms and abbreviations were performed and proper clarifications were included throughout the manuscript. Additionally, some acronyms were omitted and spelt out instead, when the use of the acronym was not needed. Furthermore, the abstract acronyms and abbreviations were rewritten and spelt out, so that only key acronyms are kept.

3) *Due to the lack of detail in how the data collocations were calculated, it is not possible to judge whether the 100 km collocation criteria was applied to (i) the horizontal distance between the compared observations or (ii) the distance between the satellite ground track and the ground-based station. If the latter is the case, the collocation criteria has not been applied correctly since the Aeolus wind observations are done at a slant angle of 35 degrees with specialrespect to the satellite nadir direction, and hence the distance between the measurement track and the ground track cannot be ignored especially at the lower altitude levels. The look direction is also different for ascending and descending orbits, which means that the measurement track will be either East or West to the satellite ground track. The authors must therefore describe how this has been done*

and confirm that the measurement distance of 100 km has been applied in the statistical comparison.

The authors appreciate this comment, as it highlights that this important information was not clear. The coordinates of each independent observation have been used to determine if they lie or not within the different collocation criteria. Thus, the existing incident angle has been considered and, consequently, each observation presents different coordinates. In this study, the satellite ground track was only used to calculate the minimum distance between the satellite ground track and the station (which is a widely used indicator in the Cal/Val community) and to plot the ground tracks of former Figure 1 (according to comment 23 Figure 1 was removed from the manuscript). To solve this misunderstanding for future readers, the following sentence was included in line 179:

> "Regarding the horizontal spatial collocation of the measurements, the coordinates of each Aeolus observation (incidence angle of the laser beam considered) were used in order to determine if the different spatial collocation criteria were fulfilled. Thus, the orbit ground track was not used to apply these criteria."

Additionally, line 268 was rephrased as:

> "Considering Aeolus observations within a 100 km distance to the station, …"

4) *There is some confusions created due to an incorrect use of the Aeolus terminology measurement, observations and bin in several places. Throughout the paper, the word "bin" should be replaced with "observation" for the Aeolus wind data. An Aeolus (vertical) bin refers to an altitude level along the instrument line of sight over which the received atmospheric backscatter has been integrated. The altitude of a bin with a given number varies according to the vertical sampling scenario applied to the instrument for different latitudinal regions, and is also not the same for the Rayleigh and Mie winds. This is illustrated in several of the references Aeolus papers.*

We appreciate this comment and agree there is an important misunderstanding. For the sake of clarification, the term 'bin' has been replaced by 'observation' throughout the manuscript.

5) *In several places in the paper it is written that the Aeolus wind data have a larger or smaller error, based on the statistical comparison with the ground-based or radiosonde data which are defined as the truth. However, this should be rephrased to state that the Aeolus observations differ to a larger or smaller extent with the ground based data, and that this can be due to 1) errors in the Aeolus observations, 2) errors due to different horizontal representativity of the Aeolus (observation data representing ~87 km horizontally integrated measurements and 1 to 2 km vertically integrated signals) and ground-based or radiosonde (point observations) datasets, 3) horizontal variability in the wind field between the location of the Aeolus observations and the location of the groundbased observations (up 10 100 km). This mut be clarified throughout the manuscript. The authors refer to the errors of the ground-based lidar data and radiosondes in the text, but it would be better to also include the information in the figure capture where the datasets have been compared. As it is now, only the estimated errors of the Aeolus data are reported in the figures.*

We appreciate this suggestion by Reviewer#1, as it highlights an important discussion that was missing in the former version of the manuscript. Thus, we have included the following paragraph at the beginning of Section 3.2, in line 193:

> "Aeolus observations are compared to the analogous measurements from upwards probing instrumentation, which are considered as the ground truth for the comparison. The possible existing discrepancies could be due to (i) errors of Aeolus observations, (ii) errors associated to the representativity of Aeolus observations along the horizontal signal accumulation, (iii) errors associated to the representativity of Aeolus observations in the bin's thickness signal accumulation and (iv) the differences in the wind fields between Aeolus observations and the ground truth caused

by the wind field horizontal variability. However, due to the temporal coverage considered (with different meteorological conditions) and the statistical methods used, issues (ii), (iii) and (iv) are expected to be minimal and the differences detected in Section 4 are mainly associated with Aeolus biases."

On the other hand, no comments were made on the ground-based Doppler lidar data errors, while the comments made on the radiosondes' accuracy were based on estimations of the radiosonde model and not on exact and constant values. Moreover, these errors are so little that given the scale of the figures the error bars would not be distinguished at all, while Aeolus errors would make the interpretation of the figures more difficult. Additionally, Aeolus error estimates are not presented in the figures as error bars, but a full exploration of their values is performed. Then, the authors consider that including the ground-based Doppler lidar or radiosonde uncertainties to the presented Figures will not provide valuable information.

6) *A lot of statistical analysis is done, mentioning the inhomogeneity of the wind field, and especially in the atmospheric boundary layer (ABL) due to terrain, local temperature gradients and ABL stability. However, no examples of the typical variability of the wind field in the study area is show. This should be done for some examples, e.g. by plotting wind fields at 1 km, 3 km, 5 km and 10 km altitude from a regional weather forecast model (e.g. 1-2 km horizontal measurement resolution) or the ECMWF model forecast field (about 10 km horizontal measurement resolution). This could be done for a homogeneous and highly inhomogeneous case and for timesteps representative of the ground-based and Aeolus data or Aeolus and radiosonde data. This would support the discussion about horizontal variabilities to be expected in the region.*

The authors appreciate this discussion from Reviewer#1. Definitely, it is an interesting point of view. However, the authors believe that this analysis is out of the scope of the present study. We intended to compare ground-based Doppler lidar and radiosonde measurements with Aeolus products within a distance criterion amply fulfilling ESA's requirements and recommendations. We believe that studying the wind field patterns in the region will entail an independent study itself (Ortiz-Amezcua et al., 2022). Additionally, as mentioned now in line 198, the authors expect that the effects introduced by the possible wind field differences are minimized due to the long temporal coverage considered and the statistical methodologies applied. Further discussion was made in comments 34 and 40.

Technical Corrections

7) *Start the abstract with a short explanation of what Aeolus is: An ESA mission carrying the first Doppler Wind Lidar in space, which is providing profiles of wind observations in the troposphere and lower stratosphere globally. Explain that it provides wind profiles from the lower stratosphere down to thick clouds or to the surface in cloud free conditions. Several wind profile products a are stored in the so-called level 2B product.*

Following this Reviewer#1's suggestion, the following sentence was included in line 10, at the beginning of the abstract:

"In 2018, the European Space Agency launched the first Doppler wind lidar system into space, providing wind observation profiles from the lower stratosphere down to the surface in two different channels based on the scene classification: cloudy or clear."

8) *Spell out and explain L2B wind product or drop L2B and write wind profile products.*

Any reference to "L2B" was omitted from the abstract, and "wind products" was used instead. Following this suggestion, "HLOS" was spelt out and the acronym was omitted too. We appreciate this suggestion, as we expect the abstract is now more intelligible for the readers.

9) *Line 12: Instead of writing that the data are from the period between the Aeolus public data release (i.e. May 2019 and not the reprocessed data releases which are taking place every 6-12 months) and the time of the orbit shift, write the exact time period (dates).*

The suggestion was taken into account accordingly and the sentence in line 13 was restated as:

> "The validation activities with the automatic ground-based lidar system lasted from July 2019 to the orbit shift of June 2021."

10) *Line 13: Propose to rephrase to "Aeolus data from two different on-ground data processing software baselines were validated, using a 100 km horizontal and 30 minutes temporal collocation criteria. This resulted in 109 collocations."*

The sentence in line 14 was rephrased as:

> "Aeolus data from two different processing baselines (10 and 11) were validated with 30-min averages of coincident ground-based lidar measurements, using a 100 km horizontal spatial collocation criteria. This resulted in 109 collocations and a mean observation distance from the ground-based lidar system of ~50 km."

11) *Line 14: What do you mean with "mean bin distance"? The authors probably mean the distance between the ground based or radiosonde wind observations and the Aeolus wind observations that were compared. Please rephrase.*

Following the aforementioned suggestion 4, the expression was rephrased as "mean observation distance". However, in order to make it clear, the sentence in line 16 was then rephrased as:

> "… and a mean observation distance from the ground-based lidar system of ~50 km."

12) *Line 15: Explain HLOS wind speed and Rayleigh clear and Mie cloudy.*

Following the aforementioned suggestions 2 and 8, the acronym HLOS was omitted from the abstract and was spelled out in line 17. Thus, it was omitted from line 26 too.

13) *Line 15: "… and equal over and underestimation of Aeolus wind speed" is not understandable. It does not precisely describe the main conclusions of your results. Do the authors mean that no constant or wind-speed dependent biases were found? Or do the authors mean that the results were not the same for different time periods, or for different collocation criteria?*

The authors understand the misunderstanding raised by this sentence. We meant to say that no constant biases were found. Overestimation and underestimation were found for different observations but without a specific pattern. However, in average terms these biases seemed to compensate, so that the mean difference is close to 0. For clarification, the sentence in line 17 was rephrased as:

> "The comparison did not raise any significant over- or underestimation of Aeolus horizontal line-of-sight wind speed during that period for the Rayleigh clear and Mie cloudy configurations."

14) *Line 16: Refrain from using "lowermost bins" in the abstract and write instead Aeolus observations close to the surface of just above the surface.*

Following the comment, the sentence in line 18 was rephrased as:

> "However, the ground-based lidar measurements were limited to the lower 3.5 km of the atmosphere and, consequently, the obtained results."

15) *Line 16: The sentence "However, the reliability of the results was constrained to …, due to the limited vertical coverage" This is not understandable and not correct English. I assume that the authors mean that the ground-based lidar observations were limited to the lowest 3 km of the atmosphere, which is only a fraction of the vertical extent and valid wind observations from*

Aeolus. Also, the Aeolus observation resolution is quite coarse in the lowest 3 km, and the horizontal and vertical variability of the wind field is large, which makes the comparison difficult. Rephrase to make that clear.

As in previous comment 14, this suggestion was taken into account and line 18 was rephrased as:

> "However, the ground-based lidar measurements were limited to the lower 3.5 km of the atmosphere and, consequently, the obtained results."

16) *Line 17: What do the authors mean with spin-off analysis? This is not clear. Explain what the authors mean by that, and if possible choose a more precise expression to make it clear.*

Spin-off refers to the comparison coming from the principal comparison. In this case, we wanted to address the short comparisons performed with different collocation criteria, which were also important to the principal comparison as they could be taken into account as support analyses. In this sense, "spin-off" is related to "secondary". However, in order to make the abstract simpler, "spin-off" was removed, and this change did not affect the meaning of the sentence. Thus, line 19 was rephrased as:

> "Multiple analyses were performed varying the criteria of maximum distance and the average period for the ground-based lidar measurements, …"

17) *Line 18: "… varying the maximum distance to consider an Aeolus bin into the comparison and the average of the ground-based lidar measurements, …" It is not understandable what the authors mean here. Do the authors mean that the authors performed an analysis to find the optimum collocation criteria to be used for the statistical analysis?*

We meant to say that we tested multiple criteria considering different maximum distances and also several averages for ground-based measurements in order to assess its impact on the statistical analysis. The ultimate goal of these tests was not to find the optimum criteria, but to confirm that the criteria chosen (which agrees with ESA's requirements) was the optimum. Thus, the sentence in line 19 was rephrased as:

> "Multiple analyses were performed varying the criteria of maximum distance and the average period for the ground-based lidar measurements, in order to confirm the reliability of the criteria considered."

18) *Line 19-20: Be explicit of the start and end date of the second dataset that is studies with yet another Aeolus data product baseline. Explain that the analysis was done separately because the collocation with the ground station changed because the groundrack of Aeolus was moved. Another reason is that the data from this period was processed with yet another on-ground data processing software baseline.*

We appreciate this suggestion, as we think it helps to make clear the two campaigns performed. Following this comment, the sentence in line 21 was rephrased as:

> "A separate study was performed with Aeolus products after the orbit shift (baseline 12) with different collocation criteria (mean observation distance of ~75 km, to the station), from July 2021 to May 2022."

19) *Line 23-34: What do the authors mean by "an approximately equal overestimation and underestimation."? Equal to what, and over and underestimation compared to what? This needs rephrasing to make it clear what the authors mean.*

This suggestion is directly related to suggestion 13, as the same issue was raised. Thus, line 24 was rephrased as:

"The radiosondes could provide full vertical coverage of Aeolus profiles (from surface up to ~20 km above sea level) and the comparison did not yield any significant over- or underestimation of the Rayleigh clear wind speed, while the Mie cloudy wind speed was significantly overestimated."

20) *Line 25: Again, refrain from using spin-off analysis. Be clear about how this analysis differs from the other analysis done.*

In this case, following the suggestion and similarly to suggestion 16, "spin-off" was replaced by "multiple" for the sake of clarity, and the sentence in line 26 was then rephrased as:

"Multiple analyses were performed in order to test…"

21) *Line 28: The authors motivate the study with the need of winds to study aerosols and clouds. This is a valid point, but is not at the core of this paper. It is also not the main objective of Aeolus. Therefore, this sentence can be move to the end of this paragraph.*

We agree that the main motivation of this study was not the study of aerosols and the sentence may distract the readers from the main topic. Thus, former lines 28-29 and part of line 30 (according to previous line numbers) were removed from the manuscript.

22) *Line 166: Please add that the recommended collocation criterion of 100 km is valid for large-scale wind field comparisons. For comparisons in areas where the wind variability is large, on scales lower than 100 km, more stringent criteria should be applied. This is especially true when comparing observations in the ABL and close to the surface where the wind field is strongly modulated by the local and regional terrain. This will also be investigated in this paper.*

The authors understand the point of view of Reviewer#1. Straume et al. (2019) stated the recommended 100 km collocation criteria to compare measurements with Aeolus wind products. However, the "large-scale wind field" is not specified (or at least the authors cannot derive it from the text). In any case, it seems reasonable that the lower the collocation criteria the better for the comparison, especially given the possible wind field differences. However, using the tests changing the maximum distance for comparison, this is not what was observed in our study.

The discussion raised by Reviewer#1 seems logic, and it has been included in line 168 as:

"However, this criterion will work smoothly in large-scale wind field comparisons, and might be too coarse in situations with large wind variability (e.g., in the boundary layer)."

23) *Line 169: Figure 1: From the description here, it is not clear whether the authors pick the observation collocations as a function of distance between the instrument ground track and the measurement station, of the measurement track on ground and the station, or the difference between the observations at the respective observation altitude. It is important that the authors pick one of the latter options, especially since the instrument looks to the east of the ground track on ascending orbits, and to the west on descending orbits. Please plot also the measurement track on ground in Figure 1 to illustrate this and mention that the distance to the ground track position decreases with the measurement altitude. Specify in all places throughout the document whether the distance between the Aeolus measurement track and the station, or the orbit ground track has been used.*

We appreciate this comment, as it highlights an issue that could mislead the reader. The observation collocations were set in all cases with the observation coordinates and not the satellite ground-track. However, former Figure 1 illustrated the satellite ground-track together with the 100 km collocation criteria. The authors believe this fact may mislead the readers, as the ground-track is presented with the collocation criteria that actually is used together with the observation coordinates. Thus, the authors decided to omit this figure, which was included just as an illustration of the changes induced by the orbit shift. We decided not to include the observation coordinates as they may change significantly from overpass to overpass.

On the other hand, the latter comment of this Reviewer#1 suggestion was already taken into account in the previous comment 3.

24) *Line 196: Please discuss that the Aeolus laser output energy was high at the start of the laser B operation in July 2019, and then steadily decreased over time, with the exception of the regular periods where the laser output energy was increased. This led to the random noise of the L2B wind product to steadily increase with time, for then to increase again a little bit when the laser output energy was adjusted upwards. This will influence the wind statistics w.r.t. random errors (the bias mainly changed when a new processor version was used). This is described in the papers by Michael Rennie. Please take this information into account for this analysis and discuss here how this is impacting the results for the data series which spanned 2-3 years. This has also been discussed by several authors who have submitted papers to the Aeolus special issues in AMT and the Q.J.R. Met. Soc.*

The authors believe that this information is highly appreciated and was missing in the manuscript. It is now included in line 118 as:

> "In June 2019, Aeolus operating laser changed from laser FM-A to laser FM-B. After the change, the laser output energy, directly related to the signal-to-noise ratio and subsequently to the products random noises, steadily decreased over time and was regularly readjusted (increased) in order to ensure the reliability of Aeolus operation. The switch from laser FM-A to FM-B was described by Lux et al. (2020a) and Rennie et al., (2021)."

Additionally, a comment regarding the impact in our study is presented in line 189.

> "Regarding the switch from laser FM-A to FM-B, the dataset considered in the study covers a period with just laser FM-B operation. Additionally, the initial filtering of the error estimates will ensure quality data not affected by the laser output energy."

25) *Line 221: What do the authors mean with "Both variable profiles presented the same variable vertical resolution …"? Do the authors mean that the Mie cloudy and Rayleigh clear wind profile products were sampled with the same vertical resolution? Make it clear that the Mie channel sampling differs from the Rayleigh sampling, and both may not have been the same for the whole period of the observations at all altitudes. This could impact the statistics. Please check and verify.*

No reference was made to the scene classification in Section 3.3. In the mentioned sentence, the authors mean that the $V_{HLOS_{Aeolus}}$ the φ_{Aeolus} profiles presented the same vertical resolution. This is now clarified in line 230 as:

> "Both the wind speed and the azimuth angle profiles presented the same variable vertical resolution mentioned in Section 2.1."

On the other hand, the sampling differences between the channels is now included in line 85 as:

> "Additionally, the Mie and Rayleigh bins height grid do not coincide and may shift along the orbit."

26) *Line 233-243: It is difficult to follow your explanation on how you have averaged the two datasets to compare them in vertical in steps of 0.5 km or 2 km. From the plots later in the paper, it shows that you have used the vertical averaging of 0.5 km in the ABL when comparing with the ground-based lidar, and 2 km when comparing with the radiosonde. Please specify that here, and explain what the vertical sampling of the Rayleigh observations were in the ABL, at higher atmospheric altitudes, and whether it remained the same. If it was larger than 0.5km, explain what that means for the statistical comparison done on 0.5 km scale.*

We appreciate this suggestion and take it into account to ease the understanding of the text. Considering the suggestion, the information has been rephrased as:

"In order to get the vertical distribution of this parameter, the mean Δ between Aeolus and the upwards probing instruments derived from the comparison was averaged in fixed vertical intervals of 0.5 and 2 km for the ground-based Doppler lidar and radiosondes, respectively. These vertical intervals should not be confused with the original Aeolus bin height grid described in Section 2.1. Thus, the mean Δ between Aeolus and the instruments of each vertical interval was calculated as:

$$\Delta(r) = \frac{1}{N} \sum \left(V_{HLOS_{Aeolus}}(z) - V_{HLOS_{UP}}(z) \right) \quad (4)$$

where r is the vertical interval of 0.5 or 2 km, z is the bin's altitude (z lies within r) and N is the number of bins whose z lies within r."

The authors do not understand the comment "Please specify that here, and explain what the vertical sampling of the Rayleigh observations were in the ABL, at higher atmospheric altitudes, and whether it remained the same". In Section 3.4 no mention of the scene classification was done, so we do not understand why the vertical sampling of the Rayleigh observations should be mentioned here. Additionally, the Aeolus vertical bin heights was specified in Section 2.1 ("...divided into 24 vertical bins with a variable vertical resolution of, nominally, 0.5 km between 0 and 2 km, 1 km between 2 and 16 km and 2 km between 16 and 30 km"). Thus, for a couple of overpasses it may happen that some 0.5 km bins do not contain any Aeolus observation. However, we expect that the previous rephrasing of the information helps to solve this suggestion.

27) *Line 237: The authors write that the Aeolus wind speed is over or underestimated. As mentioned above, this is not necessarily correct, since the difference between the two collocated datasets can be either due to the individual observation errors, spatial or temporal representativity errors. Especially, for comparisons in the ABL, differences could be expected to be dominated by spatial and temporal variability effects. Please clarify this here.*

This suggestion is related to suggestion 5. The authors agree with Reviewer#1 that individual differences between the datasets cannot directly be attributed to specific Aeolus biases. However, due to the use of long-term datasets we expect that individual differences are minimized and the obtained differences are attributed to Aeolus performance, as in previous satellite comparisons (e.g., Iwai et al., 2021; Kottayil et al., 2022; Ratynski et al., 2022; Wu et al., 2022).

28) *Line 262-265: It is not understood which statistics the authors are presenting here as mean HLOS wind speed values. Is this the average of all observations over all altitudes throughout the whole time period? If yes, what is the purpose and physical meaning? Please clarify or remove.*

The statistics refer to the average of all observations, as it is now specified in line 274 as:

"For all observations, the mean HLOS wind speed value (± SD) was…"

The authors understand that a specific physical meaning is lacking for this statistic. However, it was included to provide more information about the general performance of the satellite. These statistics are compared with and without the error estimate filtering in order to provide a general view about how the filtering affects the measurements.

29) *Line 273: Why do you think that a maximum wind speed of 77 m/s is not realistic? At which altitude was the wind detected? Please comment.*

The wind speed of 77 m/s is not described as unrealistic in line 285. Instead, it is described as a possible outlier but feasible wind. However, it is true that in previous sentences the word "unrealistic" is used, but when referring to a wind speed of 244 m/s. The authors understand that this may lead to misunderstanding, so it will be replaced with "improbable".

30) *Line 275: Discuss why the Mie error estimates are generally smaller than the Rayleigh ones, and refer to discussions provided in the cited papers by M. Rennie.*

We kindly appreciate this suggestion, as this interesting discussion was lacking in the former manuscript. The following sentence has been added to line 287:

> "On the other hand, the mean error estimates of the Mie cloudy configuration were significantly lower than those of the Rayleigh clear configuration. This can be explained with the larger signal-to-noise ratio of the backscattered signal coming from the clouds, which results in lower error estimates (Rennie et al., 2021)."

31) *Line 290: with "equally significant", do you mean statistically representative? Please explain.*

Indeed, we meant to say that both datasets present the same statistical representativeness. Thus, the sentence in line 312 has been rephrased as:

> "… so both datasets present the same statistical representativeness."

32) *Line 300-307: From Figure 3, it looks like the intercept for the Mie wind comparison was negative and not positive. Please check and correct if needed. Discuss that from both the comparisons to the Mie and Rayleigh winds, there seems to be possible wind-speed dependent biases which you report are statistically significant. Discuss whether this could be related to local wind condition effects. Note here that the other studies were performed at other geographical locations.*

Although it may look like the intercept is negative given the axis resolution, the real intercept is positive but close to 0 m/s. Additionally, the obtained value of (0.3 ± 0.4) m/s is indicated in the text in line 323.

On the other hand, the proposed discussion was now added to line 327, as:

> "Additionally, positive intercept values were obtained for both configurations. These values could be due to the effects of local geographical conditions or to existing biases in Aeolus measurements."

and to line 331 as:

> "It is worth mentioning that the mentioned studies by Iwai et al. (2021) and Wu et al. (2022) were performed in different locations with particular geographical features."

33) *Line 311: Contrary to what you write, the Mie cloudy HLOS wind speed bias is not 0 m/s as reported by von Bismack. Also, the results from Wu and Iwai differ. As explained in the papers by M. Rennie et al., the Aeolus telescope temperature bias correction method is tuned to minimize the Aeolus wind bias globally. This means that local biases can still be present in the data product. Please check this in the papers by Rennie et al, and discuss this accordingly.*

Regarding Reviewer#1 and Reviewer#2 (no downloadable document was available) comments, the authors decided to omit any reference to von Bismarck et al. (2022) from the manuscript.

On the other hand, regarding the telescope temperature bias correction, the following information was included in line 333:

> "Moreover, the telescope temperature bias correction (Weiler et al., 2021b) is tuned to minimize the global biases, but local biases can still be present and detected in individual validations (Rennie et al., 2021)."

34) *Line 315: Please add here some examples figures of typical wind variabilities at a few altitude levels for the study region, for two typical prevailing weather conditions. You can use for this either ECMWF model weather maps, or regional weather maps. Discuss the example weather maps and how the local topography impacts the wind variability in the study area at different altitudes and for different conditions. Discuss this as part of your analysis of the comparisons between Aeolus and the ground-based lidar. This should also be used for the discussion about the wind differences as a function of altitude in Figure 4. An alternative way could be to make a statistical comparison of the wind direction reported in the Aeolus L2C product (ECMWF model data at the location of the Aeolus observations) and the wind direction from your ground-based*

lidar. This would indicate whether the wind conditions are generally very different at the location of the Aeolus measurement track and the ground station.

The authors appreciate this discussion from Reviewer#1. Including this information and validating Aeolus L2C products would be an interesting supplement to the manuscript. However, the authors believe that this analysis is out of the scope of the present study and would be an appealing topic for another study. For example, Ortiz-Amezcua et al. (2022) performed an interesting analysis about the wind patterns over the city of Granada, and only this analysis took a lengthy manuscript. However, following the Reviewer#1's comment, the authors have performed the suggested statistical comparison for the available L2C wind direction for the coincident period of time for the ground-based Doppler lidar validation of Aeolus L2B products (Figure R1). The mean difference between the Aeolus L2C wind direction and the ground-based Doppler lidar wind direction was 3 ± 50º for both channels, while the mean absolute difference was 40 ± 30º for both channels. The 40º mean absolute difference indicates that there is a generally good agreement between the wind directions, and they can be assumed to come from the same quadrant and even from the same octant. Thus, the wind conditions could be assumed to be similar.

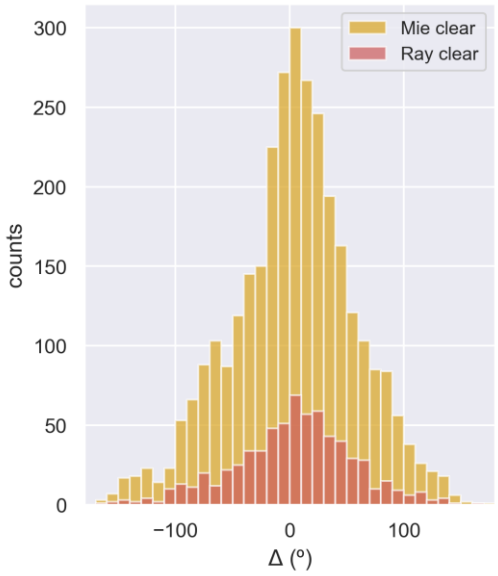

Figure R1. Histogram of the differences between Aeolus L2C wind direction and ground-based Doppler lidar wind direction.

35) *Line 334: As mentioned before, it is not possible to conclude whether the differences in wind speeds between the Aeolus and ground-based lidar observations are due to measurement errors only, or also due to different representativeness errors. Please add this here.*

The suggestion has been added to line 372 as:

> "However, it is worth mentioning that it is not possible to conclude whether the observed differences in the wind speed between Aeolus observations and the ground-based Doppler lidar measurements are due to measurement errors only or also due to multiple representativeness errors. Nevertheless, due to the significant temporal coverage of the study and the statistical methods applied, the different induced errors would be minimized and the differences could be attributed mainly to Aeolus biases."

36) *Line 335: The large RMSE for the Rayleigh winds at 0.5 km altitude are probably due to the low number of observations compared, the large horizontal averaging of the Rayleigh observations (87 km along-track averaging), and the large variability in the wind field close to the surface also within the 100 km radius (large representativeness errors). Furthermore, possibly imperfect removal of ground return contamination in the lowest Aeolus observation bins play a role. Please*

discuss this here. This is less of an issue for the Mie winds which are sampled at smaller horizontal and vertical scale.

We kindly appreciate this comment as it will significantly improve the discussion of the results. This discussion was missing in the manuscript and was added to line 368 as:

> "The significant discrepancies obtained for the lowermost vertical interval can be attributed to multiple effects such as (i) the lower number of observations considered, (ii) the horizontal averaging of the signals (specially for the Rayleigh observations), (iii) the large variability of the wind fields closer to the surface within the 100 km radius, (iv) the lower strength of the signal closer to the surface and (v) possible imperfect removal of the ground return contamination of the lowest observations."

37) *Line 352: Again, discuss the difference in Rayleigh wind representativity compared to Mie winds here while interpreting these results.*

This was included to line 387 as:

> "This can be due to the fact that the Mie winds are sampled at smaller horizontal and vertical scales and, consequently, with higher representativity."

38) *Line 365: Add here that your conclusion, that a 100 km collocation criteria is the best choice, is based on your dataset analysis for this station. Therefore, it would not necessarily be applicable to all geographical locations and local (weather) conditions for weather station across the world.*

We appreciate this suggestion, as we believe this information was missing. Following this suggestion, the following comment was added to line 402:

> "Then, based on the available dataset and the geographical features of the station region, it is recommended to work with observations within 100 km to the station, following ESA's requirements."

39) *Line 382. Add here that another possible reason for the less good results for the ANX2 case is the constant decrease in the Aeolus lidar performance. The Aeolus wind errors were significantly larger in 2021 and 2022 than in 2019 and 2020. Also Aeolus biases may have changed slightly.*

This suggestion was added to the text accordingly, in line 417:

> "These differences could be due to the gradual and constant decrease in the Aeolus lidar performance caused by the laser decay, which may have affected Aeolus biases."

40) *Line 423-424: It is not the differences in the cloud conditions at the location of the radiosonde and the Aeolus observations that matters the most, but the local variability of the wind field. Please compare the difference in the wind speed and direction reported in the Aeolus L2C product (ECMWF model forecast) compared to the radiosonde.*

Following the Reviewer#1's suggestion, we have performed a comparison between Aeolus L2C wind speed and direction with radiosonde measurements. The linear regression for the zonal, meridional and horizontal winds can be found in Table R1. There is a significantly good correlation between the instruments for all cases. Additionally, similar results were obtained in the manuscript (Table 2, row *a*), except for the mean differences. The mean difference between the Aeolus L2C wind direction and the radiosondes wind direction was -2 ± 30º for both channels, while the mean absolute difference was 20 ± 20º for both channels. These values are significantly smaller than those obtained in comment 34, indicating that the wind conditions captured by each instrument are significantly similar. The mean absolute difference of 20º indicates that wind conditions generally coincide. In fact, in Figure R2 it can be seen that differences larger than 35 º are significantly scarce. Thus, the wind conditions could be assumed to be similar.

		slope ± SE	intercept ± SE (m/s)	R	Δ ± SD (m/s)	|Δ| ± SD (m/s)
zonal wind	Mie	0.89 ± 0.01	0.6 ± 0.1	0.94	0 ± 4	3 ± 3
	Ray.	0.91 ± 0.02	0.6 ± 0.2	0.94	0 ± 4	3 ± 3
meridional wind	Mie	0.96 ± 0.01	-0.04 ± 0.10	0.95	0 ± 4	3 ± 3
	Ray.	0.97 ± 0.02	0.1 ± 0.1	0.95	0 ± 3	3 ± 2
horizontal wind	Mie	0.82 ± 0.01	2.8 ± 0.2	0.90	0 ± 5	3 ± 3
	Ray.	0.85 ± 0.02	2.1 ± 0.4	0.91	0 ± 4	3 ± 3

Table R1. Statistical results of the comparison between Aeolus L2C products and radiosondes measurements.

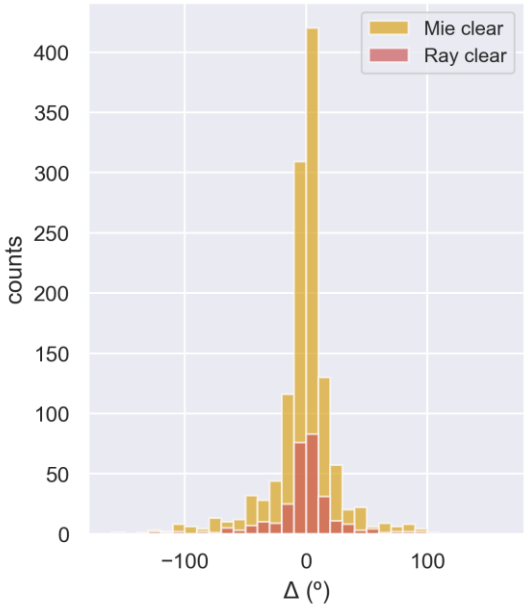

Figure R2. Histogram of the differences between Aeolus L2C wind direction and ground-based radiosondes wind direction.

41) *Line 441: Other reasons for the disagreement for the Rayleigh and radiosonde winds between 0 and 2 km altitude could be the variability of the wind within the 87 km Aeolus observations, and possible ground contamination of the lowest altitudes. Please add this.*

We appreciate this suggestion, as it will improve the discussion of the manuscript. Analogously to suggestion 36, the following discussion was added to line 497:

> "The discrepancies obtained for the Rayleigh clear lowermost vertical intervals can be attributed to multiple effects such as (i) the lower number of observations considered, (ii) the horizontal averaging of the signals, (iii) the large variability of the wind fields closer to the surface within the 100 km radius and (iv) the lower strength of the signal closer to the surface."

42) *Line 468: A time difference of 12 hours can lead to a very large difference in the local wind regime, especially in costal and mountainous terrain and when a front is passing. Please add this to the discussion below why the comparison results got worse.*

We kindly appreciate this suggestion. The authors believed this could be an issue. However, it was part of the analysis to test it. The discussion was added to line 529 as:

> "It is worth mentioning that a time difference of 12 h could lead to a very different wind regime comparison, depending on the local geographical features and the meteorological front scenario."

43) *Line 493: Also in the conclusions, please explain ANX4.5 and the product versions again since it has a similar function as the paper abstract.*

Following this suggestion, the acronyms HLOS, SNR, ABL, ESA and RMSE were spelled out in stead, and the reference to the ANX4.5 and the product version was rephrased in line 540 as:

> "… products before the orbit shift (versioning B10 and B11) …"

44) *Line 495: In stead of writing only "minor disagreement", please report the number for the magnitude here.*

Following this comment, the sentence in line 541 has been rephrased as:

> "The slopes, 1.2 ± 0.2 and 1.3 ± 0.1, and intercepts, 0.4 ± 0.9 and 0.3 ± 0.4 m/s, showed minor disagreements between the instruments, for the Rayleigh clear and Mie cloudy configurations respectively."

45) *Line 498: Specify that the vertical coverage limitation refers to the ground-based lidar.*

We appreciate this suggestion. The instrument we were referring to was specified in line 546 as:

> "The vertical coverage limitation of the ground-based Doppler lidar system was observed to increase during night-time or early morning overpasses, …"

46) *Line 511: Remove "and to the fact that cloud properties may vary significantly", which is not relevant here. Rather, the horizontal and vertical wind variability is important.*

Following this suggestion, the comment in line 511 (previous line numbers) was removed.

Editorial Corrections

47) *Throughout the whole manuscript, replace "bins" with "observations"*

Done.

48) *Line 17: spell out asl, above sea level*

Done.

49) *Line 30: "However, there is a current lack of accurate worldwide near-real-time atmospheric dynamics tracking in the Global Observing Systems (GOS), which affects the reliability of Numerical Weather Prediction (NWP) models" This sentence is difficult to understand. Proposed rephrase: "Numerical Weather Prediction (NWP) models need accurate wind profile observations in order to produce accurate weather forecasts. This is currently limited by the lack of globally distributed wind profiles in the World Meteorological Organization (WMO) Global Observing System (GOS)."*

The sentence was finally rephrased in line 30 as proposed by the Reviewer#1.

50) *Line 31: "For this aim" replaced with "Therefore"*

Done.

51) *Line 35: "…a single meteorological instrument, …" I propose to delete meteorological*

Done.

52) *Line 37: "into" to be changed to "to". "non-stop" to be removed. It is not clear what you mean with non-stop. The authors mean continuously over several years?*

Both suggestions were taken into account. With non-stop the authors meant uninterrupted. However, it was finally removed.

53) *Line 40: Propose to update "… Aeolus processing chain changed and product versioning has been updated, with subsequent improvements and fixings …" to: "… the Aeolus onground data processing is being continuously improved, resulting in processor updates about every 6 months and hence new product versions. The updated processors contain both improved calibration routines, bug fixes and retrieval algorithm improvements".*

We kindly appreciate this suggestion, as it helps to improve the writing of the manuscript. The sentence was finally rephrased in line 40 as:

> "Since the satellite launch, the Aeolus on-ground data processing is being continuously improved, resulting in processor updates about every 6 months and hence new product versions. The updated processors contain both improved calibration routines, bug fixes and retrieval algorithm improvements, in order to provide valuable near-real-time information that can be assimilated by the NWP models."

54) *Line 43: Propose to replace "… fields after being processed under NWP models" with "fields where Aeolus L2B winds have been assimilated in the weather forecast model of the European Centre for Medium Range Weather Forecasting (ECMWF), and the 3D wind field from the model has been stored at the location of the Aeolus observations."*

We appreciate this suggestion, as the information provided is more complete. The sentence was finally rephrased in line 44 as:

> "… and L2C wind fields where Aeolus L2B winds have been assimilated in the weather forecast model of the European Centre for Medium Range Weather Forecasting (ECMWF), and the 3D wind field from the model has been stored at the location of the Aeolus observations."

55) *Line 44: Propose to rephrase "…, Aeolus provides L2A optical information about the atmospheric components" to "Aeolus also provides profiles of atmospheric backscatter and extinction coefficients along its line of sight, which are stored in the Aeolus Level 2A product. All data products contain metadata, observation geolocation and other supporting variables" [RD-XX]. Add here references to the papers by Flament et al.*

The suggestion was kindly taken into account and the sentence was rephrased in line 46 as:

> "Aeolus also provides profiles of atmospheric backscatter and extinction coefficients along its line of sight, which are stored in the Aeolus Level 2A products (Flamant et al., 2020; Flament et al., 2021)."

However, no reference was made to metadata and geolocation information as we assume this is not relevant. Additionally, Reviewer #2 suggested to omit that information.

56) *Line 46: Add that operational monitoring of the Aeolus wind profile product quality was set up with the European Centre for Medium Range Weather Forecast (ECMWF), providing information of the wind quality within 3 hours of sensing.*

This information is kindly appreciated and was included in line 49 as:

"An operational monitoring of the Aeolus wind product quality was set up with the ECMWF, providing near real-time information of the wind quality, within 3 hours of sensing."

57) *Line 52: Add here the publication by Ehlers et al 2022, https://doi.org/10.5194/amt-15-185-2022*

Done.

58) *Line 58: The Aeolus wind products were not developed at Granada, rephrase this sentence to clarify this.*

The sentence in line 62 was rephrased as:

"This work presents Aeolus wind product Cal/Val activities carried out at Granada, …"

59) *Line 67: This sentence repeats the launch date repeated in the previous section. Proposed rephrase: "The Aeolus satellite orbit is sun-synchronous, with an orbit altitude of 320 km, an inclination of ~97°, and an orbit repeat cycle of 7 days."*

Following this suggestion, the sentence was rephrased in line 71 as:

"The Aeolus satellite is located in a Sun-synchronous orbit, at 320 km from the Earth surface and an inclination of around 97°. It completes an orbit in around 90 min and provides a full coverage of the Earth every week (orbit repeat cycle of 7 days)."

60) *Line 70-71: The description of the dual Fabry-Pérot interferometer channel, which is used to measure the Doppler shift from the atmospheric backscatter by molecules, and the Fizeau interferometer channel, which is used to measures the Doppler shift of the atmospheric backscatter from particles, is not very clear. The channels measure the Doppler shift of the backscattered light, caused by the movement of the molecules or particles along the instrument LOS. From this, the wind speed is derived by comparing the measured frequencies with the laser emit frequency. It is also a repeat of what is written in the section before. Rewrite and remove duplications in the manuscript where possible.*

The authors understand the misunderstanding. Thus, line 75 has been completed as:

"… that is a Fabry-Pérot interferometer registering the Doppler shift from the atmospheric backscatter caused by molecules known as Rayleigh channel, and a Fizeau interferometer registering the Doppler shift of the atmospheric backscatter caused by particles (clouds and aerosols) known as Mie channel."

Additionally, duplications were removed from the manuscript where possible.

61) *Line 76: Not Aeolus, but the on-ground data processing performs the horizontal projects the LOS winds measured by Aeolus. Rephrase.*

Following this suggestion, the sentence in line 80 was rephrased as:

"Aeolus on-ground data processing provides …"

62) *Line 77: what do you mean by "… which is a variable able to sufficiently characterize the wind field"? Mention that the wind direction is not measured by Aeolus, and also not the vertical wind component, which is assumed to be 0 in the data processing from LOS to HLOS. Mention that the winds need to be assimilated in a weather model to yield the full 3D wind vector.*

The authors meant to say that, according to ESA (2008), the HLOS component of the wind, i.e. a single-component wind, would be sufficient to characterize the wind fields. On the other hand, the mentioned information has been added to lines 45 and 91. No information was added about the vertical wind component as: (i) it is not a variable addressed throughout the manuscript and (ii) the processing of Aeolus signal in order to retrieve the HLOS is neither addressed.

63) *Line 80: Mention that the vertical resolution of the Aeolus wind observation profiles are not fixed, that they differ for the two Aeolus wind channels, as a function of latitude (i.e. along the orbit) and for different time periods through the mission. This is explained in the CAL/VAL plan, in papers by Rennie et al. etc.*

We appreciate this suggestion, as it provides valuable information that was missing from the manuscript. The information has been included in line 84 as:

> "However, this vertical resolution is not fixed and has changed for different time periods through the mission. Additionally, the Mie and Rayleigh bins height grid do not coincide and may shift along the orbit."

64) *Line 86: The Level 2B data processing is performed by ECMWF under ESA contract, as part of the Aeolus ground segment.*

We appreciate this clarification and was included in the manuscript in line 91 as:

> "As part of the Aeolus on-ground data processing, L2B products are processed by the European Centre for Medium-range Weather Forecasts (ECMWF) under ESA contract in order to provide improved wind fields (wind vectors), classified as L2C products."

65) *Line 88: The authors again mention the Level 2A product. I propose to only explain it in line 45, and also put the references to details on this product there.*

We understand the double mention of the information. Thus, the information was removed from line 88 (previous line numbers) and was included in line 46.

66) *Line 91-97: Also here the text is not clear. As explained in Rennie, the first step in the data processing from measurement to observation scale is to sort the measurements into two categories called clear and cloudy. The clear category contain measurements from molecular backscatter, and the cloudy category from particle backscatter. The classification is done by using a threshold for the measurement scattering ratio. The clear measurements are then averaged over about 87 km along the satellite track for the clear measurements to yield wind observations called Rayleigh clear winds. Cloudy measurements are averaged over about 13 km along the satellite track for the clear measurements to yield wind observations called Mie cloudy winds. Please rephrase to make this clear. The authors can mention the further Rayleigh cloudy and Mie clear products briefly, but explain that these are generally of less good quality and are quite sparse and are therefore not analysed here.*

We appreciate this suggestion. The information was included in the manuscript and the paragraph starting in line 96 was rephrased as:

> "First, the on-ground processing chain applies a scene classification based on the backscatter ratio (total-to-molecular backscatter coefficient ratio) to determine if the measurements correspond to a cloud or a clear atmospheric region (Tan et al., 2017; Reitebuch et al., 2018). Then, the measurements are averaged and processed to yield up to four separate wind configurations, namely Mie cloudy, Mie clear, Rayleigh cloudy and Rayleigh clear. However, only the Mie cloudy and the Rayleigh clear configurations are generally used (Lux et al., 2020b; Martin et al., 2021). For each observation, quality flags address different parameters related to the measured signal, from ground echo interference to SNR thresholds."

67) *Line 99: The main reason why issues with the Aeolus instrument alignment and detector quality could be detected quite fast and mitigated though improved instrument calibration and on-ground data processing corrections was that the L2B wind product is operationally monitored at ECMWF. ECMWF compares the Aeolus winds arriving within 3 hours of sensing (e.g. in near-real-time) directly with the short-range forecasts fields and produces error statistics. The Aeolus CAL/VAL activities were able to confirm the errors detected by the ECMWF model monitoring and provide further information. This allowed for the Aeolus data processing to be quickly improved. Please rephrase accordingly.*

We understand the misunderstanding and appreciate this suggestion. Following this comment, line 103 was rephrased as:

> "From the ECMWF near real-time monitoring and validation of Aeolus products and previous Cal/Val activities, …"

68) *Line 105: This is a repeat of what is written earlier in the paper on the on-ground data processing baseline versions. Please harmonize and rephrase, to write that the processor updates are done approximately every 6 months, allowing for bug fixes, improved data calibration and data processing, and thereby improving the product quality. About once a year, the datasets are reprocessed with one processor baseline in order to produce long datasets with the same data quality.*

We do not understand how this information was previously mentioned in the text, as the first mention to the on-ground processing versions (baselines) is in line 105 and the whole paragraph addresses the same topic. However, the suggested information about the frequency of updates is kindly welcomed and was included in line 110 as:

> "On-ground processing chain updates are released approximately every 6 months, improving the product quality by implementing bug fixes and improved data calibration. About once a year, datasets are reprocessed under a single processor baseline in order to release longer-term datasets with the same data quality."

69) *Line 110: The text is not clear hear, mentioning malfunctioning periods and "rarely interrupted" in one sentence. Please rephrase. With "malfunctioning periods" I guess you mean the periods where the instrument switched itself off automatically, which means that it took several weeks to switch it on and realign it again. Mention that these spontaneous switch-off events have been rear.*

We appreciate this suggestion, as the sentence was itself inconsistent. Then, line 116 has been rephrased as:

> "The satellite is constantly checked and laser sensitivity tests (and other analysis) are frequently performed. For these reasons, validity flags have to be taken into account. However, the instrument regular operation is rarely interrupted (e.g., March 2021 due to instrument anomalies)."

70) *Line 111: For a non-expert, it is not possible to understand what ANX4.5 means. This is the longitude of the reference ascending note of the orbit used for the longitudinal placing of the 7 day repeat cycle. I propose that you explain this and refer to Figure 1 to illustrate how the weekly overpasses close to Granada shifted in June 2021.*

Following this suggestion, the next comment was included to line 122:

> "During the second half of June 2021 Aeolus orbit setting changed from ANX4.5 (ascending node crossing 4.5, where 4.5 is the longitude of the reference ascending node orbit used to set the satellite orbit) to ANX2.0. The orbit shift took place in the framework…"

On the other hand, Figure 1 was removed according to comment 23.

Line 135: rephrase the explanation of the system measures the frequency shift of the backscattered signal, caused by the movement of particles along the instrument LOS. From the frequency shifts, the movement of the atmosphere along the LOS, and hence the wind in this direction, can be obtained. The system measures along 3 LOSs or it is scanning, yielding the 3D wind field.

We highly appreciate this comment, as it yields information that could lead to misunderstanding. On the one hand, the reference to Aeolus was omitted, in order to avoid any mislead and the sentence in line 136 was rephrased as:

> "The system is able to detect the backscattered signal's Doppler shift caused by the movement of the atmospheric components along the line-of-sight of the instrument."

On the other hand, an explanation on how the Doppler lidar measures was added to line 142:

> "The vertical wind component is obtained with the Doppler lidar system vertical stare mode, while the horizontal wind components, and the wind direction, are obtained through the velocity-azimuth display approach (Browning and Wexler, 1968), …"

71) *Line 136: Rephrase to say that the system can only measure the backscatter from particles due to the wavelengths used (1500 nm) which is compatible with Mie scattering. The signal strength is important for the instrument range.*

Following this comment, the sentence in line 138 was rephrased as:

> "However, due to the 1500 nm radiation used by the Halo Photonics StreamLine Doppler lidar, the exploitable signal is limited to the one backscattered by particles (e.g., aerosol particles and clouds), still in the Mie scattering regime."

72) *Line 140: explain "agl". Explain that the telescope overlap means that winds cannot be measured below this altitude.*

The term was spelled out instead and the full-overlap height was explained in line 140 as:

> "… emits radiation at 15 kHz and with a heterodyne detector (Pentikäinen et al., 2020) is able to retrieve wind profiles ranging from 60 m above ground level (full-overlap height) with a vertical resolution of 30 m and a tunable temporal resolution down to 2 s."

73) *Line 165: Like for line 11, explain ANX2.0.*

The explanation was included in line 123.

74) *Line 174: The authors have mentioned before that you compare two of the four wind products in the Aeolus L2B data. I propose to refrain from mentioning this again and simply say that the Aeolus Rayleigh clear and Mie cloudy products will be analysed.*

This suggestion was taken into account and line 172 was rephrased as:

> "In this work, the Rayleigh clear and Mie cloudy configurations were analyzed."

75) *Line 177: What is meant with " … where the configuration could provide several wind estimates even for the same bin height, …" Do you mean that Rayleigh clear and Mie cloudy winds sometimes occur at the same geolocation ad altitude? Please clarify.*

That sentence meant to say that within the, generally, 100 km spatial collocation criteria several observations at the same bin height could be considered for a single overpass. This was observed to be more frequent for the Mie channel. However, this could also happen for the Rayleigh channel, so the authors have decided to omit this information from the manuscript, from line 177 (previous line numbers).

76) *Line 178: I do not understand this sentence: "… presenting a vertical coverage limited to the clous extension …) . Please rephrase and be more precise what you mean here and which data product you refer to. The whole sentence to too long with too many parenthesis and commas. Mention that Aeolus provides good quality winds on top of water clouds in the ABL.*

We appreciate this suggestion, as it helps to make the manuscript easier to follow. Taking into account this suggestion, and the previous one, the sentence in line 173 was rephrased as:

> "… while the Mie cloudy products were limited to estimates within clouds (or high backscatter conditions), generally between the surface and 12 km asl."

77) *Line 184: Rewrite to say "Aeolus observations where the quality flag was set to valid was used." Write observations and not bins also in the further parts.*

We appreciate this suggestion, and it was taken into account in line 182 as:

> "Aeolus observations with valid quality flags were used."

We decided to omit "was set to" in order to not give the wrong idea that the quality flags were manually set.

78) *Line 194: Specify whether you have used data from January to June 2020, and if yes which processor version these have been processed with (B11?)*

That period was not used in the comparison. It is now specified in line 202.

79) *Line 201, 213: Specify again what you mean with a spin-off comparison.*

Spin-off means that the comparison is coming from the principal comparison. In this case, we wanted to address the short comparisons performed with different collocation criteria, which were also important to the principal comparison as they could be taken into account as support analyses. In this sense, "spin-off" is related to "secondary". However, the word was finally removed from the text.

80) *Line 245: Replace "On the other hand" with "Furthermore" or something similar. To be checked by a native speaker.*

The suggestion was taken into account in line 256.

81) *Line 254: You have not explained B11 before, and for this data period it was used. B11 and the period it was covering should be introduced earlier in the manuscript.*

The suggestion was taken into account and B11 is now mentioned in lines 188 and 202. Also, B12 is now mentioned in line 188.

82) *Line 254-255: The language is not clear here and needs to be checked. "were B10" etc is not clear.*

Following this suggestion, the sentence in line 265 was rephrased as:

> "101 overpasses were B10 products while 43 were B11 products, …"

83) *Line 255: Mention here that earlier studies have found that the Aeolus winds have orbit phase dependent biases. Refer to the papers describing this, e.g. Rennie et al. Mention that this motivates you to check also orbit phase dependent statistics in the later analysis.*

We appreciate this suggestion was take into account in line 266 as:

> "The distinction between the orbit phase was motivated by previous studies that found orbit phase dependent biases in Aeolus wind products (Rennie et al., 2021)."

84) *Lines 258-260: Again, it is difficult to understand what is written. Language needs to be checked and refined.*

We appreciate this type of suggestion, as these kinds of issues may not be detected by the authors. Lines 269 to 273 were rephrased as:

> "From these, 5.6 % (2.8 %) of the available Rayleigh clear (Mie cloudy) observations exceeded the error threshold of 8 m/s (4 m/s) that was mentioned in Section 3.1. A total of 358 (40) observations were filtered out. The mean minimum distance (± standard deviation, SD) from the Aeolus ground track to the station was 19 ± 10 (14 ± 9) km, for the Rayleigh clear (Mie cloudy) observations. Meanwhile, the mean distance (± SD) from all Aeolus observations to the station was 54 ± 17 (51 ± 27) km."

85) *Line 357-358: This sentence is difficult to understand. Please rephrase to make it clearer what you mean and see from the data analysis.*

This suggestion was taken into account, and line 393 as:

> "In this case, the slope and intercept significantly increased (slightly decreased) when the maximum collocation distance decreased, for the Rayleigh clear (Mie cloudy) configuration."

86) *Line 399: replace "values" with "observations"*

Done.

87) *Line 410: Mention the geographical location of the wind comparisons done by Baars et al (2020), and discuss why the result could be different.*

The suggestion was taken into account, in line 455 as:

> "… by Baars et al. (2020) for previous baseline Aeolus products and radiosondes over the Atlantic, …"

While the discussion was included in line 459 as:

> "However, it should be noticed that the studies were performed in different locations with particular geographical features."

88) *Line 443: The phrase "Aeolus did not present a particular performance …" is not understandable and should be changed, e.g. to "The Aeolus and radiosonde wind observations agreed well …"*

The suggestion was taken into account. Therefore, the sentence in line 490 was rephrased as:

> "The Rayleigh clear and radiosonde observations agreed well between 2 and 12 km asl, with biases ranging around zero (Δ between -0.9 and 0.6 m/s)."

89) *Line 445: "The RMSE presented a similar value between 4 and 18 km …"? Similar to what?*

In this sentence we wanted to express that the values of the RMSE were similar among them. The sentence in line 492 was rephrased as:

> "The RMSE values were similar between 4 and 18 km asl …"

90) *Line 498: Change "emphasize" to "increase"*

Done.

91) *Line 500: "The limitation is softened …" This is not understandable, please rephrase.*

The sentence in line 548 has been rephrased as:

> "The impact of this limitation is lowered under cloudy conditions, …"

92) *Line 502: Rephrase to: "The 100 km collocation criteria proposed by ESA was shown to be suitable for this study."*

We appreciate this suggestion, and line 550 was rephrased as:

> "Thus, the 100 km collocation criteria proposed by the European Space Agency (ESA) was shown to be suitable for this study."

93) *Line 514: replace "handicap" with "drawback", and "Additionally" with "However"*

Done.

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