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Atmospheric Measurement Techniques Discussions

Interactive comment on "On the relationship between wind observation accuracy and the ascending node of sun-synchronous orbit for the Aeolus-type spaceborne Doppler wind lidar" *by* Chuanliang Zhang et al.

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Dear Dr. Karsten Schmidt,

We are truly grateful to your critical comments and thoughtful suggestions. Based on these comments and suggestions, we have made careful thoughts. We are now sending you the corresponding replies. Please point out the mistakes and weaknesses for correction if any. Below you will find our point-by-point responses to your comments/ questions, the comments and suggestions you gave are marked in blue, our replies are

marked in black:

Major specific comments

1. The authors should compare SBR computed by means of their model with measured in-orbit SBR data for certain time ranges. The reviewer 1 provided some data in his review supplement. This would increase the confidence in the authors model. Moreover, plots over a year show that SBR measured by Aeolus is maximum in June and December (see again supplement). Thus the authors can argue that their investigations for June and December are for the worst cases with maximum Rayleigh channel wind errors due to SBR.

Response: Thanks for your suggestions. As to the comparison between simulated and measured solar background radiation (SBR), we explained the topic in detail in the reply to Q3 of reviewer 1. The main ideas are described briefly as follows:

First, because the SBR is mainly determined by solar zenith angle of the off-nadir points, we computed the solar zenith angles of off-nadir points within one-year range. The variations of solar zenith angles are in consistent with the variations of in-orbit measured SBR indicate that the variation trend of simulated SBR would be in consistent with the measured SBR.

Then, we simulated SBR received by Aeolus during 15 days near summer and winter solstices. And the simulated SBR in the two days of summer and winter solstices was shown in Fig. 4 of the reply to reviewer 1 to compare with Fig. 1(a) of the supplement. The comparisons show that the two are consistent in variation trend and magnitude, which increase the confidence of our model.

In addition, in the ATBD L1B Products (issue 4.4 20.04.2018), the highest background integration times is 3750 μs which is related to 446 km vertical height of 25th range gate. In our simulation, the integration times for solar background radiation is 1680 μs which is related to 200 km vertical height of 25th range gate.

Furthermore, thanks very much for your suggestions in reply to reviewer 1, we have stated in the response to reviewer 1 that we are considering the worst conditions of solar background radiation in the manuscript.

2. In lines 60-61, the authors write that the "received SBR of Aeolus ranges from 0 to 169 mW*m-2*sr-1*nm-1". The authors should give the corresponding reference. Aeolus measures primarily ACCD counts of SBR.

Response: Thanks for your kind reminder. The results were obtained from (Zhang et al., 2019). We will add the citation in the revision.

3. It is of course possible and interesting to consider sun-synchronous orbits other than dawn-dusk orbits. The authors should explain their choice of orbits with LTANs of 15:00 and 12:00 in Section 2.1.

Response: Thanks for your reminder. We will add the related explanations in Section 2.1 in the revision. The detailed reasons were given in Q1 of the reply to reviewer 1.

4. It becomes not clear which kinds of aerosols are considered by the authors in their simulations (only aerosols in the planetary boundary layer (PBL) or also above it). The authors should specify this. Furthermore, the authors should replace "clear sky" by "cloud-free" in lines 15 and 379 due to the presence of aerosols.

Response: In the manuscript, only the aerosols in the PBL were considered. The aerosol above it (stratospheric aerosols) were not taken into account.

The optical properties of aerosols used in this manuscript was obtained from the LIVAS (LIdar climatology of Vertical Aerosol Structure for space-based lidar simulation studies) database (Amiridis et al., 2015). In the database, the products of aerosol optical properties include "355_Aerosol_Backscatter_Mean", "355_Stratospheric_Backscatter_Mean", and "355_Total_Backscatter_Mean". "355_Aerosol_Backscatter_Mean" was used as the backscatter coefficients of aerosols in this manuscript.

In the revision, we will replace "clear-sky" by "cloud-free" in lines 15 and 379. Thanks for your detailed suggestions.

5. For the simulations, the Aeolus instrument parameters have been taken by the authors from the Algorithm Theoretical Basis Document (ATBD; Reitebuch et al., 2006). There is however a newer version of this document (issue 4.4, 20.04.2018), e.g. available by ESA for Aeolus CalVal users. Furthermore, the authors considered observations consisting of 50 accumulations (measurements) of 14 shots, resulting in a horizontal resolution of about 100.8 km per observation. However, Aeolus has 30 measurements per observation with 20 laser pulses per measurement (in the level 1B processing), resulting in a horizontal averaging length of about 90km per observation. So the averaged wind observation uncertainties, derived by the authors in the present study, are only some estimates. It is proposed to use the newer/current parameters in future simulations in order to increase their usefulness.

Response: Thanks very much for your reminder. Tab. 1 (Fig. 10 at the end of the text) of the reply to reviewer 1 illustrate that the parameters used in our simulation and the new parameters are of quite difference, which are mainly reflected in narrower bandwidth of Fabry-Perot Free Spectral Range and FWHM of Rayleigh channel of the new instrument parameters. Under new instrument parameters, the same values of SBR would excite fewer photon counts on the ACCD of Rayleigh channel. However, the photon counts excited by atmospheric backscattered signals are similar under new and old parameters. Finally, the fact would lead to smaller wind observation uncertainties using new parameters.

Our simulation indicates that the largest and mean difference of wind observation uncertainties were 2.17 and 0.61 m/s under a specific atmospheric condition and the SBR of 72.19 mW$\cdot m^{-2}\cdot sr^{-1}\cdot nm^{-1}$. The details of the experiment can refer to Q3 of the reviewer 1.

Given the large difference between the old and new parameters, if the associate editor give the chance to revise, we will use the new parameters in our simulation model.

6. Eq. (7) has been numerically verified in the Appendix by neglecting noise (see item (5) in line 436). Consequently, Eq. (8) holds only for this restriction. Then, Eq. (8) is reformulated to Eq. (10) by using Eq. (6). However, Eq. (6) does contain noise, and consequently also Eq. (10) and its solution (11), which are used in the following investigations. The authors should comment on this. It becomes also not clear whether the results in Fig. (A3) have been obtained with or without noise.

Response: The results in Fig. (A2, A3) have been obtained without noise.

When deriving the formula, we considered the influence of the noise of the detection unit on the accuracy of wind observations, so all formula derivations include noise, including Eq. (7). As Tab. 4-1 of ADM-Aeolus ATBD Level1B Products shows, the detection chain noise for each measurement is 4.7 e-/pixel, and the dark current is 1.9 e-/(pixel·s), which is negligible compared to the photon counts excited by atmospheric backscattered signal and solar background radiation. Therefore, in the verification for the equations in Appendix (Fig. A2 and A3), the noise is not taken into account.

Is it reasonable to neglect the noise in verification the noise in the simulation? If not, we will taken the noise into account in the verification. If so, we will explain the reason why we neglect the noise in Appendix.

7. In the discussion of Fig. 5 on pages 12-13, the authors should comment on the jump in the required laser pulse energy when going from the troposphere to the stratosphere. It is obviously due to the increase of the bin thickness and the resulting larger Rayleigh channel signals. Furthermore the authors should speculate why less energy is required in PBL, compared to the upper troposphere, though the PBL bin thicknesses are smaller, the laser energy and the Rayleigh channel backscattering damping are larger, and ESA's accuracy requirements are more restrictive in PBL. Is there any cross talk from the Mie channel caused by PBL aerosols?

Response: Thanks for your kind reminder, we will explain the jump when going from troposphere to stratosphere in line 321 of Page 12.

Original text: Higher energy is needed mostly in the upper level of troposphere and stratosphere near the regions close to Antarctic and Arctic circles. The closer the orbital LTAN is to noon, the averaged values of the required laser energy will become larger.

Modified: Higher energy is needed mostly in the upper level of troposphere and stratosphere near the regions close to Antarctic and Arctic circles. On the boundary line with a height of 16 km, there is an obvious sudden jump in required laser energy, and the required laser energy is reduced. This is mainly because the vertical thickness of measurement bins changes from 1 km to 2 km at height of 16 km, which makes the integration time of detection units of Rayleigh channel double. And larger atmospheric backscattered signal would be received. On the other hand, the required wind observation uncertainty increase from 2 m/s to 3 m/s in the stratosphere. Therefore, the required laser energy reduced suddenly when going from troposphere to stratosphere. The comparisons among the required laser energy of the three orbit illustrate that the closer the orbital LTAN is to noon, the averaged values of the required laser energy will become larger.

Solar background noise has main impact on the wind observation uncertainties on Rayleigh channel. The impact of SBR on Mie channel is negligible (Rennie, 2017). Due to the widespread presence of aerosols in PBL, Mie channel is used to observe the wind in PBL. And the main topic of the manuscript is to study the impact of SBR on the Rayleigh channel wind observations. Therefore, the Rayleigh wind observations in PBL is not considered. Sentences in the original text may misunderstand readers, we will modify as follows:

Original text: In fact, the Mie channel is mostly used for wind observations in the PBL, which are of higher accuracy. It is meaningless to study the wind observation accuracy of the Rayleigh channel in the PBL, the accuracy of the Rayleigh channel in the PBL is not considered in the following of this paper. (line 279)

Modified: In fact, the Mie channel is mostly used for wind observations due to the widespread presence of aerosols in PBL. Therefore, the accuracy of the Rayleigh channel in the PBL is not considered in the following of this paper.

As to the questions why less energy is required in PBL, in the PBL, the Mie channel wind observation uncertainties is much less than that of Rayleigh channel as Fig. 1(a) at the end of the text shown. However, the photon counts of Mie channel excited by atmosphere aerosols and solar background radiation is also much less than that of Rayleigh channel as Fig. 1(b, c) shown. The optical properties of aerosols are obtained from RMA dataset. So I don't know the reasons for the phenomenon that the wind observation accuracy on Mie channel is higher than that of Rayleigh channel. It may be due to the detection mechanism of Mie channel. Hope for further discussions to the question with reviewers.

minor specific comments

8. The authors should be more specific in the abstract in line 15 by writing "increment of averaged Rayleigh channel wind observation uncertainties", since they consider only Rayleigh channel winds.

Response: Thanks very much for your suggestions. We will modify the text according to your suggestions in the revision.

9. The authors write in lines 108-109: "Figure 1(b) shows that the solar zenith angle of the observation points of the two new Aeolus-type instruments is low compared to that of Aeolus, and thus lead to larger SBR." However, this figure does not show solar zenith angles. The authors should comment on this.

Response: Thanks for your reminder. From Fig. 1(b), we cannot get the information that the solar zenith angle of two new Aeolus-type instruments is low compared to that

of Aeolus. We will add a graph to illustrate the point, as Fig. 9 of the reply to reviewer 1 shows.

10. In lines 273-274: Where do the 18 wind uncertainty profiles come from? And is there 1 profile for every 10? latitude stripe?

Response: Yes, there is 1 profile for every 10° latitude stripe.

In Fig. 2 of the manuscript, the maximum SBR of each grid (the earth was divided into $1^{\circ}\times1^{\circ}$ grids) was illustrated. Because the SBR is not much different at the same latitude as Fig. 2 of the manuscript shows, the SBR are averaged within 10° latitude. Then the 10° latitude averaged atmospheric conditions were obtained from Ozone Monitoring Instrument (OMI) database as mentioned in subsection 2.2 of the manuscript. Finally, the 10° latitude averaged uncertainties of wind observation on Rayleigh channel derived and show in Fig. 3 of the manuscript.

11. Table 2 shows that the averaged increment in the wind observation uncertainties of the 12:00 orbit in the stratosphere is 1.23 m/s, compared to the 18:00 Aeolus orbit. In the text however, 1.4 m/s is reported (lines 16, 286, and 380). Thus the value in the text could be lowered.

Response: Thanks very much for kind reminder, we will lower the value in the corresponding text.

12. the authors should rename the title of Section 4.4 to "Uncertainties of wind observations resulting from an increased laser pulse energy" because they only consider an increased laser pulse energy as a new instrument parameter. Furthermore, the authors should mentioned in line 341 that their proposed laser energy of 80 mJ has been already required by ESA (see e.g. ATBD; Reitebuch et al., 2018). Moreover, the authors should delete the phrase "new instrument parameters, of which" in the caption of Fig. 6. Additionally, the authors should replace "instrument parameters" by "laser energies" in the caption of Tab. 6.

Response: Thanks for your kind suggestions.

We will rename the title of Section 4.4 to "Uncertainties of wind observations resulting from an increased laser pulse energy".

The sentence in line 341 will be replaced with "while taking the existing technical level into account, the laser energy of the two new spaceborne DWLs is set to 80 mJ, which has been already required by ESA for Aeolus." And add the corresponding citation.

The caption of Fig. 6 will be renamed as "The zonal distributions of wind observation uncertainties of the three spaceborne DWLs, the laser energy of Aeolus is 60 mJ, and the laser energy of the two new Aeolus-type spaceborne DWLs is 80 mJ."

The caption of Tab. 6 will be renamed as "The averaged wind observation uncertainties of the three spaceborne DWLs with the proposed laser energies."

13. In the abstract, the authors should recall the conditions for which they have derived their results (no clouds, aerosols, noise (?), laser energies of 60 mJ and 80 mJ respectively, number of measurements per observation, number of laser shots per measurement, only Rayleigh channel winds).

Response: Thanks for reminder. We modified the original text as follows:

Original text: the impact of the local time of ascending node (LTAN) crossing of sun-synchronous orbits on the wind observation accuracy was studied in this paper by proposing two added Aeolus-type spaceborne DWLs operated on the sun-synchronous orbits with LTAN of 15:00 and 12:00 combined with Aeolus. (line 11)

Modified: the impact of the local time of ascending node (LTAN) crossing of sun-synchronous orbits on the Rayleigh channel wind observation accuracy was studied in this paper by proposing two added Aeolus-type spaceborne DWLs operated on the sun-synchronous orbits with LTAN of 15:00 and 12:00 combined with Aeolus.

Original text: On the two new orbits, the increments of averaged SBR received by the

new spaceborne DWLs range from 39 to 56 mW$\cdot m^{-2} \cdot sr^{-1} \cdot nm^{-1}$ under clear skies, which will lead to the increment of averaged wind observation uncertainties from 0.3 to 0.4 m/s in the troposphere and from 0.9 to 1.4 m/s in the stratosphere. (line 13)

Modified: On the two new orbits, the increments of averaged SBR received by the new spaceborne DWLs range from 39 to 56 mW$\cdot m^{-2} \cdot sr^{-1} \cdot nm^{-1}$ under cloud-free conditions, which will lead to the increment of averaged wind observation uncertainties from 0.3 to 0.4 m/s in the troposphere and from 0.9 to 1.4 m/s in the stratosphere on Rayleigh channel. In our simulation, one observation consists of 14 measurements and 50 laser pulses are accumulated in one measurement.

The following changes are proposed to improve the readability of the paper.

14. There are several incidences where different statements are separated only by a comma in one sentence (e.g. lines 10-13). Please check the paper for that and introduce separate sentences.

Response: Thanks for your suggestions. We will make corresponding modifications in the revision.

15. Please replace "by 0.18, 0.69 m/s" by "by 0.18 and 0.69 m/s" in line 62.

Response: Thanks for your suggestions. We will make corresponding modifications in the revision.

16. Please provide the reference for the quantum efficiency of the Rayleigh channel detector in line 181 (obviously Reitebuch et al., 2006).

Response: Thanks for your suggestions. We will provide the reference value for the quantum efficiency and add related citation.

17. In the caption of Fig. 2, please interchange the 2. and 3. sentence (i.e. first the 3. and then the 2. sentence as the last sentence). Furthermore, do Figs. (c,d) and (e,f) really show numerical differences to Figs. (a,b)? Or do the contours in Figs. (c,d) and

(e,f) only show values from the right-hand side scale?

Response: Thanks for your suggestions. We will interchange the 2. and 3. sentence of the caption of Fig. 2. The caption will be modified to “Global distributions of SBR received by spaceborne DWLs operated on the three orbits. Figs. (a, b), (c, d) and (e, f) present the sun-synchronous orbits with LTAN of 18:00, 15:00, and 12:00 respectively, and the upper panels denote the SBR in summer, and the lower panels denote the SBR in winter. The contours in the Figs. (c, e), (d, f) denote the difference between the SBR in Figs. (c, e), (d, f) with the SBR in Figs. (a, b), respectively.”

Yes, Figs. (c, d) and (e, f) show numerical differences to Figs. (a, b).

18. Please add the SBR increments [mW*m-2*sr-1*nm-1] 60.68-20.99=39.69 and 76.36-20.99=55.37 in line 263 because they are listed in the abstract and in the summary.

Response: Thanks for your suggestions.

Original text: Statistics illustrate that the averaged SBR of the three spaceborne DWLs are 20.99, 60.68, and 76.36 mW$\cdot m^{-2}\cdot sr^{-1}\cdot nm^{-1}$ respectively.

Modified: Statistics illustrate that the averaged SBR of the three spaceborne DWLs are 20.99, 60.68, and 76.36 mW$\cdot m^{-2}\cdot sr^{-1}\cdot nm^{-1}$ respectively. The increments of averaged SBR received by the new spaceborne DWLs are 60.68-20.99=39.69 mW$\cdot m^{-2}\cdot sr^{-1}\cdot nm^{-1}$ and 76.36-20.99=55.37 mW$\cdot m^{-2}\cdot sr^{-1}\cdot nm^{-1}$.

19. The sentence in lines 263-264 ("The quantile statistics of SBR is presented in Table 1, which means that the corresponding percentages of the grids (the earth is divided into 1°×1° grid) of which the SBR will be smaller than the values listed in the first line of Table 1.") is unclear. Please provide a clearer formulation, e.g. also by adding an example (e.g., 90% of the grid points (?) or tiles (?) of the 12:00 orbit have SBR values smaller than 105.77 mW*m-2*sr-1*nm-1).

Response: Thanks for your suggestions. An example would make it clear.

Original text: The quantile statistics of SBR is presented in Table 1, which means that the corresponding percentages of the grids (the earth is divided into 1°×1°grid) of which the SBR will be smaller than the values listed in the first line of Table 1.

Modified: The quantile statistics of SBR is presented in Table 1, which means that the corresponding percentages of the grids (the earth is divided into 1°×1°grid) of which the SBR will be smaller than the values listed in the first line of Table 1. For example, 90% of the grid points of the 12:00 orbit have SBR values smaller than 105.77 mW$\cdot m^{-2}\cdot sr^{-1}\cdot nm^{-1}$.

20. Please replace "upper layer of troposphere and stratosphere" by "upper layer of atmosphere" in lines 277-278.

Response: Thanks for your suggestions.

Because in the lower layer of stratosphere, the wind observation uncertainties would meet the accuracy requirement of ESA. So we think the original expression is more accurate.

21. In the captions of Figs. 3, 5, and 6, the authors write that the "correspondence relationship between the subgraphs and orbits, seasons is consistent with Fig. 2". It is proposed to reformulate this sentence, e.g. to "The arrangement of the subgraphs corresponds to that of Fig. 2".

Response: Thanks for your suggestions. We will reformulate the sentence according to your suggestions.

22. In lines 298-299: Is the accuracy level of Aeolus, mentioned here, that one shown in Figs. 3 (a) and (b)? If so, please note this here.

Response: Yes, the accuracy level of Aeolus is the one shown in Figs. 3(a) and (b). We will add the expression in the revision.

Original text: Supposed that the wind observation accuracy of the two new spaceborne DWLs is required to reach the accuracy level of Aeolus, which can be used for joint observations of the three satellites.

Modified: Supposed that the wind observation accuracy of the two new spaceborne DWLs is required to reach the accuracy level of Aeolus as shown in Figs. 3(a, b), which can be used for joint observations of the three satellites.

23. It is assumed that the results shown in Figs. 6 (a) and (b) are identical to those of Figs. 3 (a) and (b). It is however not directly seen due to the different color scales. If so, please make a corresponding note in the text or caption of Fig. 6. If not, please explain why it is not the case.

Response: Thanks for your suggestions.

Figs. 6 (a, b) are identical to those of Figs. 3 (a, b). Because both of the laser energies are 60 mJ. We will make a corresponding note in the text in the revision.

Original text: The comparison between Fig. 6(c-f) and Fig. 3(c-f) illustrate that, as for the two new spaceborne DWLs, when the laser energy increases from 60 mJ to 80 mJ, the observation accuracy would be improved significantly. (line 357)

Modified: Figs. 6(a, b) are identical to those of Figs. 3 (a, b), for that both of them are observed with laser energies of 60 mJ. The comparison between Figs. 6(c-f) and Figs. 3(c-f) illustrate that, as for the two new spaceborne DWLs, when the laser energy increases from 60 mJ to 80 mJ, the observation accuracy would be improved significantly.

24. technical corrections

lines 53-55: Doppler wind lidar which sensing -> senses, Mie/Rayleigh channel sensing -> senses

line 122: expect the mean altitude -> except

Different notations are used for the uncertainty of wind observation in the Rayleigh

channel in Eqs. (1) and (8). Please use a consistent notation.

line 233: the wind observation uncertainty which were calculated -> was

line 454: is also need -> needed

line 463: Subsect. 3.4 does not exist, replace by Subsect. 3.3

Response: We're very sorry for the low-level mistakes. And we will correct these mistakes in the revised manuscript.

References

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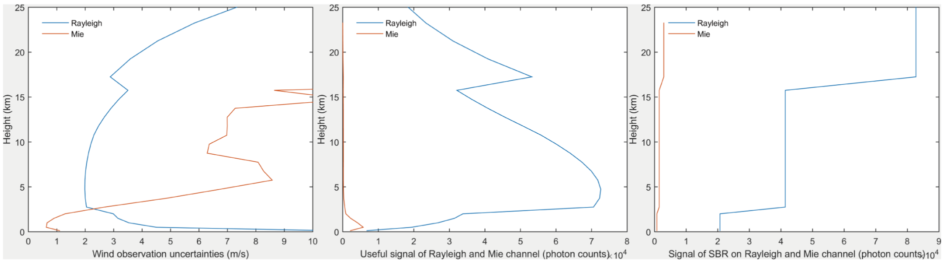

Fig. 1. Comparisons between the wind observations of Rayleigh channel and Mie channel. (a)Wind observation uncertainties; (b)useful signal of atmospheric backscatter; (c)signal excited by SBR.