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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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general comments

ESA's Earth Explorer Aeolus is the first satellite in space which measures globally wind profiles by use of a Doppler Wind Lidar (DWL). Due to its success, different scenarios of a follow-on mission are currently discussed. The paper contributes to this discussion. Sun-synchronous orbits with local time of ascending node (LTAN) of 15:00 and 12:00 of two additional Aeolus-type spaceborne DWLs are considered. The solar background radiation (SBR), seen by these satellites, is computed. It is found that SBR is increased due to the choice of the orbits. As a result, also increased Rayleigh channel wind errors, compared to Aeolus, are obtained for the two new satellites at cloud-free atmospheric conditions. The influence of an increased laser pulse energy of 80mJ is investigated to compensate the larger Rayleigh wind errors. In particular, a scheme is derived and applied to quantitatively design the required laser pulse energy of the new DWLs to meet specific accuracy requirements. Thus the paper addresses relevant scientific questions within the scope of AMT. It is of interest for the scientific community and should be published in AMT. However, there are some points which should be considered by the authors.

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.

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

(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.

(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.

(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.

(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.

(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?

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.

(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.

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

(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.

(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.

(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).

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

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(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.

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

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

(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?

(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.

(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 $1^{\circ} \times 1^{\circ}$ 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).

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

(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".

(22) In lines 298-299: Is the accuracy level of Aeolus, mentioned here, that one shown

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in Figs. 3 (a) and (b)? If so, please note this here.

(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.

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

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