

Atmos. Meas. Tech. Discuss., referee comment RC2  
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## Comment on amt-2021-260

Anonymous Referee #2

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Referee comment on "Inter-comparison of wind measurements in the atmospheric boundary layer and the lower troposphere with Aeolus and a ground-based coherent Doppler lidar network over China" by Songhua Wu et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-260-RC2>, 2021

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First of all I am very happy with this paper and the presented results. Especially the time evolution in Aeolus wind quality seen for the different baselines is impressive and valuable for other users.

Some detailed comments:

line 30:

you mention that you used data for different baselines. However, later in the paper for example on line 195 you only refer to a date for which Aeolus data was used. This makes it hard and in some cases impossible for users at a later time to know which data was used.

For example the date of 10-May-2020 will be part of the 2nd reprocessing campaign, and therefore the same day will be available for 2 different baselines (the NRT data stream which baseline was baseline 2B10 and the reprocessing baseline 2B11).

Therefore please state clearly which baseline and date you used if you refer to the Aeolus data for a specific date or period..

line 52:

\* typo: the emotion => the motion

line 68:

\* typo: in the worldwide => in the world

line 79:

\* typo: section 5 summaries => section 5 summarizes

page 5: table 1: what is DBS (measurement mode)?

line 163:

HLOS is compared to HLOS, and in section 3.2 you try to also take vertical velocity as observed by the CDL in to account. It is not yet clear to me how you apply this vertical velocity. In the example given in figure 5 it seems you apply the vertical velocity as measured at the exact time of the collocated Aeolus measurement. This I think is not correct. From figure 5 it can clearly be seen that there is a lot of variation in the vertical wind. This is probably due to convection present in the boundary layer giving sometimes updrafts and sometimes downdrafts. Aeolus accumulates over 15 to 85 km along track (in just of few seconds), so if the typical dimension of this convection is smaller than this averaging length, than the average vertical wind observed by Aeolus will be much closer to zero than what is observed by the CDL, and the net effect will be a broadening of the observed Doppler shifted spectral line only. So before applying a correction for vertical wind, first the typical size of the convective cells should be determined. Only then it can be decided if applying a correction is useful or not.

figure 8(a) shows a case where Aeolus data from 21-Jan-2020 is used. You should mention als here that the adaptive bias correction based on ECMWF data and M1 telescope temperatures that was added with baseline 10 was not yet in place for this date (even though you do explain this in a later section). This explains the noticable bias for the Rayleigh channel winds. For this date the NRT data stream was baseline 2B07.

line 255:

you mention that you apply a data selection using "the original standard deviation" but it is not clear to me what this is or how it is defined. Please give more details.

line 279: here you mention the different results for ascending and descending tracks of Aeolus data. This orbit phase depending bias has been largely solved by the adaptive bias correction based on ECMWF data and M1 telescope temperatures that was added with baseline 10. So If you only use data after 20-Apr-2020 (baseline 2B09 or newer) this should be much improved.

line 286:

you write:

"The baseline 10 dataset consists of baseline 09 data from April 2020 to October 2020 and the FM-B low bias reprocessed dataset of 2019"

this should be:

"The bias corrected dataset consists of baseline 09 data from 1 to 20 April 2020 and baseline 10 data from 20 April 2020 to 8 October 2020 and the FM-B low bias reprocessed dataset of 28 June 2019 to 31 December 2019."

summary tables 7a and 7b do not really belong to this study. It raises much more questions, i.e. are all these studies just looking at the boundary layer as well? (I guess not). How much data did they use? What extra quality controls did they implement, etc. My feeling is that comparing CalVal results from many teams should be published in another report or paper and not here.