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Comment on acp-2021-41

Anonymous Referee #2

Referee comment on "Intercomparison of wind observations from ESA's satellite mission Aeolus, ERA5 reanalysis and radiosonde over China" by Boming Liu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-41-RC2, 2021

The authors compare ESA's satellite Aeolus wind data with radiosonde winds in China in a period 20 April - 31 May. They also compare Aeolus wind fields with ERA5 wind fields. They use a fourth data set - "ECMWF wind fields" (need to be clarified, see below) as part of the Aeolus L2C data set - in a second period (July -Sept). Unfortunately, comparing all 4 data sets in an overlapping period was not possible. Numbers for correlations and mean differences are provided. Aeolus Rayleigh-clear winds and Mie-cloudy winds are considered separately. Conclusions are drawn by interpreting the various comparisons. They find that Aeolus winds are biased, and the bias is strongly different for ascending and descending orbits. They also find ERA 5 is biased over China. They find that a time difference criterion and a distance criterion does not seem to matter when they select Aeolus overpasses closest to the radiosonde start time and start location.

The figures 1, 3, 4, 5, 7, 8, 9, 10, 11 are illustrating their work and supporting their conclusions.

Major revisions are necessary before the paper allows the reader to understand what was done with which data and what conclusions can be drawn, and how relevant they are compared to what was already known.

Firstly, before publication, the authors need to describe exactly what the data are (versions), and who provided them. For instance, what is meant by "ECMWF data"? Were the background wind fields or the analysed wind fields used from the L2C product to compare to L2B? This makes a difference for the conclusion. Best support for their conclusion would be if they had used both background and analysis which would allow to illustrate how the change during data assimilation relates to Aeolus. Also, I assume L2C contains Aeolus obs error and data assimilation status flags, these would have been beneficial to consider in this paper. The L2B provided with the L2C data set contains Rayleigh winds only or also Mie, HLOS or components?

Also, it would be important whether the Aeolus winds used in period 1 are comparable to period 2 - were they obtained with the same L2B processor? Are we comparing same Rayleigh / Mie winds?

Rayleigh and Mie winds cover different vertical ranges. Comparing both should take this into consideration. Where height dependency is considered (e.g. as done in Fig 7 and 8) conclusions can be drawn more easily.

Second issue before publication, the authors need to describe in a reproducible manner their data processing. Figure 2 is not useful. The two periods, and each data set should be described separately, Figure 2 is in contradiction to the text. They need to state for each comparison, which data were (automatically) excluded, as this determines their resulting means and correlation coefficients. Also, in section 2.5 the wind components are discussed. Most discussion in the paper refers to the horizontal line-of-sight (HLOS) wind. It is not clear, where Aeolus wind components are needed and which numbers refer to wind speed or wind components or HLOS winds.

Third issue, conclusions from the various comparisons have to be discussed in a scientifically rigorous manner. What can and what cannot be concluded from the 2 periods? Actually ERA5 should be available in both periods. However, known seasonal dependency of Aeolus biases might limit the option of drawing conclusions from the spring and autumn period ignoring their different season. Also, the known dependency on topography is ignored here but might matter (compare Fig. 7, 850 hPa)

Fourth major recommendation concerns bringing the findings of the paper into perspective with what is known from other literature, e.g., bias of Aeolus (ascending/descending) was known before (simple google search brought me to https://doi.org/10.5194/amt-2020-404), and so are ERA5 biases over complex topography. In the abstract it is concluded that the findings give sufficient information to apply Aeolus wind products in numerical weather prediction in China. This surely might be a valid point, but needs a criterion what is meant by "sufficient information". Are both Rayleigh and Mie winds considered useful, or one more than the other, useful always or under certain circumstances? How does the size of differences between data sets compare to other literature? Discussing known literature will help to illustrate the added value of this paper, which is studying the region of China in detail.

Several of the experienced co-authors should be able to rewrite this paper in a more scientifically stringent manner.