Review – Aeolus – Liu et al
Special Issue: Aeolus data and their application (AMT/ACP/WCD inter-journal SI)

Scientific significant	- 3 (good). Aeolus intercomparisons are relevant.
Scientific quality	- 2 (fair). There are too many statistics repeated in the paper.
Presentation quality	- 2 (fair). The figures are not well-constructed.

1. Does the paper address relevant scientific questions within the scope of ACP?
    1. yes
2. Does the paper present novel concepts, ideas, tools, or data?
    1. Some new data
3. Are substantial conclusions reached?
    1. Not very substantial
4. Are the scientific methods and assumptions valid and clearly outlined?
    1. I am concerned that the processing of some of the data was in error.
5. Are the results sufficient to support the interpretations and conclusions?
    1. They are insufficient.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
    1. Yes, it is traceable (notwithstanding a possible processing error.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
    1. I liked the summary of the other Aeolus references. The authors should find references on relative observation impact experiments (e.g., Buehner et al 2018, Mon. Wea. Rev.).
8. Does the title clearly reflect the contents of the paper?
    1. Yes
9. Does the abstract provide a concise and complete summary?
    1. Yes
10. Is the overall presentation well-structured and clear?
    1. Yes, it is well-written overall.
11. Is the language fluent and precise?
    1. Acceptable
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

1. Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
    1. Many of the figures were not well-designed.
14. Are the number and quality of references appropriate?
    1. Yes
15. Is the amount and quality of supplementary material appropriate?
    1. Yes

General comments

Significant: The difference Aeolus-ERA mean zonal wind differences for ascending and descending show a significant negative bias on ascending (toward NW) passes and positive zonal wind bias on descending (toward SW) passes. Is there any corroboration of these results from any other studies? This is a critical result. As is discussed below regarding Fig. 8, it appears that the RS data may have their own bias, which would undercut the conclusions of this paper. I have a hard time concluding that the ECMWF data has such a zonal wind bias.

Fig.8 is potentially the most important result of the paper. Since ECMWF includes the Aeolus winds, but ERA5 does not, could one conclude that there is a bias with the RS winds? It appears that this is the case. How do the authors explain this issue? On p.13, L14-15, they suggest a possible ECMWF wind bias, but there is no other evidence that that is the case.

- I assume that Chinese RS data are used in both ECMWF and ERA5 analyses. Is this the case? The text should specify this clearly.
- The paper needs adding profiles for Aeolus winds (clear/cloudy/all) vs. ERA5. After all, Aeolus vs. ERA5 is shown in Fig. 9.

As noted below, I suspect a misprocessing, possibly vertical height assignment, for either the RS or ERA5 data.

I understand that these statements will likely result in rejection of the manuscript. I urge the authors to either find the suspected error or more carefully support their logic and then, resubmit the manuscript. I thank the authors for their work in this intercomparison for the important Aeolus data even though I find these problems.

The higher accuracy of Mie/cloudy winds than Rayleigh/clear winds are as expected – good. Can the authors provide correlation coefficients in Fig. 5 for other observation types? It's hard to tell what these numbers mean. I can only tell that they are from the same atmosphere at the same time with R* ~0.9.

Fig. 9 should use the same color scale for all graphics. This is confusing. Similarly, Fig. 7 should use the same color scale at least for the same vertical levels (850, 500, 100 hPa), but they don't. The colors in these graphics are confusing and should be applied uniformly.

Please remove national borders for areas of territorial disputes. The insert for the South China Sea area in the figures (1, 6, 7, 9, 10, 11) provide no scientific information whatsoever and should be removed.

Specific comments

p.2, L3-5. How is it known that this behavior is due to aerosols?

p.4, L1-2. The mean biases for A2D and Aeolus shown by Lux et al 2020 were *against the ECMWF model* – please correct.

The authors were not careful to ensure that the graphics all use similar scales. Instead, they simply used the range for each set of numbers rather than forcing a common range for the scales. Therefore, the colors mean different things for each plot. Fig. 5c does not use the same horizontal axis scale as 5a and 5b – please correct. Similarly, Fig. 5e does not use the same scale as Figs. 5d and 5f – again, please correct. Same problem for Fig. S1.

For all difference fields (Figs. 3, 7, 9, 10, 11), please use a white or gray color for differences close to zero, +/- 0.5 m/s for instance.

p.11, section 3. There is no need to repeat the statistics in this paragraph that are already obvious in the figures unless the authors want to draw some conclusion from those statistics. This paragraph can be shortened significantly.

p.11, L18. What data assimilation settings? Observation and background error values? If so, please say so.

Fig.7. What conclusions should the reader make from Fig. 7. There are some color differences the discussion on p.12-13 doesn't tell much except that the RS colors may very approximately match those from the other data sources.

Fig.8 other issues.

- Side issue: I suggest that the authors add the vertical profile of the number of Aeolus observations for Fig. 8. That may help solve the unexplained results in Fig. 8. Are there far fewer Aeolus observations for the 0-5km layer? I will guess that is the case.
- Is the altitude above sea level or above ground level? This should be added to the caption.
- What is the evidence that behavior in the 0-5km layer is affected by aerosols? This statement appears to be speculative.

- The paper needs adding profiles for Aeolus winds (clear/cloudy/all) vs. ERA5. After all, Aeolus vs. ERA5 is shown in Fig. 9.

Fig.9.

- The stronger winds in RS than in ERA5 (Fig. 9e,f) possibly evident (very hard to visually average this) at least matches the vertical profile in Fig. 8e,f. Why is it not possible that the RS values are in error? Another possibility is an error in the vertical elevation matching using higher elevation in ERA5 or lower elevation in RS. This would result in the ERA5 having stronger winds up to ~12km and lower winds above.

Fig.10.

- Please use the white or gray color for +/- 0.5 m/s. Then revise the number of sites for which difference is negative, i.e., less than -0.5 m/s.
- Fig. 10e and its geographical pattern suggests the possibility of a misprocessing since the RS sites with larger differences are to the west, perhaps with higher elevation.

Technical corrections

p.1, L27. It would be more accurate to say that the Aeolus winds observations were assimilated "into the ECMWF analysis", not "into the ECMWF winds" since the data assimilation is multivariate.
p.3, L18. Aeolus misspelled.
p.6. ERA5 misspelled.
The references are considerably out of alphabetical order – this is a problem for reviewers.
Fig. 9. 'orbits', not 'obits'. Same in p.13,L8. Please spell-check the entire article and remove any 'obits'.