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Comment on amt-2021-54

Anonymous Referee #1

Referee comment on "Validation of Aeolus winds using ground-based radars in Antarctica and in northern Sweden" by Evgenia Belova et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-54-RC1, 2021

In this contribution, the authors seek to validate horizontal line of sight (HLOS) winds from Aeolus against ground-based radar measurements in the Arctic and Antarctic. Compared to MARA in Antarctica and ESRAD in the Arctic, the Aeolus HLOS product generally compares favourably, with a significant bias documented in austral summer over Antarctica.

This well-motivated study seeks to address whether bias-corrected Aeolus data validates well against ground-based remotely sensed data in the polar regions. It represents a significant contribution in the effort to characterize Aeolus measurements in data sparse regions that are important for numerical weather prediction applications and for scientific understanding. The work is generally clearly presented and laid out and within the scope of AMT and the Aeolus special collection. My comments do not suggest any major changes are necessary to bring the paper to publishable form. I therefore suggest minor revisions to the current ms.

The main analysis recommendation is to see if more insight can be gained by digging further into the origin of the differences seen between ESRAD and Aeolus, in particular to what extent biases in ESRA might account for those differences (versus the lack of coincident data, particularly in the Mie channel, which is clearly described). Regarding ESRAD biases, taking a quick look at Belova et al. (2020) AMTD (https://amt.copernicus.org/preprints/amt-2020-405/amt-2020-405.pdf), the differences of ESRAD from radiosonde, HARMONIE-AROME, and ERA5 are complex, and I couldn't find a clear statement in it about recommended bias correction to ESRAD (apologies if I missed it). I was looking for this because the nature of and rationale for the bias correction made to ESRAD in the current paper (e.g. p.7, line 85-87) is not entirely clear. At p.4, ll.14-16, what does it mean to say that there is a 'systematic underestimate of wind speed by about 8% in zonal wind and 25% in meridional wind'. Wind speed is a scalar, so is the issue that ESRAD winds are weaker (lower speed) than the other products? From the other Belova et al. (2020) paper in AMTD, it seems like these statistics refer to separate linear regressions carried out for U and V. Is this suitable to apply when comparing ESRAD to an HLOS product like Aeolus? Again, why separately correct zonal and meridional winds

rather than wind speed and direction? Have the authors done a separate analysis of the comparison of ESRAD to Aeolus without the Belova et al. 2020 bias corrections on ESRAD? If so, what does this reveal?

Also regarding ESRAD, it is interesting that the fcx_aeolus was implemented as a special effort for the calibration/validation effort (p.7,l.78, and Table 1). But apart from the mention on p.7, a separate analysis of this data does not appear. Were systematic differences were found for this mode?

The main textual revisions I recommend are to clarify in the abstract and elsewhere that the analysis is based on a single six-month period of 1 July-31 December 2019, to clarify what the nature of the 'winter' and 'summer' seasons are here, and to discuss the implications of the use of a single season to characterize these errors. The reader might assume from the abstract that the analysis would take place over the entire Aeolus period instead of just when the homogenized and reprocessed 2B10 data was available. The authors could expand on their justification of only including this data in their analysis at p.3, line 84. In addition, while it is ok to characterize 1 July- 24 September (24 September - 31 December) as boreal (austral) 'summer', the complementary periods are shoulder seasons (boreal autumn/austral spring) and not 'winter'. This nomenclature is used when the authors interpret some of the wind biases in terms of winter-versus-summer seasonality (p.12, ll.63-65; p.16, ll13-14). This interpretation should mention that results could be influenced by a small number of weather systems that happened to occur at these sites during the six-month analysis period.

It is clear that the amount of sunlight distinguishes the two periods (p.8, ll.04-05) and the reason to separate the periods in this way is to focus on the role of insolation backscatter in controlling errors. So could the authors call the 'winter' period something like the 'non-summer' or 'non-sunlit' period?

Specific comments:
* p.2, l.43: Clarify what is meant by 'hot pixels'.
* p.2, l.55: Is this comment necessary for this paper? Perhaps it would be better placed in the discussion. The description of the 'limitation' of the Aeolus orbit design is distracting. The sun-synchronous orbit presents a challenge for calibration/validation but it is a reasonable strategy for capturing free tropospheric/lower stratospheric winds whose diurnal cycle is relatively weak, especially on the typical horizontal measurement scales of 10-100km achievable by this technology.
* p.3, l.78: to about *an* 87 km horizontal
* p.3, l.79: for better impact on weather prediction -> to improve the impact of the retrieved Aeolus winds on numerical weather prediction
* p.6, l.38: ">8 m s^-1": While it seems reasonable, how was this rejection threshold chosen, and what impact did it have on the results?
* p.6, starting l.45: Replace "Mie winds" with "Mie wind measurements"
* p.7, Table 1: Is there a typo in the end heights (104km, 100 km) - and if not is this consistent with the descriptions in Section 2?
* Figures 3 and 6 show strong negative values for ascending HLOS in Aeolus but not in MARA; can the authors comment on these outliers? These extreme differences might be worth pointing out. Is it possible that there are ranges of horizontal wind speeds that

aren't captured by MARA's retrieval?
* p.12, l.63: The wording is confusing here, suggest "... do not vary between the seasons and weater systems are more variable in winter rather than in summer, which would lead to more spatial variability in winter than in summer". But again, as pointed out above, it is speculative to make this kind of generalization when only analyzing a single season, especially since there is a lot of synoptic variability in Antarctica year-round.
* p.16, l.10: "For winter the random differences are higher than in the comparison with radiosondes." Could this be quantified?