Response to comments of Anonymous Referee #1 on *AMT* preprint "Exploiting Aeolus Level-2B Winds to Better Characterize Atmospheric Motion Vector Bias and Uncertainty" by Katherine E. Lukens et al. Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-277-RC1, https://doi.org/10.5194/amt-2021-277-RC2, https://doi.org/10.5194/amt-2021-277-RC3, 2021

Thank you very much for your careful and helpful comments. We have revised our manuscript following your suggestions. We greatly appreciated the opportunity to iterate with you on some of your comments during the discussion period.

[Below, quotes from your comments are repeated verbatim in *green italic*. All your comments from the discussion are included here. Comments made by us and by you that are resolved are included here but are indented. Changes in quoted text are indicated as additions and ~~deletions~~. Comments from author to author are in this color.]

General comments

The paper is important and covers an important Task since Aeolus was launched: the comparison of Aeolus Winds with AMVs, for the continuous improvement of both sources of wind observations. The paper should be accepted after some a minor revision/correction.

Thank you.

Please consider all following items.

We have.

Specific comments - Important comments

1a. Please include a description, including a formula, of the Statistics used in your paper (MCD, SD, SDCD,…; possibly also the Speed Bias, Speed SD, RMS, Vector Diff, Vector RMS included in Table 1), and their relationship with the Statistics used as Standard procedure for AMV validation defined by the International Winds Working Group in its 1996 Workshop (http://cimss.ssec.wisc.edu/iwwg/iww3/index_3rdWindsWorkshop.htm) in following report: http://cimss.ssec.wisc.edu/iwwg/iww3/p17-19_WGReport3.pdf.

This is a good point. Following our discussion with you in the open forum, we now add,

"Because we are comparing the AMV and Aeolus HLOSV, a scalar quantity, our statistics can only be analogs of the standard one. We include the formulae for all the statistics in Appendix A."

And we have added that appendix.

This is important; if I look throughout the internet "Mean collocation differences (MCD)", I find very few references, and all of them are from the year 2021.

We define MCD when first used and its computing formula is included in Appendix A.

1b. Why were the original Statistics for winds in this IWW3 Report not used?

See our response to 1a.

1c. The collocation criteria used between AMVs and Aeolus winds in lines 180-183 are also different to those defined by the International Winds Working Group in its 1998 Workshop (https://cimss.ssec.wisc.edu/iwwg/iww4/index_4thWindsWorkshop.htm) in following report: https://cimss.ssec.wisc.edu/iwwg/iww4/p19-20_WGReport3.pdf. Please comment on the differences on both criteria, and if you could expect some impact in using one or the other procedure.

The manuscript now states, "Our choice of collocation criteria is conservative compared to those defined by the IWWG 1998 workshop (Velden and Holmlund 1998). Although the larger time and distance criteria defined by IWWG (90 vs 60 minutes and 150 vs 100 km) might retain more collocation pairs and thus a larger sample, the collocated winds would more likely have larger MCD and SDCD. Our smaller time and distance criteria restrict the number of possible Aeolus matches to any one AMV and help avoid Aeolus matches from two different orbits. The IWWG height criterion is a fixed pressure difference (25 hPa) that might be too small at lower levels where pressure layers are tightly spaced in elevation but too large in the upper atmosphere where the elevation distance between pressure layers is much larger. Our height criterion is based on a $\log_{10}$ scale and accounts for the varying distances between pressure layers throughout the vertical and corresponds to pressure differences ranging from approximately 300 to 1 hPa for pressures from 1000 to 10 hPa, respectively."

2a. There is an important error in lines 48-50: with the text: "(1) water vapor cloud-top (WVcloud) channels are used to track upper-level cloud top motions, and (2) water vapor clear-sky (WVclear) channels are used to detect upper-tropospheric features (e.g., jet stream and waves) by tracking water vapor motions in clear air you seem to say that some WV channels are used for Clouds and other ones for Clear sky, and this is completely wrong. The same channel (f.ex. WV062) can be used to calculate Cloud AMVs (tracking clouds) and Clear air AMVs (tracking moisture patterns in other parts of the image). Please correct the text.

Done. The manuscript now states,

"Infrared bands that are specifically sensitive to water vapor (WV) absorption can capture different atmospheric motions using the same channel by tracking ~~in two ways:~~ (1) ~~water vapor cloud-top (WVcloud) channels are used to track~~ upper-level cloud-top motions, and (2) water vapor motions in clear air ~~water vapor clear-sky (WVclear) channels are used to detect~~ related to upper-tropospheric features (~~e.g.,~~ including the jet stream and atmospheric waves) ~~by tracking water vapor motions in clear air~~ (Velden et al., 1997)."

2b. Related to the previous comment, please change throughout all the text all expressions "AMV channel" and "AMV channel type" to "AMV type", to avoid the same error. And for example change in line 142-143: "IR, WVcloud, and WVclear channels" to "IR, WVcloud, and WVclear AMV types". Please be careful with this.

The manuscript now refers to type everywhere.

3. Lines 73-74: "Such a direct comparison has not previously been possible due to the sparse spatial coverage of other available reference datasets, e.g., rawinsonde winds". I am not so sure you cannot use rawinsonde winds for this; please provide more detail to this sentence, so that you can conclude what you say.

Agree. The manuscript now states, "Such a direct global comparison has not previously been possible due to the ~~sparse~~ limited spatial coverage of other available reference datasets, e.g., rawinsonde winds, which are mostly available in the Northern Hemisphere over land."

4a. Line 110: please try to give more detail or reference about why in spite of the "M1 bias correction", there are still some "remaining biases" which you mention in line 26.

Done. The manuscript now states,

"While the M1 bias correction is capable of considerably reducing the telescope-induced wind bias, some residual bias may remain, e.g., in cases where the top-of-atmosphere reflected radiation strongly influences the telescope temperature (Weiler et al., 2021). Additionally, residual biases may remain in part due to potential calibration issues of the Aeolus L2B winds that could in turn lead to biases between Aeolus and NWP background winds (Liu et al., 2022)."

4b. Line 110: please give some detail or reference about the "Quality Controls" used in Aeolus.

Done. The manuscript now states, "They found that with the application of the M1 bias correction and proper quality controls (QC) (see Rennie and Isaksen (2020a) or Section 3 for specific QC criteria used in this paper) as well as Aeolus black-listed dates taken into account…".

4c. Line 110: please give some detail or reference on why there are "Aeolus black-listed dates".

Done. The manuscript now states, "… Aeolus blocklisted period (defined as a period of time when the Aeolus dataset is known to be degraded and should not be included in research or operations).".

5a. What is the "horizontal/vertical accumulation lengths" you mention in lines 198-199?

The manuscript now states, "Horizontal and vertical accumulation lengths refer to the horizontal and vertical distances over which individual measurement signals are accumulated and averaged to improve the signal-to-noise ratio. In this way, the Aeolus observations represent wind volumes and not discrete points or levels. The accumulation lengths can vary and depend on the processor settings."

5b. What is the "L2B uncertainty" you mention in line 201?

The manuscript now states, "L2B uncertainty refers to the Aeolus HLOS wind error estimate assigned to each wind measurement."

6. Differences in time and location between the two observations can be not so important as the ones you mention in lines 230-231: "differences due to collocation (i.e., due to different times and locations 230 of the two observations) could play a role in increasing the differences between the collocated HLOS winds". Please check as an example: "Chapter 2.3 IMPACT OF THE REPRESENTATIVITY OF THE RADIOSOUNDING WINDS" in following reference: https://www.nwcsaf.org/AemetWebContents/ScientificDocumentation/Documentation/GEO/v2016/NWC-CDOP2-GEO-AEMET-SCI-VR-Wind_v1.0.pdf. Then comparatively check how much the wind can change considering the time and distance implied in your collocations, and update your sentence if needed.

We have deleted this sentence.

7. Important: In chapter 4.1.2 line 419 you say you compare only IR cloud AMVs and WV cloud AMVs with Aeolus Mie Cloud winds. Why don't you do the same in chapter 4.1.1, comparing Aeolus Rayleigh Clear Air winds with WV clear air AMVs only? Please include in the text an explanation of why you are acting differently in both chapters. Evaluate also if there could be two different elements here, which behave differently:

- flow related to cloud features, evaluated by both Aeolus Mie winds and cloudy AMVs,

- and flow related to clear air features, evaluated by both Aeolus Rayleigh winds and clear air AMVs.

We now add, "To increase the size of our collocation data set, we compared all types of GOES-16 AMVs to both Rayleigh-clear and Mie-cloudy winds. In addition, we do not show results from WVclear AMV collocations with Mie-cloudy winds as correlations for this category of collocations are poor and the sample size is very small (see Table 1), and this result may be unreliable. With a larger data set it might be possible to compare Rayleigh-clear and Mie-cloudy winds to clear and cloudy AMVs only, respectively."

In RC2 you wrote:

This sentence is better, but I think it still misses some indication to the fact that wind observations obtained from clear air and cloud AMVs do not behave exactly in a similar way. For example, it is a recognized fact inside the AMV community that time scales of winds related to clear air AMVs are longer (30-60 minutes) than the time scales of winds related to cloud AMVs (10-15 minutes), and this is enough so that show some differences.

The discussion included the following exchange:

> Thank you for making the point about AMV time scales. We will add additional information on AMV time scales to the manuscript, and we plan to cite one or more of the following supporting references. Can you recommend any peer reviewed references that could be used?
>
> De Smet, A., 2002: Operational AMV products derived with Meteosat-6 rapid-scan data. Proc. of the Sixth Int. Winds Workshop, Madison, WI, WMO, 179-185. Available online at http://cimss.ssec.wisc.edu/iwwg/iww6/session4/deSmet.pdf.
>
> Schmetz, J., K. Holmlund, H.P. Roesli, and V. Levizzani, 2000: On the Use of Rapid Scans, Proceedings of the Fifth International Winds Workshop, Lorne, Australia, 28 February – 3 March 2000. EUM P28, Published by EUMETSAT, D-64295 Darmstadt, 227-234. Available online at http://cimss.ssec.wisc.edu/iwwg/iww5/S5-2_Schmetz-OnTheUse.pdf.
>
> *The references you have found are very fine. Although they are already 20 years old, the conclusions they extracted are completely valid today. It was more or less in the era that the "AMV time scales" were defined and understood. About finding a "peer reviewed reference", general research related of AMVs in general is found in the publications of the "International Winds Wokshops" (http://cimss.ssec.wisc.edu/iwwg/iwwg_meetings.html), but beyond this, much information from these Workshops is not published in peer-reviewed journals (due to costs or the time needed to publish a paper). What I would do here:*

- Proceedings from the International Winds Workshops are a perfectly valid reference, and the IWWG has the interest to keep them online throughout the time.

- Because the main reference you use (Velden, C., D. Stettner, and J. Daniels, 2000) is not available anymore online (only an abstract), I would include the second reference from the 5th International Winds Workshop (Schmetz, J., K. Holmlund, H.P. Roesli, and V. Levizzani, 2000), whose title is atemporal and unrelated to any specific satellite. This can be valid now and in any coming future.

Thank you for making the point about AMV time scales. We also now add, "Additionally, winds retrieved from tracking clear-sky and cloud motions represent different dynamical features and tend to behave differently. For example, the recommended time interval for tracking cloud motions is 10-15 minutes to capture short cloud lifetimes and rapid intensification/deformation, while the recommended time interval for clear-air motions of 30 minutes is suitable to capture variations in jet streams and other clear-air features (Schmetz et al., 2000)."

Technical corrections.

1. I do not like very much the expression "enhanced wind shear". "Enhanced" reminds me of the word "Improved", which has nothing to do with the wind shear. I would prefer something like "strong wind shear" (throughout all the text). Please update or comment back.

Agree. Fixed.

2. You use "rawinsonde" and "radiosonde" in different parts of the text. I assume you are referring with both to the same. Aren't you? This way, use the same word all throughout the text for homogeneity (the one you prefer).

Yes. We now use *rawinsonde* everywhere.

3. Line 150: I think you are listing all the LEO satellites and radiometers you are using. So I would remove the text: "including but not limited to".

Agree. Fixed.

4. Tables 2 to 5: your options "Bolded and bolded/underlined statistics" do not differentiate well in the text. I suggest to use "Cursive and cursive/underlined statistics" to appreciate better the differences.

Agree. As an alternate solution, we now replace Tables 2-5 with new Figs. 2-3 that display the information graphically.

5. In lines 321-322, I think the clouds in "Surface effects may also play a role, as very cold brightness temperatures at or near the polar surface may be misinterpreted as very high cloud tops due to the low temperature contrast between clouds and the surface snow or ice" can actually be located in any height. I would simply change "very high cloud tops" for "cloud tops". Please consider.

Agree. Fixed.