

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 again for your careful and helpful comments. We have revised our manuscript following your suggestions.

[Below, quotes from your comments are repeated verbatim in *green italic*. Changes in quoted text are indicated as [additions](#) and [deletions](#).]

This is a comment by Anonymous Referee #1, on the questions defined by the other Referee in the first round of review of this paper.

He/she has not answered to the review of this paper in this second round, which is a surprise to me due to the important review process which was requested by him/her in the first round.

I am checking if all his/her questions have been answered satisfactorily. Please reconsider those elements defined in [yellow](#) in a small editorial further review.

Main comments

- *“I feel a scientific paper requires a clearer interpretation of these statistics than is presently provided”*

→ *The paper has been reviewed with detail, and I consider the information provided in the latest version is clear enough, and not prone to doubts. It can be published taking into account the few elements remarked in yellow below.*

Thank you. We have considered all points and have revised our manuscript accordingly.

- *“One stated goal of the paper is to guide improvements to AMV quality, but it is not clear to me whether the paper indeed provides new insights into where improvements can be made. It should be possible to address these aspects through a very major revision of the text, particularly in the results and conclusions sections, though some further analysis may also be required to draw firmer conclusions”*

→ *Please consider what is said in “Main general point 2”*

Main general points:

1. *About “The paper needs to be clearer on which new insights the study provides and which overall conclusions can be drawn; One stated goal is to improve AMV quality. What do we learn about this from the study? Could any of these features be investigated with the collocation dataset,*

hence addressing the stated goal of the paper to aid the development of AMVs?”

→ As said above, The paper has been reviewed with detail, and I consider the information provided in the latest version is clear enough, and not prone to doubts. The other reviewer is asking for several additions about “how to improve AMV quality and aid the development of AMVs”, but I consider this can be outside of the scope of this paper, and additionally, this paper needs to be published soon so that corresponding findings can be available to everybody.

→ My suggestion here could be: if you have the possibility and the interest, think of writing another paper later which can be based on these ideas: “Could any of these features be investigated with the collocation dataset, hence addressing the stated goal of the paper to aid the development of AMVs? How should affected AMVs change to improve their quality using Aeolus winds as reference?”

Thank you for this suggestion. We will certainly consider writing an additional paper addressing these topics.

2. About “What is the basis for stating that “AMVs compare well to Aeolus winds”? What does “well” mean in this context? It appears that the authors compare Aeolus/AMV difference statistics directly to values from AMV/sonde or AMV/NWP comparisons, despite very different uncertainties in the respective comparison datasets or the collocation methods.”

→ All the paper has been rewritten, and this ambiguous comment “AMVs compare well to Aeolus winds” has been fully removed from the text, specifying clearly how AMVs/Aeolus winds match and differ, and what can be the cause to these differences. With this, I think this point has been solved.

3. About “The statistics presented are affected by collocation/representation error, as well as biased sampling, and my impression is that this may play a considerable role. This aspect should be discussed and, if possible, an attempt at quantifying the magnitude of these aspects should be made”

→ This question has been thoroughly answered in lines 212-270.

Specific points:

- 1. The sentences have been rewritten, and I do not see contradictions anymore.*
- 2. The abstract has been reduced, removing the mentioned lines from the text.*
- 3. The text has been rewritten as requested.*
- 4. UTC changed for LT as requested.*
- 5. The text has been changed and explained in more detail, such as requested by the reviewer. However, I see in the text two “blocklisted” in lines 86 and 121 that should be changed to “blacklisted”.*

We have added a footnote about this terminology in the manuscript at line 86:

[“The meaning of the term “blocklist” is identical to the term “blacklist”; however, “blacklist” has racist connotations \(Conger, 2021\). The term “blocklist” is intentionally used in an effort to support the usage of more neutral computing terminology in scientific](#)

research; in fact, the Aeolus project has already adopted this new terminology and refers to the list of dates when Aeolus data should be excluded as “blocklisted” dates.”

6. *The text has been changed as requested and presented in a table with more information, such as suggested. However, I see here again that you consider again “water vapor cloudy channel (WVcloud), and water vapor clear channel (WVclear) AMVs” as if coming from different satellite channels. As I already said in my first review, WVcloud and WVclear can be calculated using the same WV channel, tracking different features (clouds or moisture). Rephrase again here and in any other part of the text where this occurs again as: “water vapor cloud AMVs (WVcloud), and water vapor clear air AMVs (WVclear)”*

Thanks for catching that. All instances of this phrase that appeared in the text have been rephrased accordingly.

7. *The content has been changed such as requested, making clear the big variability of results considering different processing centres and circumstances. I think the information is successfully provided in a more qualitative way, such as requested.*
8. *Table 1 has been removed, and so this comment does not apply anymore.*
9. *The text is more clearly presented and easier to understand.*
10. *The question has been answered in the text.*
11. *The question has been answered in the text.*
12. *Please specify also in line 145 that you are using the “forecast-independent QI”.*

Done.

13. *Please include the clarification “1.25° (approximately 140 km in the N-S direction)” in both Figures 4 and 5.*

Done.

14. *Tables 2-5 have been changed to Figures 2-3, which are clearer and easier to read.*
15. *Answered in “Main general point 2”*
16. *Text in the figure has been made bigger, and now it can be seen better.*
17/18/19. These three comments define again an important question I made in the previous review round: it could have been more useful to compare Clear air AMVs with Rayleigh/clear air Aeolus winds only, and Cloudy AMVs with Mie/cloudy Aeolus winds only. The study would have been more helpful.

You already gave your reasons to do what you did, and now it would be very late to change all this. But at least you could define with some more detail why you did this way (why you could not do as a I say above).

In your text, the only explanation I find is in lines 376-380, which say “To increase the size of our collocation dataset we compared all types...” and “With a larger dataset it might be possible to compare Rayleigh-clear and Mie-cloudy winds to clear and cloudy AMVs only,

respectively”. I find this explanation weak, considering the implications this has had in the results.

We now include the following text in Section 5 of the manuscript:

“Future studies should use larger datasets like those the authors are preparing to compare clear-scene AMVs with Aeolus Rayleigh-clear winds only, and cloudy-scene AMVs with Aeolus Mie-cloudy winds only. Such studies are anticipated to yield additional insights into the seasonal performance of AMV characteristics representing different dynamical features in clear and cloudy scenes and how this might be accounted for or improved upon in AMV algorithms. Moreover, the robustness of dynamical features identified in AMV monitoring could be further validated following this approach. In addition, Aeolus Mie-cloudy comparisons using larger datasets are expected to have a significant impact towards improving our understanding and characterization of AMV quality in cloudy scenes, given the cloudy/cloudy sampling effect and the small contribution of Aeolus Mie-cloudy error to the total SDCD throughout the vertical in all geographic regions, implying that the corresponding adjusted SDCD better depicts true AMV uncertainty. This is especially critical where AMV height assignment errors are likely large but Aeolus Mie-cloudy errors are small and remain relatively constant with respect to height and AMV wind speed, e.g., in layers of strong vertical wind shear and in the SH.”

Beyond this, all the paper has been rewritten following “Main general point 2”, specifying clearly how AMVs/Aeolus winds match and differ, and what can be the cause to these differences.

20. *I agree with the conclusion provided.*
21. *I agree with the conclusion provided.*
22. *Answered in “Main general point 2”*
23. *Answered in “Main general point 2”*
24. *Answered in “Main general point 2”*
25. *I agree with the conclusion related to this provided in lines 672-680.*