Response to Review #2

We also thank anonymous reviewer #2 for the constructive comments and suggestions to the manuscript. Here our response to the comments. The response is given within the %%%--- ---%%% symbols. In addition to the points raised by both reviewers, we found small errors in the calculation of the spectral ratio in Table 2 for all corrections and sharpened criteria for the CSAT3 at Risø, which have now been corrected.

Regards,
The authors

<u>General Comments</u>

In this paper, the authors present a novel methodology to evaluate the relative accuracy of u, v, and w measurements from a sonic anemometer by applying Kolmogorov theory to the relative magnitude of the u, v, and w spectra within the inertial subrange. Based on that theory, the v and w spectra should be 4/3 the magnitude of the u spectra. Using field data from Metek USA-1 and CSAT3 sonic anemometers at different towers, different field sites, without any shadowing correction, with shadowing correction, and with path averaging correction, the 4/3 relationship was tested. For the Metek, while the uncorrected anemometer was much lower than 4/3 for the w-to-u relationship, after applying a wind tunnel based calibration provided by the manufacturer this relationship becomes∼4/3. For the uncorrected CSAT3, the w-to-u relationship is a little close to 4/3 than was the case for the uncorrected Metek, but after applying a shadowing correction, the ratio is still lower than 4/3, being ∼1.2. Thus, the CSAT3 correction could be interpreted as providing only a partial amount of the correction required to achieve a 4/3 relationship.

%%%--- Thanks for this comment. This is a good way to summarize the main findings of our analysis ---%%%

This is a very novel idea, and has the potential to be fairly influential in the discipline. There are two items that seem very important that should be given more emphasis. First, this technique is among the few that does not require a comparison between one anemometer and another. Rather, it can be applied for any single anemometer at more-or-less any field site (I recognize that this is mentioned in the paper, but this is EXTRMEMELY important, so make sure there is no doubt the reader appreciates how powerful this statement is).

%%%--- We agree with the reviewer. We now include the following sentence in the abstract "and does not require the use of another measurement as reference" to further highlight the advantage of the method ---%%%

Second, because this methodology is entirely based on Kolmogorov theory about the 4/3 ration between w-u and v-u spectra, it should be emphasized that in general all of the results from the v-u tests conform to this theory. While the reader must evaluate results such as Table 2 to determine whether w-u ratios of ∼1.0,∼1.1,∼1.2 are evidence of underestimated w measurements, it is crucial to note that the v-u ratios are almost entirely 4/3 for all cases. This gives a lot of credibility that the theory of isotropy is correct, and that the 4/3 standard is reasonable.

%%%--- As suggested by the reviewer, we now state for each of the cases analyzed that the v to u spectra ratios are always close to 4/3 irrespectively of the correction type or criteria used for filtering ---%%%

One improvement that will be necessary is to better clarify the differences between 4/3, 5/3, and 2/3 slopes in the inertial subrange. I was completely confused upon my first reading, and I had to consult my Kaimal and Finnigan (1994) book to sort this out. While it is clear from Equation 1 that the slope of the inertial subrange is -5/3, figure 1 is a -2/3 slope, which presumable is because it is a "frequency weighted" spectra where the y-axis is actually spectra multiplied by wave number. This is related to second sentence in section 5.1, where the frequency-weighted spectra is multiplied by kˆ(2/3) to give a straight line. This is entirely confusing unless the reader recognizes that it is already multiplied by kˆ(3/3), which is essentially kˆ(5/3), i.e., it makes the -5/3 slope in Equation 1 appear as a flat line. Yet, all of these details are rather unimportant when compared to the 4/3 value in equation 2, which is actually written as 3/4, but constitutes the most important relationship of the paper. This is the assumption from which all of the conclusions of this paper are drawn from. In summary, the reader should not have to consult Kaimal and Finnigan (1994) to sort out the meaning of the different ratios in this paper.

%%%--- We understand the readers could be confused. We now include a statement in the caption of Fig. 1 so that the reader understands why the slope of this wavenumber pre-multiplied spectra is -2/3. When first presenting the computed spectra (for the USA-1 at Risø), we also add that these are flatten by multiplying by k1^2/3 in contrast to the spectra in Fig. 1 ---%%%

I would recommend that more quantitative techniques be used to ensure that spectra included in the analyses conform to the theoretical -5/3 slope. On suggestion is that rather than only fitting a 0th order polynomial, a better method is to fit a 1st order polynomial with statistical software, then to test for the statistical significance of the 1st order coefficient. If, for example, the p-value for the 1st order (i.e., slope) coefficient is > 0.05, it could be considered non-significant, and then it could essentially be concluded that the slope of the inertial subrange cannot be distinguished from -5/3. Once this is done, using arguments similar to model selection analysis, the 1st order polynomial model can then be reduced to the 0th order model currently described in the paper. In a manner, the goal of this is similar to the "sharpened" criteria in section 5.2/Table 2. The benefit of the statistical criteria is that it provides a defensible justification as to which 10-min periods should be included in the analysis based on their inertial subrange. Another similar idea could be to use a statistical break-point or change-point analysis for each 10-min period to determine the range over which the inertial subrange slope is -5/3 (i.e., use statistics to optimize the range of the inertial subrange for each 10-min period). This seems more complicated to me, but it could also work.

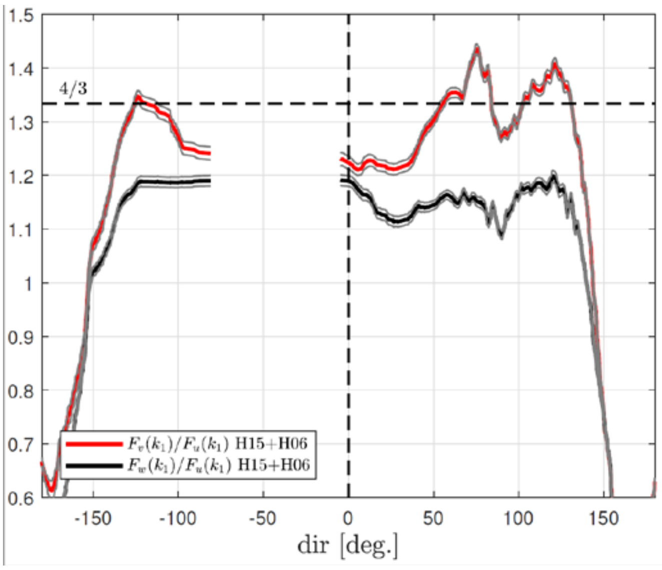
%%%--- Although we understand that the selection of the wavenumber range did not appear quantitative, we have chosen not to follow this recommendation. As the reviewer points out, the goal of the recommendation is similar to our "sharpened" criterion, and we have chosen to focus on this selection, First, we now include the sharpened criteria to all sites in Table 2. Second, we now clarify at the beginning of section 5 (third paragraph) that the sharpened criteria are indeed used to filter out 10-min samples where the spectra do not follow the expected behavior within the inertial subrange. The strength of the sharpened criteria is that one might have slopes close to -5/3 outside the inertial subrange and so the uw-co-covariance test aids in determining the closeness of the selected range to conform to isotropy. As stated in the Discussion, several other selections have been tried with no difference in the result. See also our answer to the reviewer's specific comment "Page 11 line 22" regarding the choice of the thresholds for the sharpened criteria: there we show the sensitivity of the results to the thresholds in the sharpened criteria. ---%%%

A more quantitative approach than a running mean such as in Figures 4, 5, 7, and 9 or mean/standard deviation such as in Table 2 would improve the interpretation that the results are statistical different/similar to 4/3. One suggestion I have for the figures is to replace the running mean with a local (i.e., LOESS or LOWESS) regression.
This is a statistical technique that provides results similar to a running mean, but it also comes with confidence intervals. Thus, a similar figure could be produced, but with the added benefit that for any wind

direction it can be tested whether or not the best fit line is significantly different from 4/3. A great example is figure 7 (right frame) where obviously for some wind directions neither the red or black lines are even close to 4/3, but for other directions the red is similar to 4/3. A LOESS fit would give a quantitative metric to determine where this is significantly different from 4/3 and where it is significantly similar.

%%%--- We understand the suggestion of the reviewer. We have actually performed loess fits for the USA-1 at Risø and for the CSAT3 at Nørrekær Enge in the original manuscript (so it is now stated that these are the fits) and the estimation of the standard errors of such fits in Figs. 4, 7, and 9 and found that they were very small and difficult to discern when plotted besides the fit. For completeness, we now anyway provide some numbers related to such standard errors in the caption of the above mentioned figures ---%%%

%%%--- We agree with the suggestion of the reviewer. We have now implemented loess fits for all sites. In addition, we have now performed the estimation of the standard errors and confidence intervals of the fits for the values in Figs. 4, 7, and 9 and found that they were very small and difficult to discern when plotted besides the fit. For completeness, we now provide the standard errors in the caption of the above mentioned figures. When adding the 95% confidence interval, we have to assume that all the observations are independent, which is likely not the case. However, the observations are indeed significantly different from 4/3 for most wind directions; please see the example below for the CSAT3 at the Risø site (the grey lines show the error range for the 0.95 confidence interval) ---%%%



I would encourage the authors to reconsider their interpretation of the Huq et al. (2017) paper. While it is correct that those results suggest a magnitude of correction similar to Horst et al. (2015) (i.e., 3-7% as mentioned in their abstract, or ∼6%, which is the average of second column of their Table 2), one important distinction is that when Huq et al. (2017) applied the Kaimal (1979) and Wyngaard and Zhang (1985) corrections to their numerically simulated data, the improvement in relative error was rather small (i.e.,2.4-3.4% correction, as derived from their Table 2). Frank et al. (2016) presented data from seven sites around North and Central America where these corrections increased the w measurements by 4.5-6.8% (their Table 2). While it takes a bit of interpretation to compare the results from these papers, one interpretation could be that the numerically simulated turbulence in Huq et al. (2017) tends to produce corrections (either Kaimal or Wyngaard) that are less than what are typically observed in nature. Thus, while an overall correction of ∼6% is similar to that of Horst et al. (2015), the Kaimal/Wyngaard correction only accounts for ∼50% of this. From this perspective, the findings of Huq et al. (2017) are very similar to those of this paper, which is to say, the currently accepted CSAT3 corrections do improve w measurements, but perhaps only provide a portion of the correction that is ultimately required.

%%%--- In the introduction, we would not like to go beyond the interpretation given by Huq et al. (2017) when they summarize their findings, and therefore, we have left the text as it was in the original submission. In the Discussion we have changed the formulations to be more precise (the new version is included in the bottom of this answer).

The abstract of Huq et al. (2017) states that *"A comparison of the corrections for transducer shadowing proposed by both Kaimal et al. (Proc Dyn Flow Conf, 551–565, 1978) and Horst et al. (Boundary-Layer Meteorol 155:371–395, 2015) show that both methods compensate for **a larger part** of the observed error, but do not sufficiently account for the azimuth dependency*."

Further, the authors state in the last paragraph on page 23: "*For the standard deviation of the w-component, Horst et al. (2015) report a relative error of between 3 and 5%, which is **almost the same** as our error. We suspect the error from our numerical experiment is slightly larger because the turbulence intensity is not quite as large as in the field, where more intense turbulence tends to weaken flow-distortion effects.*"

We have written *"Huq et al. (2017) presented a novel approach for estimating the accuracy of the CSAT3 by using numerical simulations. The results of the study pointed to flow-distortion errors of **similar** magnitude as those in H15."* and suggest that this is an accurate representation based on the above quotes from Huq et al. (2017). As we state in the manuscript, our results can only be used to quantify the sonic anemometer error, if we make assumption on how the different velocity components are affected. Therefore, it is hard to say whether the findings by Huq et al. (2017) agree with our results

The section 6.2, where the Huq et al. (2017) paper is again cited has been reformulated to:
"If we assume that the discrepancy to 4/3 is due to remaining uncorrected flow distortion and further, that flow distortion affects the observed frequencies equally, which is an assumption supported by the results presented in Huq et al. (2017), the imperfect ratios correspond directly to underestimation in the velocity variances. Since our results do not indicate how each velocity component is affected, it is still difficult to directly use the results presented here to correct the variances. However, some qualitative comparisons can be made. If, for example, the u- and v velocity components are measured with no error, the observed ratios of 1.12--1.19 can only turn into 4/3 if the w variance is increased by 18--26\%, which means that the w component itself should increase by 8--12\%. This error range is in agreement with the results by Frank et al. (2016), but higher than the error suggested by Huq et al. (2017). If we, on the other hand, assume equal errors on all velocity components (positive for u and v, and negative for w) the ideal ratio of 4/3 can be reached with a 4--6\% correction on the velocity components. These examples illustrate that our method can be a useful tool for judging whether flow distortion corrections of a particular sonic anemometer are adequate or not, but that it cannot be used directly to quantify the error." ---%%%

I believe that by addressing these major comments and the following specific com-ments listed below, that this paper will be appropriate for Atmospheric Measurement Techniques.

Specific comments:

Page 3, line 30: A better definition for "isotropic" should be given before ", which also means". The assumption of isotropic is critical for the theory that leads to the 4/3 ratio from which the entire paper is based. So, a clear definition is important.

%%%--- As suggested by the reviewer we have added a description of what isotropy means and reformulated the sentences regarding local isotropy ---%%%

Page 4, line 1: The statement "...the velocity power spectra follows the relation," is not self-evident to the casual reader. I would recommend clarifying that Kolmogorov determined this.

%%%--- We now refer the reader to Pope's 2000 textbook after the equation ---%%%

Page 4, line 5-6: Clarify "outer scale" Does "the most energy containing scales" refer to something similar to the peak of the spectra as shown in figure 1? Is there a way to describe the "Kolmogorov length scale", i.e., when energy dissipation begins?

%%%--- Scales are now added to Fig. 1 to clarify the scales in the spectra. We have also added text describing how \eta can be estimated and that this is much smaller than the sonic path length ---%%%

Figure 1. Could add a -2/3 slope reference line for comparison.

%%%--- Added as suggested by the reviewer --%%%

Page 5, line 7: Does the component i refer to u, v, or w?

%%%--- See our response to the next comment ---%%%

Page 5, lines 6-15: I found this "crude" description confusing. k2 and k3 should be defined. The Phi function should be defined. The sentence on line 15 is a repeat of an earlier statement. My big question is whether or not this section is necessary? I'm not sure it really matters much to the main understanding of the paper why the path averaging correction affects u different than v and w. At least, it might not be important enough to derive the theory behind it.

%%%--- We agree with the reviewer. The explanation of the different effects of path averaging on the three velocity components is not essential for the paper and so it is now removed ---%%%

Page 6 line 7 versus Page 7 line 9: Be careful where U is defined as instantaneous versus U defined as an average over 10-minutes.

%%%--- U was instantaneous and so to conform also to the definitions in Sect. 2.2.2, we changed "U" in Sect. 2.2.3 to Sh (and call it instantaneous horizontal wind), and replace V by S ---%%%

Table 1: In generally, is there a reason why H06 was only applied to 2 of the 3 datasets? This should be clarified. Also, it is not clear until Table 2 exactly which permutations of the different calculations were analyzed. It wasn't clear from Table 1 and throughout this section which different versions of these data sets were actually tested.

%%%--- We now include the possible permutations in Table 1 and in the caption of the table we have added "Due to the height of the instrument at Nørrekær Enge, we did not apply a PA correction as the error should be negligible" ---%%%

Page 8, lines 18 and 23: The terms "quality signal equal to zero" and "no warnings" are confusing. I am assuming these refer to the manufacturer's diagnostic value that comes from the CSAT3.

%%%--- We are now consistently referring to the manufacturer's quality signal ---%%%

Page 8, lines 26-30: This is a strange introduction to the results. It is somewhat telling that the results are described as “we show examples”. My intention by encouraging the authors to perform more rigorous statistical analysis (via 1st order polynomial p-values or LOESS regression, etc.) is to make the results less about “examples” and more about rigorous objective metrics. The word “closely” at the end on line 28 implies some sort of goodness of fit test.

%%%--- We have now extended the paragraphs that introduce the results. The sharpened criteria are also firstly described here. See our response to the reviewer’s general comment regarding this issue ---%%%

Page 8, last line on page/Page 9 line1: The first part should probably belong in the methods. For the second part, is this something that was observed from this study, or a more general finding that should have a citation?

%%%--- We have moved some of the lines mentioned by the reviewer to the beginning of Sect. 5 as suggested. As we now state in the same lines, normalization with U is in our study found to reduce the scatter in the velocity spectra ---%%%

Figure 3: Which lines does the “c” plot (i.e., 0th order polynomial) fit, w or v? It probably isn’t u since that is much lower on the graph. On the caption, when it says “perpendicular”, does this mean wind can flow in either direction, e.g., left-right as well as right-left? I assume the range +-10 deg means the average wind direction, not the range of instantaneous wind direction within the 10-min period?

%%%--- As we have now reformulated the beginning of Sect. 5, it should be clear that the fit is performed on the w spectra. We also added the information in the caption of the figure. Further, we have stated exactly at which relative directions we refer to when saying “parallel” and “perpendicular” in the caption. We now also include in the first paragraph of data treatments that we computed the mean wind direction for each 10-min period and a statement regarding what direction is meant hereafter ---%%%

Page 9, lines 10-12: This might be a vast overreach of the data to assume that because “both intervals” in figure 3 appear to fit within a specific inertial subrange, that it applies “irrespective of the wind conditions”.

%%%--- The paragraph has been rephrased and now we include that this assumption is in fact tested using the sharpened criteria ---%%%

Page 9, second to last sentence: the use of 4/3 is somewhat misleading here. It really has nothing to do with the 4/3 in Equation 2. It is purely coincidence that the uncorrected Metek had a w-u ratio of ∼1, such that the improvement from uncorrected to corrected increases the value by ∼4/3. To emphasize that this value is not the same as the 4/3 in equation 2, I would simply state it was a 33% increase.

%%%--- We have changed all instances where 4/3 is mentioned in relation to the 3D corrected to uncorrected variances and used the suggestion by the reviewer ---%%%

Figure 4. Why is the same graph of w-u red on the left and black on the right? If the running mean was replaced with a LOESS fit, then the confidence interval lines could also be added. In the caption, should clarify if this is the average “wind direction” over the 10-min period.

%%%--- It was not the same graph but we also showed in both frames the 3D corrected w- to u-velocity spectra ratios for a better understanding of the results. However, we now show in right frame the v- to u-velocity spectra ratio for the non- and 3D-corrected data ---%%%

Table 2. The sharpening criteria should be mentioned earlier in the methods. Also, with a 1st-order polynomial/p-value criteria to include only 10-minute periods with no significant deviation from the -5/3 line, then the sharpening criteria would not be necessary.

%%%--- See our response to the general comment regarding the sharpened criteria ---%%%

Page 11, line 18: Does "lower absolute directions" mean "directions more in line with the boom"?

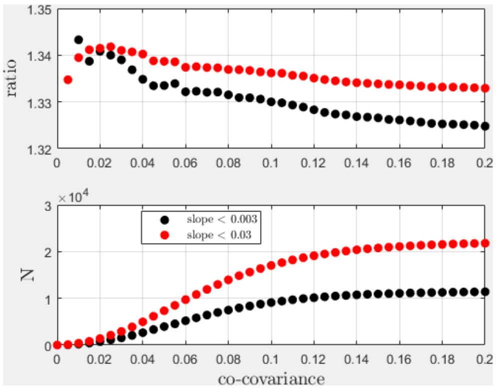
%%%--- We meant low relative directions. We have modified the wording according to the suggestion by the reviewer ---%%%

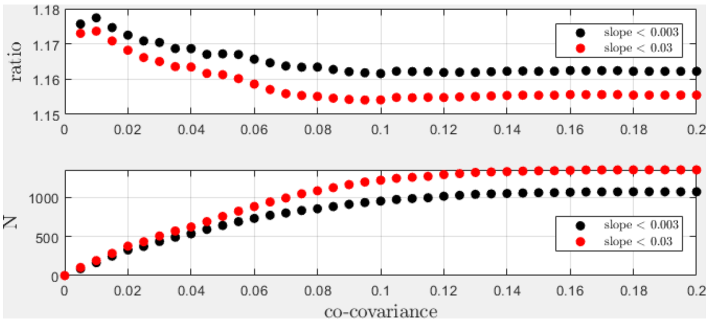
Page 11, line 22: The threshold +/- 0.003 seems arbitrary without some justification. The Fuw/sqrt(FuFw) <0.02 criteria should be explained in the methods with the definition of isotropy.

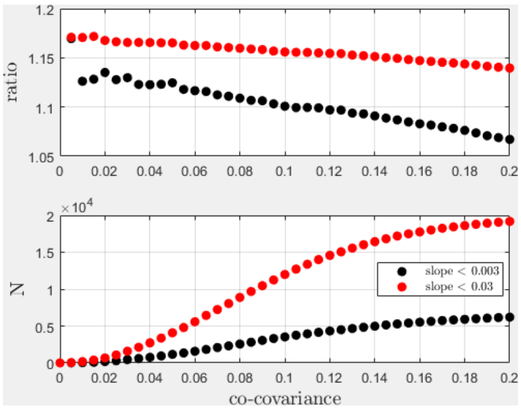
%%%--- The Fuw/sqrt(FuFw) criterion, i.e., the uw co-covariance is now moved to the beginning of the results section. As we responded to an earlier comment, the text describing isotropy has been extended and reformulated.

Regarding the choice of thresholds: we have now added the following text to Sect. 6.1 about the uncertainties:
"The choice of thresholds for the sharpened criteria compromised the amount of data left for the analysis; about 4\%, 25%, and 1\% of the original amount of 10-min periods for the USA-1 at Risø, CSAT3 at Risø and the CSAT3 at Nørrekær Enge, respectively. The choice, however, did not change the velocity spectra ratios significantly. The softening of the values to, e.g., 0.03 and 0.2 for the w-spectral slope and the uw-co-covariance, respectively, resulted in a change of the w- to u-velocity spectra ratio of $\approx$0.6\% for the USA-1, $\approx$1.5% for the CSAT3 at Risø, and $\approx$0.8\% for the CSAT3 at Nørrekær Enge, only"

For the reviewer's interest we have made some graphs showing the sensitivity of the w to u spectra ratios to both thresholds and the results are shown in the next three figures (for the USA-1 at Risø, the CSAT3 at Risø and the CSAT3 at Nørrekær Enge, respectively). The top frame in each plot shows the ratio as a function of the uw co-covariance (x-axis) and two thresholds for the w slope in the inertial subrange. The bottom frame is similar but showing the amount of measurements as function of the thresholds. As illustrated, the ratios at all three sites vary very little when changing these thresholds but the choice was made so that there was still enough data to be analyzed in the case where most observations were filtered out (i.e., Nørrekær Enge).







---%%%

Page 12, line 8: This statement also applies to the Metek, although it is much smaller.

%%%--- We have now a statement regarding this in the USA-1 analysis ---%%%

Page 12, line 19-20: These do not look that much different to me.

%%%--- They might not look that much different but for the CSAT3, the w- to u-spectra at +-180 deg is about 0.3 whereas it is less than 0.2 at Nørrekær Enge ---%%%

Figure 7: There are three different calculation scenarios presented on the right (no correction, H15, H15+H06). Which one of these applies to the left?

%%%--- As noticed by the reviewer, we now include this information in the caption of the figure ---%%%

Page 14, line 19: The presentation of ~ 0.5-1.5% is somewhat confusing. It might be simpler to describe this as "increase by 0.005-0.015", though by looking at the table this would be "0.008-0.018".

%%%--- We agree with the reviewer and have rewritten the sentence so that it reads ", which increased the CSAT3 ratios at Risø by 0.6—1.6% only ---%%%

Page 14, line 22: Does this really mean that the sonic was physically rotated? This probably refers to rotating the u, v, and w measurements. Also, this methodology seems overly confusing, when it would be much simpler to reprocess the data with the planar-fit rotation.

%%%--- We understand that a reader could think the anemometer was physically rotated, which was not.

We have therefore changed the sentence to “This was done by rotating the sonic anemometer measurements of the velocity components and applying an isotropic inertial subrange 3D spectral velocity tensor, as in H06, to calculate the nominal component spectra for this configuration.” Our reluctance to use the planar-fit correction stem from earlier results published in Dellwik et al. (2010) ----%%%

Page 15, line 14-15: This assumes that the uncorrected portion of the w measurement is simply a scaling issue.

%%%--- We have reformulated the whole paragraph as we explained in the response to the major comment by the reviewer with regards to our interpretation of the work of Huq et al. (2017) ---%%%

Page 15, line 20-21: I would remove this statement. It is far too oversimplified, and probably extremely unlikely.

%%%--- We find it more unlikely that the error is only on w, and would therefore like to keep the statement in a reformulated version (see our previous response) ---%%%

Page 15, line 27-28: This is a very bold statement, but it may be justified.

%%%--- We have added “, provided that an inertial subrange is clearly apparent” to the sentence ---%%%

Page 15, line 5 (near the bottom): Should clarify “from the corrected USA-1”.

%%%--- Corrected as suggested ---%%%

Page 16, line 26: I am confused about the verb tense. By saying “we propose to perform such an analysis” it reads like a recommendation for future research. That is fine, but if so, a recommendation like this should probably be near the end of the conclusions.

%%%--- Corrected as suggested ---%%%

Page 16, line 30: Similar to an earlier comment, the use of ~4/3 here is misleading because it does not have anything to do with the 4/3 in Equation 2. I would use “33% higher than the”.

%%%--- Corrected as suggested ---%%%

References:

Dellwik, E., Mann, J., and Larsen, K. S.: Flow tilt angles near forest edges – Part 1: Sonic anemometry, Biogeosciences, 7, 1745-1757, https://doi.org/10.5194/bg-7-1745-2010, 2010.