

Author Response to Review Comment #1

Dear Reviewer,

Thank you for reviewing the manuscript. Your comments were very helpful and improved the quality of the manuscript. The author responses can be found below each reviewer comment.

RC 1.1 Section 1: Another relevant publication, which compares the theoretical coherence bandwidth using the Kaimal model for different lidar scan patterns, including 2-beam and 4-beam lidars, is: Simley, Eric and Fürst, Holger and Haizmann, Florian and Schlipf, David, Optimizing Lidars for Wind Turbine Control Applications - Results from the IEA Wind Task 32 Workshop, Remote Sensing. 2018

AC This reference has been added and a description of the main finding has been added to the introduction section.

RC 1.2 Eqs. 16, 18, 19: Some derivation details (like Eqs. 6-8) or references to sources where these equations are derived would be appreciated.

AC References to the a paper where details of the derivation of the formulas can be found have been added below the formulas. The reference is also found at the beginning of the section.

RC 1.3 Eqs. 18, 19: Please clarify whether you are modeling the sequential scanning, or assuming all beams are measured simultaneously.

AC Here simultaneous measurements are assumed. This has been clarified in the text.

RC 1.4 Pg. 7, ln. 12: For the yaw misalignment correction, can you explain if you are trying to estimate the total horizontal wind speed, or the component perpendicular to the rotor? Additionally, comment on differences between measurements with the corrected velocities and the spectral model. For example, with yaw misalignment the measured wind will travel toward the rotor at an angle and reach the rotor at a different position than the model assumes. This could cause some differences between the measurements and model that the correction doesn't account for.

AC Here we trying to estimate the component perpendicular to the rotor. A clarification has been added below eq. 21. Also differences to the model have been pointed out. But in case of small yaw alignment the expected differences are assumed to be small.

RC 1.5 Table 2: Do you notice differences in length scales and other parameters if you bin by stability in addition to by sector, and would this be worth considering in the analysis?

AC We have considered binning by stability as the Mann turbulence model is a representation of turbulence in the neutral atmosphere. However, we saw no deviations of the measured point spectra to the fitted Mann turbulence model for different stability classes, see appendix C. Thus, we did not divide the data into stability classes.

RC 1.6 Figs. 9 and 10: A very important finding of this study is that even without including wind evolution, the measured coherence is very close to the modeled coherence, suggesting that wind evolution is not one of the main sources of error when estimating the rotor effective wind speed with a lidar. I think this is a key result and should be highlighted more.

AC We agree to this statement and have added a paragraph in the result section.

RC 1.7 Figs. 9 and 10: It would be easier to interpret the coherence for the 2-beam vs. 4 beam and region 1 vs. region 2 if the coherence curves for different cases were plotted in the same plot. At least a plot comparing the measured coherence curves for the four cases would make it easier to compare.

AC We have added an additional figure for the measured coherences, where the measured coherence for the 2- and 4-beam lidar systems is shown for both region 1 and 2. A paragraph summarizing the results has also been added.

RC 1.8 Pg. 15, ln. 4: "Comparing these numbers to the results of region 1 shows that flow having larger length scale parameters is beneficial for lidar systems. . ." In addition to the length scales being larger for region 2, the viscous dissipation of turbulent kinetic energy is lower. Could this also lead to improved coherence?

AC According to the model, the viscous dissipation of turbulent kinetic energy (ϵ) does not have an influence on the coherence since the spectra depend linearly on ϵ and thus cancel each other out. This is also observed from the measurements, where increased ϵ in region 2 do not lead to biases in the agreement with the model.

RC 1.9 Pg. 15, ln. 6: "There are however some slight deviations for both lidars in the region of 0.01 to 0.1 rad/m." What are some possible reasons for this mismatch?

AC Possible reasons for the mismatch can be measurement noise in the lidar or turbine data and modeling inaccuracies when calculating the REWS from turbine data. These points have been added to the section.

RC 1.10 Pg. 15, ln. 7: "When comparing the experimental data to the Kaimal model, a larger mismatch is observed compared to region 1." It appears that the coherence for the Kaimal model does not change much between Figs. 9 and 10, and that the Mann model changes similar to the field measurements. Can you comment on this?

AC The coherence changes are bigger for the Mann turbulence model because changes in the three-dimensional structure of turbulence are better represented by the Mann turbulence model. The Kaimal model is less flexible at representing changes in the three-dimensional structure of the turbulence and thus smaller changes are seen between the two regions for this model.

RC 1.11 Eq. 22: Would you expect the induction zone to slow down the advection speed? Does this appear in the field-measured time delay?

AC The expected time delay has been calculated based on the estimated lidar REWS, which has been corrected for the induction effect based on turbine measurements and the position of the focus points. So in a simple way we have included the slow-down due to induction. We have not found evidence of any biases introduced by the turbine's induction effect.

RC 1.12 Pg. 15, ln. 20: "the information theoretical delay estimator" This sounds like a great way to estimate the time delay. Could you briefly clarify how the two input signals are split into past and future? Are you comparing the past part of one signal to the future part of the other?

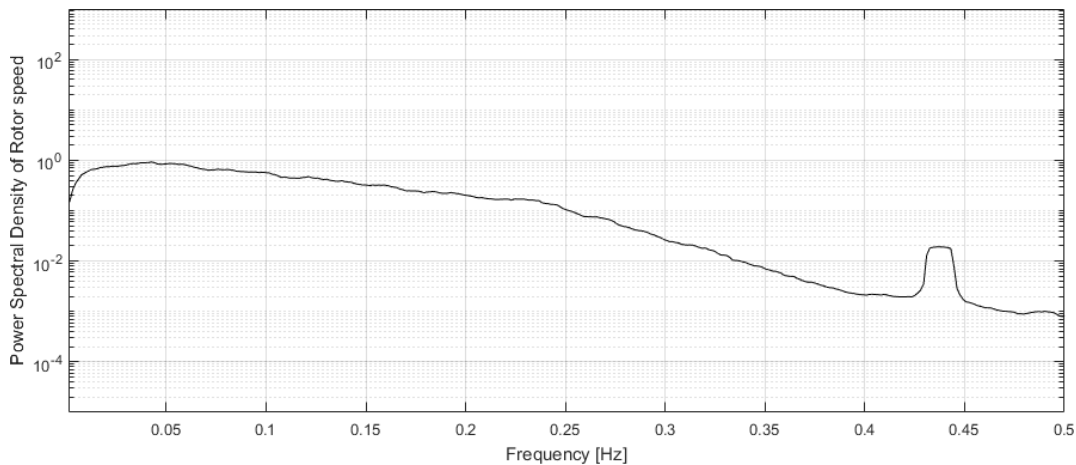
AC The signal is split in the middle. Then both past values of the signal are compared to the both future values. This has been clarified in the manuscript. More details can be found in the reference.

RC 1.13 Fig. 11: Because there is so much scatter in the field data, to understand the trend of the field time delays, the average time delay binned by wind speed would be valuable to show in the plots.

AC We have added binned mean values and ± 1 standard deviations.

RC 1.14 Pg. 16, ln. 14: Choosing 0.433 Hz for the delay frequency does not seem like the best choice, especially because it is much higher than the cutoff frequency. It would be better to base the delay frequency on the frequency where the wind disturbance impacts the signal of interest (like rotor speed) the most. This could be found with a linear model of the closed-loop turbine. Doing this would provide a more realistic value for the preview time needed (but I imagine would still be within the available preview time you have observed). See for example (Schlipf, 2015), where the delay frequency is chosen as 0.1 Hz, which is the frequency where peak of the rotor speed spectrum is located.

AC Yes, we agree to the statement. We have plotted the average spectra of the rotor speed for above-rated wind speeds below. The maximum of this spectrum is at 0.0425 Hz. We have used this as the delay frequency and redone the time delay analysis. The 2-beam lidar is lacking preview time at high wind speeds, which implies that a larger distance between measurement and rotor plane should be chosen.



RC 1.15 Pg. 2, ln. 12: "exponential decay model" -> "exponential decay coherence model"

AC This has been changed in the manuscript.

RC 1.16 Pg. 2, ln. 19: ". . .if both quantities want to be measured" would sound better as (for example) ". . .if measurements of both quantities are wanted"

AC This has been changed in the manuscript.

RC 1.17 Pg. 3, ln. 15: "where a reduction in the blade and tower DELs. . ." -> "where the blade and tower DELs. . ."

AC This has been changed in the manuscript.

RC 1.18 Pg. 5, ln. 26: "non-monotony" -> "non-monotonicity"?

AC This has been changed in the manuscript.

RC 1.19 Eq. 16: Should " $k \cdot x$ " be " $k \cdot n$ "?

AC This has been changed in the manuscript.

RC 1.20 Pg. 7, ln. 11: Consider using a different symbol for turbine misalignment since ϕ is already used for the weighting function.

AC " φ " has been replaced by " ϕ ".