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Interactive comment

Interactive comment on "Lidar Estimation of Rotor-Effective Wind Speed – An Experimental Comparison" *by* Dominique P. Held and Jakob Mann

Anonymous Referee #1

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General Comments:

This paper presents a spectral model for estimating the coherence between nacelle-based continuous wave lidar measurements and the rotor effective wind speed using the Mann turbulence model. The expected coherence from the model is then compared with the measured coherence for two types of nacelle lidars from Windar Photonics mounted on a Vestas V52 turbine. The measurement coherence is very relevant to the design of lidar-assisted control of wind turbines, making the paper a valuable contribution. In general, the modeled coherence matches the measured coherence very well. Previous papers have used the Kaimal model for such comparisons, but the authors show that the Mann model fits the field data better. The manuscript also presents one of the most thorough comparisons between modeled coherence and field-measured coherence in the literature. In addition, the present work includes the influence of different turbulence length scales on the measurement coherence, showing that the model and field measurements change in similar ways to the turbulence parameters. The paper is very well written and overall explains the methodology well. There are places where I believe more explanation of the methods should be included, however, and I believe some of the results could be presented more clearly. These and other suggestions for improvement are included below.

Specific Comments:

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

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

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

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.

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?

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.

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.

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?

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?

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?

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

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?

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.

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.

Technical Corrections:

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

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

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

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

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

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

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Discussion paper