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

Anonymous Referee #3

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The main purpose of this paper is to compare the Mann coherence model to the Kaimal model when considering lidar measurement of the rotor-effective wind speed. The work paper is generally well-written and the measurement campaign produced an impressive data set. However, I believe that a more quantitative approach should be used in comparing the two models.

My major suggestions are the following:

1. When comparing the Mann model to the Kaimal model, a more statistical approach would be preferred. For instance, on page 13 line 15, it is stated that "... the measured data agrees very well with the Mann turbulence model coherence. The Kaimal

model on the other hand seems to give a slight underestimation of coherence". Can you quantify this agreement/underestimation numerically? Perhaps as a function of frequency?

2. Could you provide concisely the benefits of the Mann model over the Kaimal model, and potential drawbacks of the Mann model if applicable? Computation time is mentioned on page 2, lines 13-14, but not elaborated on; and Mann model fitting is mentioned in Appendix C, but it is unclear to me how much more complex the Mann model (3D) is than the Kaimal model (1D) (page 15, line 9).

3. In the conclusion, it is stated that blade passage effects were not included. In the experimental data, how was the effect of blade passage removed?

I also consider the following points to be important:

a. A fuller description of lidar operation would be preferred, for readers who are not highly familiar with lidar operation. There are terms used that may not be very meaningful to readers, such as Rayleigh length, Doppler spectrum, probe volume, focus distance, and perhaps even wave number.

b. Is (3) an alternative definition of the spectral tensor? The definition from Mann (1994) does not appear in the manuscript.

c. I don't understand the i,j indices in (16). Further, the meaning of "... summation of repeated indices is implied" (page 6, line 14) is unclear to me. It appears also that in (18) and (19), the i,j indices are replaced with k,l. Could you be explicit about what each of these indices represent (especially since k has been assigned to the wavenumber previously)?

d. Labeling on plots needs some improvement. Font sizes are small and labels are unclear. For instance, Figure 6 has "WSP" on the x-axis, but this is not defined.

e. There is inconsistent notation, particularly with Greek lettering. β is used for both pitch angle and azimuth angle, e.g., page 5, line 20 and table 1. β and θ are both

used for blade pitch angle, e.g., page 1, line 20 and page 5, line 20. φ is used for both the lidar weighting function (eq. (15)) and the average turbine misalignment (page 7 line 11). alpha is used for both the half-cone opening angle (page 6, line 21) and the Kolmogorov constant (page 11 line 1).

f. It would be nice if some of the key parameters are labeled in the left panel of Fig. 4. For instance, the half-cone angle and the focus distance. The distance d_{Nac} could be labeled in the right panel of Fig. 4, especially that this is not really defined until page 15 (long after table 1 where it first appears). Delta x, which also appears in Table 1, was not completely clear to me until page 15 when it is finally defined.

g. Figures 9 and 10 could be labeled more clearly. Perhaps the “Data” line should be labeled “turbine estimates” (or something similar)? Then it may be clearer that each curve represents the coherence between the lidar data and each of the labeled items.

The following are smaller corrections and suggestions for improvement:

I. There is inconsistent notation for lidar focus distance (d_f vs L_f). See page 6, line 8 and page 7, line 7.

II. What is the difference between the induction factor and the axial induction factor on page 7? What is the relationship between them?

III. The term ‘filter’ appears to be used to refer to both a digital filter (in referring to a Butterworth Filter, page 16) and something far more general on page 12. This use (particularly the more general use, on page 12) is confusing in a scientific journal.

IV. Readers may find the use of ‘region 1, region 2’ (page 13 lines 11-12) confusing if coming from a wind energy background, where modes of turbine operation (below cut-in, below rated operation) are also referred to as ‘regions’.

V. There are some typos and grammatical errors throughout which should be corrected.

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