Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2018-72-RC2, 2019

WIND ENERGY SCIENCE DISCUSSIONS

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

Anonymous Referee #2

Received and published: 4 February 2019

The paper presents an experimental comparison of the rotor-effective wind speed (REWS) from lidar and turbine data. Overall, the paper is very well-written and includes very important findings: the correlation of lidar preview to the turbine reaction can be better modeled by the Mann turbulence model compared to Kaimal model used in previous work, even if wind evolution is not included. This is very relevant for lidar-assisted control application. The analysis is done based on the large data set (should be the largest published so far) and the paper includes interesting details, e.g. a fully analytic model of the correlation. Although the quality of the paper is already very high, the issues mentioned below might be helpful to further improve the paper.

Time delay:

The calculation of the time delay via the "information theoretical delay estimator" is an interesting approach. However, the information in Figure 11 is hard to read. Couldn't you use mean and standard deviation for each discrete time delay? Or wind speed bin? Further, the calculation of the filter preview can be improved. First, the frequency at 1P is quite high since the frequencies you are still able to measure with a good coherence are much lower. Also, the coherence level of 0.5 might be not a good cut-off-frequency, depending on the lidar spectrum. E.g. rotor and lidar estimate of the REWS can have a coherence of 1 and still have different spectra (if the transfer function is linear). In this case, using the coherence level of 0.5 would lead to no filtering, but filtering would be necessary depending on the transfer function. The transfer function at -3dB (for first order linear filter, approx. 0.7) should be better suited, especially for lidars with little averaging effect as the ones presented in this work. The 0.5 coherence level should be only close to the -3dB, if S_LL is quite similar to S_RR. Thus, would be good to better motivate the coherence level of 0.5 or use the transfer function at -3dB.

Details to improve understanding:

- Eq. 3 and 4: What is \delta, k'?

- Eq. 9: Some intermediate steps how to get there might be helpful. Maybe in the appendix? How do they relate to the equations from Mirzaei and Mann (2016)?

- Eq. 20 and 21: It is also not really clear, how the correction is applied to the real data, since only 1 Hz data are collected. Is this algorithm done on the lidar system or in post processing? It is also not clear, where this correction comes from. Line-of-sight wind speeds are often used in a wind field reconstruction algorithm which directly provides derived signals such as the REWS. And maybe I am wrong, but the correction seems to be the same than reconstructing the average horizontal wind speed. For small misalignment angles it might be not very important. For larger angles however, it is more the average longitudinal wind having an "effect" on the rotor. Thus it is not clear, why this correction is necessary. But maybe I missed something. Thus, some explication might be helpful.

Organization:

The paper is mostly well organized. Only Section 4 might be separated into subsections and Section 2 might be better organized. The part before the current 2.1 could be included in a subsection "Overview Coherence Model". Currently, 2.1 is including the model of the rotor spectrum, 2.2. how you get the REWS estimate from turbine data. Then 2.3 combines model of the lidar spectrum, cross spectrum and how you get the REWS estimate from lidar data. Thus, the subsections seem to be not on the same level. For the understanding, it might be better to first describe the model and its component (2.2 S_RR, 2.3 S_LL, 2.4 S_RL) and then the model implementation and validation against simulation (2.5) and then the modification for field testing (turbine measurements).

Minor issues (please ignore them, if too picky)

Eq. 20: Shouldn't \beta be \beta_i?

p20, l1: Up to this point, it is not mentioned that both lidar systems provides 1 Hz data. Table one might lead to 2 and 4 Hz. So it might be not clear at this point why >90% is equal to 540 measurements).

Figure 1: \theta_{FF} and \theta_{FB}. In text on page 1: FF and FB are not in math-mode.

Captions of Fig. 9 and 10 don't end with a period, others do.

Figure 3,6,7: unit in labels (partly) missing.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2018-72, 2019.