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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Better turbulence spectra from VAD scanning wind lidar" *by* Felix Kelberlau and Jakob Mann

Anonymous Referee #1

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General Comments:

The manuscript describes two approaches to reduce the effects of averaging and cross-correlation between the different velocity components that typically compromise turbulence measurements from lidars and complicate the comparability of those measurements with in-situ point measurements, e.g. performed by sonic anemometers. The topic is highly relevant under the aspect of the potential and limitations to derive reliable atmospheric turbulence data from lidar remote sensing and falls clearly into the scope of AMT. The manuscript is clearly structured and and gives a thorough description of the theoretical background and the mathematical formulation of the models to simulate the different lidar scanning and analysis methods. It is, however, not always easy to read. The introduction would be strengthened by some discussions on the im-

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plications of the performed study on practical applications e.g. for future boundary layer research in general, and for wind energy applications, as e.g. related to the investigation of loads of wind turbines, in particular. The main weak point of the study is the very limited data set used for the comparison between measurements and simulations (only about 5 consecutive hours during one day), representing one single situation of wind conditions. In terms of wind energy, this wind speed is clearly above the rated wind speed of typical wind turbines, with the blades already considerably pitched and thus not so prone to turbulent loads. For corresponding investigations and analyzes it would be highly interesting how the method works around and below rated wind speed, e.g. for 12 and 8 m/s. Investigating more situations might also enlighten further on the large discrepancies (in the same magnitude as discussed improvements due to the new proposed processing methods) between target spectrum and sonic measurements for u and v. As long as this is not sufficiently understood, I would also doubt the significance of many of the presented results.

Specific comments:

1) Title: “Better” in the title immediately associates with “than what”; I would at least think about changing to “Improved”, if not extending the title a bit more to make clearer what the reader can expect.

2) P1, L20: the actual manufacturer/distributor should be mentioned here

3) P2, L14 (and other instances): “however” should usually be separated by comma on both sides

4) P2, L24-25: what is a “correlation twice as strong”, please quantify in more detail; in addition is the fact “that the correlation increases when a time shift related to the mean wind is taken into account” presented as surprising, but it is of course not and a very standard method in many measurement applications (e.g. to correct for wind speed and direction dependent time shifts between dislocated sensors, e.g. sonic anemometers and infrared gas analyzers for the determination of latent heat and CO2

fluxes)

5) P3, Figure 1: unconventional coordinate system, in particular for meteorology, where usually u defines the E-W direction and v the N-S direction

6) P4, L14-15: The quantitative results of those other experiments should be shortly presented with the corresponding references; I also miss a reference to one of the basic statements on the issue by Willis and Deardorff, 1976 (ïĄşU/U < 0.5)

7) P5, L17: maybe better "in contrast" instead of "by contrast"

8) Section 2.4.1: would be nice to quantify and present in a table some of the key parameters for the used instrument, e.g. IR , df and a0

9) P6, L18-19: ". . . we cannot distribute the lost small scale fluctuations on spectral frequencies. . ...", but are you able to quantify the overall amount/importance?

10) P6, L29: insert "," after "composed of"

11) P8, Figure 2: some of the labels are by far too small and not or only very difficult to read; please improve

12) P7, L12: "This leads to a redistribution of energy among the velocity components u, v, and w." If it is a pure redistribution this should mean that TKE spectra derived from the measurements should be unaffected, correct? Gives this a tool for a potential quality assurance of the lidar derived variances?

13) P12, Figure 4: please specify in the caption altitude level and average wind speed for the presented example

14) P12, L4: insert blank after reference Mann et al. (2010)

15) P13, L4-6: can you quantify/reflect upon the order of magnitude of the uncertainty that is introduced by this assumption

16) P14, Equation 23: should it read sin2c instead of sinc2 ?

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17) P17, L18-21 (measurement description): very superficial description here, I suggest to include a few key infos in addition to the Pena et al. reference; what was the measurement frequency of the sonic; is it ensured that the measurements are unaffected by flow distortion/mast effects during the investigated case

18) P18, L5: “The data rate is thus for all methods 1 Hz”; does this also include the sonic data? If so, how are the sonic 1 Hz data sampled? I assume the sonics run somewhat between 10 and 50 Hz, so you could create 1 Hz data either by averaging your raw data, or picking one raw value every s. The corresponding selection will of course have an influence on your final spectra.

19) P19, Figure 5 (also Figures 6 and 7): I suggest to reconsider your presentation of the results ; in particular the black and blue squares are very hard to distinguish; this makes it very hard to follow the discussion of the results; a quick fix could be to consider in addition to the different colors also different symbols (e.g. square, star, triangle)

20) P19, Figure 5: why is the target model so far away from the sonic measurements; are there any effects of flow distortion visible (connects to cmment 17)

21) P21, L16: “. . .. show very good agreement to the model”; I feel this is a very strong statement; I can support this only for kl>10-2, but not for the rest

22) P23, L14-16: the unfortunate mis-representation of the sonic measurements by the target spectrum is again a great concern; how confident can you be in the discussion of improvement caused by your new processing methods, when the discrepancies between the target model and the sonic measurements are of the same magnitude or even larger; Again I would strongly recommend to also look at and present results for different synoptic/wind situations to provide some evidence where this mis-match between target spectrum and measurements originates from.

23) P 24, Figure 7: its puzzling that the sonic measurements and the target model fit

very well in the case of w, while the match it is rather poor for u and v;

24) P26, L6-7; This is a crucial issue of the manuscript and has to be elaborated in more detail (see also my corresponding concern in the general comment section)

25) Formatting of references is inconsistent a. Journal names abbreviated/not abbreviated b. DOI given or not c. Incomplete, pages missing: e.g. Newman et al., 2016; Pena et al., 2015; Sathe and Mann, 2012;

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