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WIND ENERGY SCIENCE DISCUSSIONS

WESD

Interactive comment

Interactive comment on "Cross-contamination effect on turbulence spectra from Doppler beam swinging wind lidar" *by* Felix Kelberlau and Jakob Mann

Anonymous Referee #3

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Kelberlau and Mann present work towards an improved measurement of turbulence spectra from Doppler lidar DBS scans. They introduce a methodology to simulate the lidar measurements in a turbulence box which helps them to analyze the quality of the lidar measurements. With the method of squeezing that has been introduced in a previous study they achieve remarkable improvements by eliminating cross-contamination effects in the lidar measurements. They show that these improvements can only be achieved if the wind speed is aligned with the DBS scan and conclude that in all other conditions, the spectra cannot be corrected. I think this study provides very interesting analysis and important insights into DBS scanning. However, I found the manuscript hard to read in some parts, mostly because of unprecise language and variable def-

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inition. Despite this there are some other major concerns which I summarize in the general comments. I recommend the manuscript to be considered for publication in Wind Energy Science after major revisions.

0.1 General comments

- I think the introduction can be improved to better motivate the use of DBS scans for turbulence retrieval. There are many studies that use VAD-scans for this purpose. What is the advantage of using DBS? Please relate this to the work of Eberhard, Frehlich, Smalikho, Krishnamurthy, Bodini etc.
- Since this is a manuscript for Wind Energy Science, I think the authors should describe a little bit more how turbulence spectra can be used in practice for wind energy purposes. I think many wind energy experts are not very familiar with this topic. How exactly do they relate to IEC 61400-1
- Please be very clear with directions, angle offsets and definitions. It is quite hard to follow the different coordinate systems that are used throughout the manuscript. A nomenclature of variables in the appendix would also help to serve this purpose.
- Section 4 with the results stops at describing the differences between measurements and simulation in a qualitative way. I want to encourage the authors to consider adding a quantification of the error between lidar and sonic estimated turbulence parameters at least for the cases with aligned wind flow with the DBS scan. Also, many unknown behaviours are described without giving ideas about how to investigate this behaviour any further. This could be added to the conclusion.
- The conclusion and outlook section is very short with a rather pessimistic ending stating that in most cases the turbulence spectra "should not be trusted". I think

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these findings should be related to the goal of wind site assessment and load prediction that is mentioned in the beginning. What are the prospects? How can this work help in future? What are alternative measurements that could be done for this purpose and what are the advantages/disadvantages compared to the method presented in this study. One question that came to my mind is if a DBS strategy which adapts the beam direction to the wind direction could be used to overcome the problem of cross-contamination.

- I recommend some language copy-editing if the manuscript is accepted for publication.

0.2 Specific comments

- p.1, l.2f: The authors write that DBS lidars generate spectra. This is confusing, because it suggests that there is only one kind of velocity spectra and it is automatically produced by the lidar. I think the authors should be very clear from the beginning how these spectra are produced (i.e. from radial velocities, vertical stare or the retrieved wind vector).

- p.1, l.7: The method of squeezing should maybe be briefly introduced, because it is not a well-known term in the community.

- p.2, l.20ff: There exist some works that simulate lidar scans in LES fields (e.g. Stawiarski et al., 2015). What are differences / advantages of the method using the turbulence box. This could be described in more detail in Section 3.2.

- p.3, l.10: How is the time scale defined that divides the mean part from the turbulence part in the Reynolds decomposition?

- p.7, ll. 9ff: I cannot follow how Eq. 16 and 17 are concluded from Eq. 13, 8 and 9. Also, it is defined in Sect. 2.1 that u is the longitudinal wind component and v

the transversal, but now it seems that these are the meteorological conventions!?

- p.8, Eqs.18-22: I think these equations could be presented in a more concise way for better readibility. For example, $\overline{\Theta} - \theta_0$ could easily be replaced by a single variable name and σ_u^2 in Eq. 21 could be presented as a function of u_{DBS}. By the way, DBS as the variable subscript is a bit unfortunate. More than one letter in the subscript should not be italic.
- p.12, l.8: ZX300 was only briefly mentioned in the introduction. Maybe repeat here what is meant with the abbreviation.
- p.12, l.12: What are .rtd-files. The file ending is not really important for the reader, but what kind of information they contain!
- p.12, l.25: The parameters should be introduced with their meaning.
- p.14, l.22: "project all focus points onto a vector..." I think this is unclear. What are the focus points in a pulsed lidar?
- p.14, l.27: Is a nearest neighbour method really the best solution? Would interpolation not be better (even though it would definitely also not be perfect in a turbulent flow)?
- p.14, ll.31ff: I recommend putting this description in a mathematical formula.
- p.15, l.6f: That the area under the power spectral density must equal the variance of the time signal follows from the Parseval's theorem and should always be checked and valid if power spectra are calculated. However, with the scaling with the wave number as it is done in Fig. 4, this does not apply. Please check and add the relevant literature and formula, if you mention it.
- p.15,l.17: What is the "long axis"? Please be more specific. Also, define what is the "target spectra". Do these spectra contain volume averaging of the lidar?

- Fig.4 Fig.5: The line styles of u- and v-component are hard to distinguish. It would be good to show the $k^{-5/3}$-slope in the plots to get an idea of how well the spectra fit to the inertial subrange theory.

- p.28, l.14: I think the number of the IEC-standard should appear in the reference.

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