We thank Anonymous Referee #3 very much for their comprehensive review of our manuscript. The feedback was of great help and we believe that the text is of a considerably higher quality in its revised version. We copied the referee comments and add our response, marked in red, after each point raised.

Anonymous Referee #3

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 definition. 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.

We extended the introduction by adding a definition of VAD and DBS, referring to previous research based on either of the two scanning strategies, and described the advantages when using DBS.

• 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

In the revised version of our manuscript we describe more accurately how turbulence spectra can be used to derive turbulence parameters for determining aerodynamic loads on wind turbines in accordance with 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.

We agree with the referee and added a nomenclature in the appendix. We also removed several inconsistencies in the variable names.

• 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.

A quantification of the systematic error of a DBS scanning wind lidar for different wind conditions is given in Sathe and Mann (2011). Determining the error based on our spectra would not create new knowledge. We therefore focus on understanding the different effects that influence the shape of the turbulence spectra. We extended section 5 (Conclusion) accordingly.

• 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 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.
We extended the conclusion by adding the note "In no case should turbulence velocity spectra from DBS wind lidar be fitted to a turbulence model." to relate our findings to the goal of wind site assessment. An auto alignment of the beam orientation with wind direction is an interesting idea. We think such a technology would require knowledge about the wind direction at different height levels before the wind measurements are taken. Also, such a scanning strategy would not improve the ability to measure the *v*-component of the wind. We instead refer to the Windscanner multi-lidar technology and add an idea for a different modification of DBS scanning wind lidar, that includes deflecting the beams of one single lidar device, so that they intersect in a common area.
• 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).
Yes, we should be clear and changed the formulation to make clear from the beginning that we work with "*Spectra generated from reconstructed wind vectors of Doppler beam swinging (DBS) wind lidars*"
• p.1, l.7: The method of squeezing should maybe be briefly introduced, because it is not a well-known term in the community.
We add that the method of squeezing "*reduces the longitudinal separation distances between the measurement locations of the different lidar beams by introducing a time lag into the data processing*" because it is not a well-known term. A more detailed description of the method follows later in the manuscript.
• 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.
LES simulations are more computationally expensive, especially since very long time series are required for deriving smooth spectra and small scale turbulent structures are not well represented in LES data (5-8 times the grid length). We added:
"Sampling in a turbulence box is a method to simulate wind lidar measurements in very large computer-generated wind fields. The creation of such wind fields according to Mann (1998) requires less computational power than for example large eddy simulations (LES). LES was successfully used before to analyse coherent structures in wind fields (e.g. Stawiarsky et al., 2015) and wind profiles (e.g. Gasch et al., 2019) but predicting lidar derived turbulence velocity spectra requires much more turbulence data. An advantage of using LES is that Taylor's frozen turbulence hypothesis does not need to be applied but a drawback is that fine scale turbulence would be suppressed."
• p.3, l.10: How is the time scale defined that divides the mean part from the turbulence part in the Reynolds decomposition?
We added the information that the time scale for averaging is ten minutes.
• 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!?
In accordance with the comment of Anonymous Referee #1, we added a step in Eqs. 16 and 17 and described in a different way where these equations come from. *u* and *v* are always the longitudinal and transversal wind components. The introduction of α (see next point) might help seeing the relation between *u*,*v* and *x*,*y*. more easily.
• p.8, Eqs.18-22: I think these equations could be presented in a more concise way for better readability. For example, $\Theta-\theta_0$ could easily be replaced by a single variable name and σ_{2u} 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.

We introduced the relative inflow angle $\alpha = \overline{\Theta} - \theta_0$ and use it in all equations. We also rearranged many terms of Eqs. 13-22 for better readability. Eq. 21 does now include a representation of u_{DBS}. We changed all subscripts that are not a variable (DBS, SQZ, hor, long, lat, rep, real, res, scan) to appear in roman font.

• p.12, l.8: ZX300 was only briefly mentioned in the introduction. Maybe repeat here what is meant with the abbreviation.

We added that the ZX300 "*is a continuous-wave VAD scanning profiling lidar.*"

• 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!

Mentioning the file ending makes it easier for readers that are familiar with the Windcube to know which files we mean because the different output files have different endings. For all other readers, we added that these files "*are standard output data files that contain the line-of-sight velocities of every single beam including their timing and carrier-to-noise ratio.*"

• p.12, l.25: The parameters should be introduced with their meaning.

That is true. We introduced the three model parameters "*the turbulence length scale L, the degree of anisotropy Γ, and the dissipation factor* $\alpha\varepsilon^{\frac{2}{3}}$".

• 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?

What we falsely named focus points are the centre points of the range gates along the lidar beams. We changed the expression to "*measurement locations*" in order to use an easy to read expression. In accordance with a comment of Anonymous Referee #1 we added a Figure 2 and extended the description of how the line-of-sight measurements are to be processed.

• 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)?

Both methods are not perfect. The nearest neighbour method has the advantage that the actual velocity values and as a consequence the total velocity variances remain unchanged. Interpolation would flatten the velocity peaks and reduce the variances slightly. We added that "*we reach that all measurement data is used with no change in velocity variance which would occur if interpolation would be applied*" to the description of our motivation to use the nearest neighbour method.

• p.14, ll.31ff: I recommend putting this description in a mathematical formula.

We followed the recommendation of the referee and put the 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.

We believe that the statement is correct for our presentation of the spectra. We added Stull (1988) as a reference who writes "Semi-log presentation. By plotting f*S(f) vs. log f, the low frequency portions of the spectra are expanded along the abscissa. Also, the ordinate for the high frequency portions are enhanced because the spectral density is multiplied by frequency (see Fig 8.9d). Another excellent quality is that the area under any portion of the curve continues to be proportional to the variance."

Stull, R. B. (1988). *Some Mathematical & Conceptual Tools: Part 2. Time Series. An Introduction to Boundary Layer Meteorology, 295–345.* doi:10.1007/978-94-009-3027-8_8

• 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?

We improved the description of the "target spectra" and state that they "*originate from sampling single points along the u-direction of the turbulence box with a frequency of 4 Hz.*" *in the revised manuscript.*

• 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.

Adding the $k^{-5/3}$-slope would not give additional information because all target spectra are already guaranteed to follow the $k^{-5/3}$-slope in the inertial subrange. In our semi-logarithmic presentation, the slope is not a straight line but a curve and we are worried to overload the plots by adding it.

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

We agree and added the number of the IEC standard in the list of references.