

## ***Interactive comment on “Better turbulence spectra from VAD scanning wind lidar” by Felix Kelberlau and Jakob Mann***

**Anonymous Referee #2**

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Within this manuscript, the authors present a new method to calculate the turbulence spectra from measurements from a CW Doppler lidar using velocity-azimuth display (VAD) scans. The authors use both a modelling and measurement approach, comparing lidar and ‘truth’ sonic anemometer measurements, to demonstrate that their proposed method provides more accurate turbulence spectra than existing techniques from VAD scans.

The novel subject matter is timely and of great interest to the readership of AMT. However, much of these results relies on only 5 hours of data under a narrow set of conditions, which really has limited the significance of the present study. Additionally, the modeled results are not in good agreement with the observations, and these differences are hardly explained or investigated. This is concerning, given the significant

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role of the modelling results in this study. With these issues, I recommend the article be reconsidered for acceptance in AMT after major revisions.

Major comments:

a) Figs. 5, 6, 7 and throughout Sect. 6: In these plots (especially 5, 6), there is a large disagreement between the modeled spectra and the observed spectra, particularly at low wavenumbers. This is concerning given that much of the presented results rely on the accuracy of the model, and it appears that the model is not accurately representing real lidar measurements. The only reason given for this difference between the real and modeled spectra is that it is a result of the heterogeneous landscape (p. 20, line 21). Given the importance of the modeled spectra in this study, this justification is insufficient and it was hardly discussed in the cited reference. The authors must investigate these differences herein further to understand their root cause, otherwise the use of the modeled spectra herein is suspect.

b) This manuscript in particular would benefit greatly from a ‘Discussion’ section between the results and conclusions that would link the results here to possible wider adoption across a variety of locations/seasons/times. This is especially important for this study given the fact that its results are solely from 5 hours of lidar data under high-wind conditions during the daytime in winter. The authors should discuss how the results are expected to vary under different conditions (weaker winds, very stable/unstable, etc), for measurements at different altitudes, circle diameters, half opening angles, or anything else the authors think would be relevant to any user that would try to apply this method elsewhere.

Alternatively, instead of adding a discussion section the authors could expand their study to more time periods under different atmospheric conditions.

Specific comments:

a) P. 2 line 12: Somewhere around here it would be appropriate to reference Eber-

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hard et al (1989) as one of the first studies where Doppler lidars were used to profile turbulence using VADs.

b) Figure 2: Parts of this figure (especially the subfigure showing the lidar beams) are extremely small and difficult to read. This should be improved, making the figure larger would help. I also suggest adding to the caption describing how the top plot visualizes the lidar beam positions.

c) P. 8 lines 5-13 & Fig. 3: This should be moved to later in the paper, perhaps Sect. 4. At this point, the reader has no context to understand the details of what is being shown as the model has not been described.

d) Fig. 3 (and throughout): The units for power spectra in the atmospheric science community are generally  $m^{-1}$ , not  $rad\ m^{-1}$ . These units appear throughout the text, in the table, and figure.

e) Eq. 10 & 11: Be consistent with these equations. Eq. 11 gives the entire variance for  $v$  while Eq. 10 only gives the variance contamination for  $u$ , but the subscript notation on the left-hand side for both indicates total variance. Also, Eq. 11 does not appear to be derived correctly (and is inconsistent with Eq. 10). Why is the  $\frac{1}{2}$  factor in the equation? The logic for how these Eqs are derived is not obvious (should be clarified), but should there also be a term for the covariance ( $u'w'$  and  $v'w'$  overbars) on the right-hand side?

f) P. 9 lines 8-16, 20-21: This text should also be moved to Sect. 4 where the readers will have the appropriate context to understand the discussion here. The rest of the text after Eq. 11 and before Sect. 3 can be combined into one paragraph.

g) Fig. 4: Nice figure. It could be improved if you added a reference vector for the mean wind to show how the field is advected. It would also be beneficial in the lower plot to highlight in a different color which pairs of measurements are used in the two-beam method.

h) Sect 4: Please make sure to explain and define all variables and notation. In par-

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ticular, I could not find definitions for:  $e_1$ ,  $\Phi$ , and  $T$ . The notation of  $_z$  was also not described.

i) Sect 5.1: Add a sentence here cross-referencing table 1 to summarize the experiment. Please also expand this discussion. The time period is 5 hours, it is unlikely the wind speed was constant that entire time. How much did it vary? How much did the turbulence intensity vary? What was the stability of the boundary-layer? Given the location, I expect near-neutral stability, but it would be good to verify and quantify the stability. How were the resonance values in Table 1 determined? What was the precision of the lidar measurements?

j) P. 19 line 2: Please add a more throughout description of how these spectra are made. Is this an average of the individual 10-min spectra? Were outliers in the spectra removed in making this plot? How much did the actual spectra vary of the time period? The description is insufficient.

k) P. 19 line 8: What is the 'target spectrum'? Is this simply the modeled spectrum assuming certain characteristics of the flow garnered from the sonic anemometer measurements?

l) Fig. 5: These two plots can be combined as the axes are identical and much of the data overlaps. By combining the plots, the VAD/SMC and two-beam methods can also be compared. Also state in the caption what the vertical dashed lines indicate.

m) P. 20 line 25: By this, do you mean that the frozen turbulence hypothesis is not completely valid as the wind field evolves in time as it advects through the measurement volume? If so, please clarify that here.

n) P. 20 line 34: Could this deviation also be caused by random errors in the lidar measurements resulting in a noise floor above the modeled value (related to last point in i) above)? Based on the model description in Sect. 4, measurements are modeled as precise (without any random error).

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o) Fig. 6: These two subplots can be combined (if kept, see comment p), as b) only contains one additional piece of information (red line) that could be easily overlaid on a) for comparison.

p) Sect. 6.2.2: This section and Fig. 6 b can be removed. If this method is not even applicable to real CW lidar measurements due to the ambiguity around a 0 Doppler velocity, why even present it as a method?

q) P. 25 line 20: Recommend changing the term from 'very good' to 'reasonable'. There are still non-trivial differences between the modeled and observed spectra in this reviewer's opinion.

#### Editorial corrections

a) P. 1 Line 19: Add hyphen to Velocity-azimuth display.

b) P. 6 line 30: Reword to: 'Turbulence with a length scale below ...'

c) P. 7 line 1: Remove 'to sense them'

d) p.7 line 10: Remove 'or cross talk'

e) P. 13 line 4: 'is' should be 'are'

f) Eq. 23: Should be 'sin' instead of 'sinc'.

g) P. 18 line 2: Remove 'used'

#### References:

Eberhard, W.L., Cupp, R.E. and Healy, K.R., 1989. Doppler lidar measurement of profiles of turbulence and momentum flux. *Journal of Atmospheric and Oceanic Technology*, 6(5), pp.809-819.

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