

We thank the Referee #2 a lot for a very useful and constructive review of our manuscript and appreciate the time and effort involved in it. We have provided a point-by-point response below.

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 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.

The Referee points out that only five hours of data under a narrow set of conditions were used in the experimental results of this study. We agree that more data would, indeed, create a more solid foundation for our findings. The reasons for using such a limited data set are detailed in the answer to Referee #1.

Regarding the fact that there is a large disagreement between the modelled and the observed spectra, we added a second reference (Larsén et al., 2016). They show that spectra based on measurement data of complete years at Høvsøre also show large disagreement in the low wave number band when compared to model spectra. The desired investigation to understand the root cause of such differences lies clearly out of the scope of our study. In this context, the referee states that the model does not accurately represent real lidar measurements. But the strong disagreement between model and observed spectra is already found when the spectral tensor is compared solely to the sonic measurements. No actual filtering is part of this comparison, so one should not conclude from the disagreement that there is an insufficiency of the lidar models. The modelling of the different lidar data processing methods is mostly coherent with the experimental results, and the differences are analyzed in detail in the discussion of the results. We have no reason to assume that the conclusions

we draw would differ if a data set that is closer to the Mann spectral tensor would have been used. In order to make the reader aware of this problem, we emphasize in the text “that, due to the poor fit of the measured spectra of the horizontal wind components and the modeled spectra at low wave numbers, we can compare the relations between the different methods but not absolute values.”

We understand and share the wish for a more comprehensive study that covers several locations/seasons/times. And we regret that we cannot provide such in this paper. Our aim is to introduce a complete numerical description of a VAD scanning CW lidar for the first time and to extend it to two promising methods of modified data processing. Further, we want to give a detailed description of the cross-contamination effects that are briefly mentioned in previous publications but never fully described. The one set of experimental data we use supports this analysis and helps us to point out the limitations of the new methods and the models.

We agreed with the reviewer’s suggestion to add a section on what we would expect from applying the methods to different conditions and measurement setups. This new section 6.4 discusses implications for varying measurement heights, cone angle, mean wind speeds and atmospheric stability conditions. This new section links our results to a possible wider adoption of cases.

Specific comments:

a) P. 2 line 12: *Somewhere around here it would be appropriate to reference Eberhard et al (1989) as one of the first studies where Doppler lidars were used to profile turbulence using VADs.*

We added a reference to Eberhard et al. (1989) to the introduction: “The estimation of second order statistics of the turbulence in the wind by means of VAD scanning pulsed Doppler lidar was first demonstrated in Eberhard et al. (1989).”

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.*

We replaced the label “Lidar” by a laser symbol, clarified the caption and increased the overall size of the figure.

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.*

Thank you also for this suggestion. We agree that the origin and details of Fig. 3 are difficult to understand here and that an explanation would be beneficial to the reader at a later point. Unfortunately, we see dividing Sect. 2 into two pieces as the worse option. Also moving it as a whole after Sect. 4 would weaken the logical structure of the manuscript: namely, describe the problem in Sect. 2, sketch our approach to tackle it in Sect. 3 and model this approach in Sect. 4. Instead, we added information to contextualize the plots sufficiently so that they can be understood already here.

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.*

We changed the units as suggested to m^{-1} .

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 $1/2$ 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?*

Eq. 10 also gives the entire variance for u . Since we look at the situation at the resonance wave numbers like in the example in Fig. 2 the contribution of u_{wind} to u_{lidar} is zero. To better clarify

this for the reader, we have added “ $_{res}$ ” to the subscript notation. A subscript “ $_{unc}$ ” was added to Eq. 11 to emphasize that the equation is valid only when the inflow is uncorrelated between the two spatially separated points. This occurs at high wave numbers. We corrected the “tan” in eq. 11 to “cot”. Thank you for this correction. We included a derivation of both equations so that the reader gets the logic behind them. The influence of the covariance terms averages out to zero.

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.

Please find our answer in the response to c). We combined the last two paragraphs.

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.

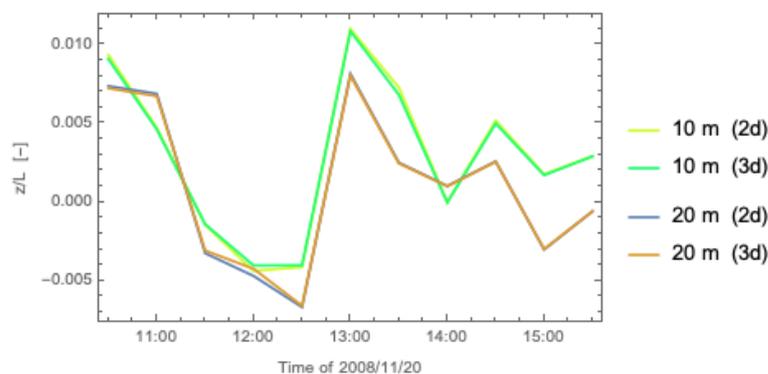
We are happy to get this suggestion and added the \mathbf{U} vector to the figure. We also included a visualization of the beams used in 2-beam processing and mentioned this in the caption and referred to it in the text.

h) Sect 4: Please make sure to explain and define all variables and notation. In particular, I could not find definitions for: e_1 , Φ , and T . The notation of $_z$ was also not described.

We introduced the unit vector \mathbf{e} and the meaning of the half cone opening angle Φ is now repeated in the text. We removed the index “z” that was not giving additional clarity in its context.

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?

The min/max/std values of U and TI have been added and a reference to Table 1 is now given. We added the formulas for the resonance values given in Table 1 to section 2.5.1. The precision of the lidar is usually better than 1%. This information is added to the text. The stability of the boundary layer during the experiment was neutral, as shown below. z/L is either derived from 10 m or 20 m with two different methods to calculate the momentum flux (2d and 3d) which give very similar results.



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.

Yes, the shown spectra are the averages of all individual spectra based on 10-min intervals. The resulting average spectra are then averaged within 30 wave number bins. Outlier detection in time domain was investigated but eventually not applied because its effect on this data set was negligible. This is already mentioned in subsection 5.2.

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?

Very good point. The target spectrum is the unfiltered one-dimensional model spectrum based on the spectral tensor we chose to best-fit to the sonic anemometer measurements. We added the equation to derive the u spectrum from the spectral tensor to the end of the model description in section 4 and refer to it here for clarification.

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.

We considered combining the subplots which, indeed, would give a better comparison between circle processing and 2-beam processing. Unfortunately, the plot appears overloaded then. We extended the caption by adding “... The grey vertical dashed lines represent the first and second resonance wave number.”. The same applies to Fig. 7.

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.

Thank you for mentioning this point. The passage was unclear and we improved it as follows: “A possible explanation is that the fluctuations of the u- and especially the w-component in the real wind field are not perfectly correlated, i.e. the frozen turbulence hypothesis which the model assumes is slightly violated. The result is a small contribution of w_{wind} on u_{lidar} that appears to a higher extent in the VAD processed spectrum. The reason for the difference is that the correlation is closer to unity in the case of SMC processing.”

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 modelled as precise (without any random error).

To consider a random error in the lidar measurements as a cause for increased energy values in the measurements is reasonable. But for two reasons we do not believe it applies here. First, the effect should then be smaller in the whole circle processing methods than in the two beam approaches because random errors average out along the measurement circle. We don’t see that in the measurements. Second, the effect of random erroneous line-of-sight measurements would lead to even stronger contamination of the w-spectra due to the higher sensitivity. This is also not represented in the results.

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.

We combined both subplots and swapped the color of the “2-beam model” line to cyan in all plots for better distinction.

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?

We added that “We included the model behavior of 2-beam processing for the sake of completeness and to show that the availability of reliable measurement data from the east and west beams would be of hardly any use.”

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.

We now describe the forecasting as 'reasonable'.

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'

We thank the reviewer for mentioning these errors which we have corrected. "sinc" is not a mistake but the abbreviation for the cardinal sine function that must be used. We now give its definition in the text. Mathematics is singular.

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.

Larsén, X. G., Larsen, S. E., and Petersen, E. L., 2016. Full-Scale Spectrum of Boundary-Layer Winds. *Bound. Lay. Meteorol.*, 159(2), pp. 349–371.