Spectral correction of turbulent energy damping on wind LiDAR measurements due to range-gate averaging

Reviewer's comments

1 General comment

The manuscript "Spectral correction of turbulent energy damping on wind LiDAR measurements due to range-gate averaging" by Puccioni and Iungo deals with the problem of spatial filtering by the probe volume of pulsed Doppler wind lidar instruments. This spatial filtering challenges the proper characterization of turbulence. Therefore, they propose an empirical transfer function, which under certain conditions, can be used to empirically correct the filtering effect. This transfer function is fitted to the ratio between the estimated power spectral density (PSD) of the along-beam velocity component and an empirical spectral model. This empirical model is based on the Blunt model (Olesen et al., 1984) and is fitted to the low-frequency range of the estimated velocity spectrum.

The solution proposed by Puccioni and Iungo is practical and simple, which is appreciated. The dataset is not novel but this is not so important here. In this regards, the paper is within the scope of AMT. The language is fluent but sometimes unprecise or unclear. While the conclusions of the paper support the proposed method and the overall content is clear, some major questions are raised by the manuscript, which should be addressed:

- I have the impression that a significant portion of the manuscript is filling. The size of the manuscript could be reduced by 40 %without affecting its core message. The value of a paper is not defined by its length, fortunately. The filling is sometimes counter-productive as shown in section 6, where the authors use synthetic turbulence generation to work exclusively in the frequency domain: there is no need to generate any turbulent field in the time-domain if the calculations are conducted on the velocity spectra only. This section deals with an interesting topic, which is the dependency of the spatial averaging on the mean wind speed and the variance of the velocity component. However, unnecessary steps are used to reach the conclusion, which erode the analysis.

- The data processing is not always clear. Also, data are sometimes over-processed. The main fitting algorithm is likely applicable only if the spectral peak is not affected by spatial filtering. If the spectral peak is filtered out, the peak frequency will become "corrupted". This limitation is not clearly highlighted in the manuscript.

- References to the existing scientific literature can be inaccurate or misleading. The number of self-citations in the manuscript is equal to almost one-third of the total number of references, which might be a little too high.

These three points are described more in details in section 2. Puccioni and Iungo deal with a challenging issue and their simple approach is, therefore, welcome. For these reasons, I recommend a major revision of the manuscript.

2 Specific comments

Point 1

Title: The term "range-gate averaging" may be criticized because the range gate does not necessarily refer to the probe volume length. I suggest using the term "spatial averaging" or "volume averaging" as a safe alternative.

Point 2

The first paragraph of the introduction reviews previous turbulence measurements in the atmosphere by Doppler wind lidar instruments. The majority of these references is inadequate:

- It is unclear how the work by Trukenmüller et al. (2004), Horányi et al. (2015) or Schepers et al. (2012) are related to Doppler Wind lidar measurements. I suggest removing these references.

- The works by Calhoun et al. (2006), Vanderwende et al. (2015) and El-Asha et al. (2017) are interesting but they are not about turbulence measurements. Their focus was on the mean wind speed only. I suggest removing these references.

- The reference to Grubišić et al. (2008) may not be appropriate because the lidars were not used to investigate turbulence characteristics. If the authors believe that a similar study must be included, the work by Spuler and Mayor (2005) might be more relevant. Note that Spuler and Mayor only collected snapshots of coherence structures, which may not be considered as "turbulence characterization" but rather "flow visualization".

- The reference to Fernando et al. (2019) may be replaced by the reference to Bodini et al. (2017) since the method used by Fernando et al. to study the turbulence dissipation rate is taken from Bodini et al.

- The reference to George and Yang (2012) may be removed because it is a review paper on vortices detection by various instruments. They did not focus on turbulence characterization and did not show any results from Doppler wind lidar measurements.

- Only self-references are used to illustrate turbulence measurements by lidars in the field of wind energy. In addition, the same results are sometimes cited multiple times because they are included in different similar papers. I recommend choosing only one of these papers and to not use self-references only.

The authors can find hereafter some of additional studies by (scanning) Doppler wind lidar instruments focusing of turbulence characterization: Lothon et al. (2006, 2009); Newsom et al. (2008); Angelou et al. (2012); Sathe and Mann (2012a); Branlard et al. (2013); Cheynet et al. (2016); Wang et al. (2016); van Dooren et al. (2017); Kumer et al. (2017); Held and Mann (2018); Peña and Mann (2019); Wildmann et al. (2019); Mauder et al. (2020). I also invite the authors to search for additional studies available in the scientific literature.

Point 3

Section 1: Some lines mentioning that the paper focuses on scanning pulsed Doppler wind lidar and not continuous-wave lidars or wind profilers may be necessary for the sake of clarity.

Point 4

Line 27-28: The sentence "Turbulence statistics of the wind velocity field can be retrieved through fixed scans while providing a spectral characterization of the inertial sub-layer" is only partly true. If the probe volume is larger than 50 m, there exist situations where spatial filtering can affect the entire inertial subrange, preventing the detailed characterization of turbulence.

Point 5

Line 29: The reference to the detection of very large coherence structures is a little strange here because it does not imply the possibility to establish turbulence statistics from them. In particular, the experiment by Calaf et al was done without knowing precisely the wind direction as they had no access to wind vanes or anemometers. Besides, the scientific literature contains many more examples of turbulence characteristics retrieved from fixed line-of-sight scans.

Point 6

Line 32: The reference to Mann et al. (2009) is only partly true: The actually estimated the auto and cross-spectral densities for the three velocity components, which is more advanced than the turbulent momentum flux.

Point 7

Line 34: A probe volume below 20 m is not so common for commercially available pulsed Doppler wind lidar. Maybe some comments can be written here.

Point 8

Line 42-43: The influence of the misalignment between the wind direction and laser beam could also be mentioned as an additional effect on the spatial filtering by the probe volume (see e.g. Held and Mann (2018)).

Point 9

Line 49: In Cheynet et al. (2017), the probe volume length was 75 m and the range gate length was 100 m. The probe length of 100 m mentioned in their study was used as an example to illustrate the spatial filtering.

Point 10

In section 2, equation (1) is not necessary for the paper. Since the spatial filtering is a function of the wavenumber, using f (in Hertz) instead of $n = fz/U$ is not desirable. Therefore, the study can be simplified by considering only equation 2.

Point 11

The Kaimal model and Simiu-Scanlan models are particular cases of Equation 2. Note that in Kaimal et al. (1972), An = 105 and Bn = 33 but in Kaimal and Finnigan (1994), An = 102 and Bn = 33. Equation (2) with unspecified A_n and B_n values should be referred to as the blunt model (Olesen et al., 1984) instead of Kaimal model. The reference to ESDU is incorrect here. The ESDU standard is using a modified von Karman model, which has a form different from Equation 2. Therefore, I suggest removing the reference to ESDU.

Point 12

The algorithm in Figure 1 is interesting but also perfectible. It does not clearly show why the iterative procure is necessary and this should be explained in a pedagogical way. For example, it could be stated that there is no need to have an iterative procedure if $f_{Th,0}$ is equal or lower than 0.01 Hz.

There is an argument in favour of the iterative procedure that is not clearly stated in section 2: Choosing a value of $f_{Th,0}$ too low will result in a poor fit of the velocity spectrum because the number of data points will be reduced. In addition, these points are associated with larger uncertainties than at higher frequencies. At the same time, if $f_{Th,0}$ is larger than the cut-off frequency, the fitting will be significantly affected by the spatial filtering.

There is also a potential limit for the application of this algorithm that was not clearly shown in the manuscript: the spectral correction may fail if the spectral peak is affected by the spatial filtering. Therefore the proposed method might only be adequate for probe volume of 50 m or lower. That is an issue that deserves further discussion.

Point 13

Equation 10: The spatial filtering is a function of the wavenumber rather than the frequency.

Therefore, I think that fitting a modified version of Eq. 10, where the frequency is replaced by the wavenumber, may be more appropriate than the original version of Eq. 10.

Point 14

Section 2: is the fitting algorithm a least-square fit?

Point 15

Line 145: Was the lidar azimuth set manually as equal to the mean wind direction or was it an automated procedure?

Point 16

Line 148: Maybe it can be explained why the sampling frequency was varying between 0.5 Hz and 3.3 Hz?

Point 17

Figure 2: The topography is a little difficult to see. Maybe you can use a digital terrain model?

Point 18

Line 163: Since the Obukhov length is calculated, I suggest replacing “static atmospheric stability” by “dynamic atmospheric stability” or simply “atmospheric stability”.

Point 19

Line 178: I do not understand the link between the sentence and the reference to Hutchins et al. (2012). Maybe this reference is not necessary?

Point 20

Lines 182-191: These lines could be summarized into a single sentence: “The second-order stationarity is assessed using a moving standard deviation with a window length of 5 min and zero overlapping”. The reference to Liu et al is not adequate since they did not invent the concept of moving standard deviation. Besides, I would recommend using overlapping windows for a more robust assessment of the flow stationarity. In Matlab, the function “movstd” can be used for this purpose.

Point 21

The test of the second-order stationarity is a nice addition by the authors. I would also recommend a test for the first-order stationarity using a moving mean function.

Point 22

Line 190: Is there any reason for choosing 40 % for the maximal IST value?

Point 23

Line 192: I am not sure I understand the “gradient-based” procedure to remove outliers. Maybe one sentence can be written to make it clearer?

Point 24

Line 194: There is no official Matlab function “inpaint”. However, there exists the function “inpaint_nans” by D’Errico (2004), which is well respected. Is this the function that you were using? If yes, a reference to D’Errico (2004) can be used.

Point 25

Eq. 17: This equation is only valid for the mean wind speed. If the variance is computed, substantial errors may arise (see eq. 2 in Sathe and Mann (2012b), which is also valid for a LOS scan mode). Some comments are expected here.

Point 26

Line 228-229: The Savitzky-Golay smoothing filter was not designed by Balasubramaniam (2005) but by Savitzky and Golay (1964). I do not recommend using this filter to smooth the power spectral density (PSD) as it also distorts the low-frequency range of the spectrum. The goal should be to smooth only the high-frequency range, which includes enough data point. I advise to simply bin-average your data over bins that are uniformly spaced in the logarithmic space. This way, the fitting algorithm will also be improved. Alternatively, you can estimate the PSD using auto-regressive methods, which can produce fairly smooth PSD estimates if the random process of interest is broad-banded, which is the case here.

Point 27

The method to estimate the velocity spectrum should be explicitly stated. Since a fitting to the velocity spectrum is done, it is important to know if the PSD estimate is reliable or not. I do not recommend using the periodogram method. Alternatives approaches are the modified periodogram method (Welch, 1967) or the multitaper method (Thomson, 1982), both available in Matlab.

Point 28

Figure 5: The high-frequency range of the downsampled velocity spectrum seems to be slightly too high. This might be due to the presence of aliasing if the time series were downsampled without filtering. Decimating the original time series with a FIR filter and an order equal to at least 10 is recommended (cf. the Matlab function 'decimate'). This should lead to a better agreement between the velocity spectra of the lidar data and the sonic data.

Point 29

Line 263-264: The small difference between the difference range gate length is unlikely to explain the difference between the different filter functions. One possible reason for the discrepancies is the presence of measurement noise, which increases with the frequency and which is accounted for in the empirical filter function used in the manuscript but not modelled in eqs. 6-7.

Point 30

Figure 7 and the associated discussion do not seem to be a vital piece of information here. Firstly, the use of subsamples with an averaging time of 25 s could be criticized as it only includes a small portion of the turbulence spectrum. Therefore, the influence of the correction method on the variance estimate becomes exaggerated, which is not desirable in relation to algorithm validation. Secondly, only one time series is included, which limits the conclusions that can be drawn from this figure. It may be a careful choice to remove this figure and the corresponding paragraph.

Point 31

Line 284-285: When applying a high-pass filter, the reader needs to know the exact value of the cut-off frequency, the order of the filter and what type of filter is applied. Therefore, mentioning a cut-off frequency of the order of 10^{-3} Hz is not sufficient. Velocity fluctuations around 1×10^{-3} Hz may still be representative of micro-scale turbulence. The use of the high-pass filter will increase the influence of the correction algorithm on the estimation of turbulence characteristics. This can be mentioned and/or quantified.

Point 32

Figure 8 includes two subfigures that can be merged into a single one by using a 3-variable scatter plot, where the colour of the markers reflects the variance. Nevertheless, I am not sure that figure 8 is vital to the paper.

Point 33

Reference to Guala et al. (2006) is inadequate. They studied turbulence in pipe flows whereas the paper discusses turbulence in the atmospheric boundary layer. A more appropriate reference would be Counihan (1970).

Point 34

Figure 9 can be reduced to a single panel. I think showing the pre-multiplied spectrum fS_u as a function of the wavenumber k is good enough.

Point 35

Line 320: "selected dataset" is not specific enough. Maybe you can mention which data from which campaigns?

Point 36

Figure 11 looks nice but it is not necessary to the paper. Firstly because we cannot see a clear difference between the left and right panel and secondly because it does not bring particularly useful information. I suggest removing this figure for the sake of brevity.

Point 37

Figure 12 is not necessary either to the paper. The textual description was already enough. I think this figure can be removed.

Point 38

Line 330: I am not sure what you mean by a quasi self-similar behaviour. Figure 13 is showing (normalized) transfer functions. I think it may be wise to keep the description as simple as possible.

Point 39

Figure 15 could be replaced by a simple table showing the median value and the interquartile range, for example.

Point 40

Section 6 could be reformulated into one or two paragraphs. Firstly, the use of synthetic wind field is not useful here, as calculations are conducted in the frequency domain only. Secondly, the computation of the profiles of the mean wind speed and variance as well as the associated discussion is unnecessary. The right panel of Fig 17 is interesting but does not shows that the error ε could be expressed as a function of the mean wind speed and variance of the velocity only.

I suggest shortening in section 6. It is possible to show the dependency of ε on the mean wind speed and the variance of the velocity as a contour map. Given a reference mean wind speed at a reference height, a logarithmic profile with and a given roughness length and the Kaimal spectrum, constructing such a map is straightforward. To change the mean wind speed parameter, you simply need to change the reference mean wind speed value. To change the variance of the along-wind component, you can simply change the roughness length.

Point 41

The conclusion should be reformulated following the previous comments

3 Technical corrections

Point 1

Lines 1-3 (abstract): Maybe you should mention that you are talking about “pulsed” lidar systems. Continuous-wave Doppler wind lidars can measure the flow within a volume much lower than 20 m and a sampling frequency of several hundreds of Hertz.

Point 2

Line 2 (abstract): a sampling frequency of the order of 10 Hz is mentioned. Do you mean 1 Hz, as written in the manuscript?

Point 3

Line 3 (abstract): the expression “back-scattered laser beam” may not be correct. Do you mean “backscattered light” or “backscattered signal”?

Point 4

Lines 9 (abstract): I suggest replacing “estimated directly from the LiDAR measurement” by a more accurate term: “estimated directly from the power spectral densities of the along-beam velocity component”.

Point 5

Line 34: The sentence “probe volumes, denoted as range gates” can be misunderstood by the reader and should be reformulated. The range gate length is different from the probe volume. For example, a range gate length shorter than the probe volume implies that the probe volumes are overlapping.

Point 6

Line 36: The syntax of the sentence “by means of a laser beam, which is back-scattered” is a little strange. I would write that the light is backscattered but not that the “laser beam” is backscattered.

Point 7

Line 38: “ from the Doppler shift **on** the back-scattered signal” should be “from the Doppler shift **of** the back-scattered signal”

Point 8

Line 38: “like those used for the present work” should be “like the one used in the present work”.

Point 9

The tilde symbol in Equation 10 and equation 11 may be explicitly defined for the sake of clarity.

Point 10

Line 156: “of the used LiDARs” may be written “of the LiDARs used” instead.

Point 11

Line 175-176: “For the SLTEST and Celina campaigns [...]through DBS or VAD scans” seems to be a repetition of the same information mentioned earlier in the manuscript. Maybe this sentence can be removed.

Point 12

Line 192: Consider replacing present tense by past tense when describing the data processing.

Point 13

Line 251: The “spectrum of the lidar signal” should be replaced with “spectrum of the LOS velocity”

Point 14

Line 276: You may replace “linear regression analysis” by “comparison”, which is much simpler

Point 15

Line 287: The term “first and second-order statistics” can be replaced by “mean value and variance”, which are the quantities you study in the paper.

Point 16

Line 307: The sentence “fitting of the LiDAR spectra” should be replaced with “fitting of the Blunt model to the along-beam velocity spectra”

Point 17

Line 324: “highest LiDAR gate” should be replaced by “highest LiDAR range gate” or “LiDAR range gate furthest from the instrument”.

Point 18

Line 336: “ [...] always underestimate [...]” Do you mean “overestimate” ?

Point 19

Figure 13: The term “convolution function” sounds strange. I would call it “transfer function” as it is the case in the field of signal processing

Point 20

Line 359: Convolution in the time domain is multiplication in the frequency domain. Therefore writing that the “synthetic velocity signal is convoluted in the frequency domain” is unclear. I think it may be simpler to write that the velocity spectrum is multiplied with the transfer function modelling the spatial averaging.

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