Reply to the comments provided by the Anonymous Referee #1 on the manuscript amt-2020-27 entitled "Spectral Correction of turbulent energy damping on wind LiDAR measurements due to spatial averaging", by M. Puccioni and G. V. Iungo

The authors are greatly thankful to the Reviewer for the thorough review and insightful comments. Our replies are reported in the following. References to pages and lines are based on the revised marked-up manuscript.

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 regard, 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…*

R: We thank the Reviewer for the positive feedback on our research strategy and the results achieved. It has been instrumental to leverage various LiDAR datasets, which have been collected by our group in collaboration with other colleagues, to prove the general applicability of the proposed model. Writing has been improved throughout the manuscript.

- *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.*

R: We are research enthusiasts and very meticulous in the execution of our projects. If sometimes, unfortunately, we end up writing lengthy manuscripts is definitely not connected with the need of filling a document, which is clearly not needed considering the length of the text, rather prove the research assumptions and corroborate our results. We have significantly shortened the manuscript and sharpened its focus. Sect. 6 has been significantly revised, and only the part related to the variability of the energy damping with wind speed, standard deviation, and sampling height is kept.

- *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.*

R: We definitely agree with the Reviewer's comment and the mentioned constraint, namely ensuring that the spectral peak in not corrupted during the post-processing, has been always verified for our data analysis. In the revised version of the manuscript, we have highlighted the importance of verifying that the energy content in the proximity of the spectral peak is not altered by the post-processing procedure. In the manuscript at line 147, it is now reported: "If during the iterative process, k_{Th} achieves a value equal or smaller than that corresponding to the spectral peak, k_p, then the procedure is arrested and a warning is dispatched indicating that the correction procedure was not successful".

- *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.*

R: As recommended by the Reviewer, we have significantly shortened our reference list.

Specific comments

Point 1

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

R: During the preparation of this manuscript, we were skeptical in using the term spatial averaging because different spatial-averaging processes may occur when operating scanning wind LiDARs, such as by varying continuously the scanning head in the azimuthal direction for VAD and PPI scans, or the elevation angle for RHI scans. Throughout the manuscript, the smoothing process under investigation is now referred to as spatial averaging, consistently with previous works, e.g. Frehlich et al. (1998) and Sjöholm et al. (2009).

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 Grubišic 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.*

R: We have revised the mentioned references according with the comments of the Reviewer.

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.

R: We definitely agree with this comment, indeed our discussion focuses completely on pulsed wind LiDARs. For instance, at line 45 it is reported: "A pulsed Doppler wind LiDAR, like those used for the present work…".

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.

R: That sentence has been revised as (line 30): "Provided the use of a probe length, l, sufficiently small to probe the inertial sublayer at a height from the ground z, e.g. $l < 2\pi z$ according to Banerjee et al. (2015), turbulence statistics of the wind velocity field can be retrieved through fixed scans, while providing a spectral characterization of the inertial sub-layer (Iungo et al., 2013)".

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.

R: We thank the Reviewer for this comment. The reference to the paper Calaf et al. (2013) and the related text have been removed.

Point 6

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

R: The Reviewer is right. At line 35, it is now reported: "In Mann *et al.* (2009), auto- and cross-spectral densities for the three velocity components were estimated through multiple scanning-LiDAR measurements".

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.

R: Only LiDAR units for research on ABL turbulence may provide capabilities to perform measurements with very short-range gates, e.g. 20 m. At line 38, it is now reported: "… wind LiDARs tailored for investigations on atmospheric-turbulence currently provide probe volumes smaller than 20 m…".

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)).

R: This is correct. Indeed, at line 130, it is reported: "… features of the low-pass filter and, thus, of the LiDAR measuring process, are functions of …relative angle between wind direction and azimuth angle of the laser beam …".

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.

R: At line 53, that statement is now revised as: "For single-point measurements performed with a Windcube 200S LiDAR and azimuthal angle of the laser beam set equal to the mean wind direction, a variance reduction of 8% was predicted for a gate length of 25 m, while it was increased up to 20% for a gate length of 100 m (Cheynet et al. , 2017)."

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 = f z/U is not desirable. Therefore, the study can be simplified by considering only equation 2.

R: We agree that providing both formulas might be redundant. The only equation with the reduced frequency, *n*, is now reported.

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 An and Bn 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.

R: We added in the manuscript that in Olsen *et al., 1984* the used spectral model is referred to as blunt model. However, even in that paper, it is reported that the spectral model was already used in Kaimal *et al.* 1972, for unstable conditions in Kaimal *et al.* 1976, Panofsky 1978, Højstrup 1981 and 1982, while for stable conditions in Kaimal 1973 and Caughey 1977. More recent papers refer to this spectral model as Kaimal model, see e.g. Risan *et al.* 2018, Worsnop *et al.* 2017 and even in the IEC standards for wind energy (International Electrotechnical Commission (2007) IEC 61400-1: Wind turbines—part 1: design requirements. 3rd edn). In the text at line 91, it is now reported: “The spectral model of Eq. 1 is typically referred to as blunt model (Olesen *et al*., 1984) or Kaimal model (Kaimal *et al*., 1972; IEC, 2007; Worsnop *et al*., 2017; Risan *et al*., 2018), and the parameter A is typically assumed equal to 105 (Kaimal *et al*., 1972), later revised to 102 (Kaimal & Finnigan, 1994), and B equal to 33.”

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_{th0} is equal or lower than 0.01 Hz. There is an argument in favor of the iterative procedure that is not clearly stated in section 2: Choosing a value of f_{th0} 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_{th0} 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.

R: We have revised the flowchart of Fig. 1 according to the Reviewer’s comments and added more details for the iterative process used for the estimation of k_{Th}. At line 137, it is now reported: “First, the pre-multiplied spectrum of the radial velocity projected in the horizontal mean wind direction is fitted with the spectral model of Eq. 1 only for wavenumbers smaller than $k_{Th,0} = 2\pi/l$. Indeed, we expect to observe significant spatial-averaging effects for turbulent length scales smaller than the probe length, l. For wavenumbers higher than the selected cut-off value, the ratio between the fitted Kaimal spectrum and the PSD of the LiDAR velocity, φ_*^2, is calculated to quantify the effect of the energy damping due to the LiDAR measuring process. Subsequently, the LiDAR-to-Kaimal ratio, φ_*^2, is fitted with Eq. 9 through a least-square algorithm to estimate the filter order, α, and provide an updated value for the cutoff wavenumber, k_{Th}. This process is iterated until convergence on the parameter k_{Th} is achieved (for this work, the convergence condition imposed is a variation of k_{Th}smaller than 1% of the previous value). If during the iterative process, k_{Th}achieves a value equal or smaller than that corresponding to the spectral peak, k_p, then the procedure is arrested and a warning is dispatched indicating that the correction procedure was not successful. This warning condition never occurred for all the data analyzed in this work. Furthermore, it should be considered that when k_{Th} achieves values close to k_p, the part of the velocity spectrum, Su, used for the fitting procedure with Eq. 1 can be so limited to jeopardize the accuracy of the fitting procedure.

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.

R: As recommended by the Reviewer, the filter of Eq. 9, it is now expressed as a function of the wavenumber, k.

Point 14

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

R: That is correct. At line 142, it is now reported: "Subsequently, the LiDAR-to-Kaimal ratio, φ_*^2, is fitted with Eq. 9 through a least-square algorithm to estimate the filter order, α…"

Point 15

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

R: For the SLTEST and Celina datasets, the azimuth was set automatically by using the feedback scan modality provided in the software of the Streamline XR LiDAR manufactured by Halo Photonics. At line 174, it is now reported: "… the azimuth angle for the fixed scans was updated automatically at the end of each DBS or VAD scan through the feedback scan mode embedded in the LiDAR software and using the wind-direction value measured at height of 53 m".

Point 16

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

R: We thank the Reviewer for this observation. The statement has been revised as follows (line 177): "To investigate possible variations of the averaging process related to the accumulation time, the sampling frequency of the fixed scans was varied between 0.5 Hz and 3.3 Hz, while the range gate was always set equal to 18 m".

Point 17

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

R: We do not aim to provide any specific information about the terrain topography, rather aerial views of the site.

Point 18

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

R: For the classification of static and dynamic stability we refer to Sect. 5.5 "Stability Concepts" of the book "An Introduction to Boundary Layer Meteorology" by Ronald B. Stull. Static stability is typically connected to convection and it is governed by the Richardson number, while an example of dynamic instability is the generation of Kelvin-Helmholtz waves, which is a shear-driven instability. For the sake of simplicity, we refer now in the manuscript to 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?

R: In Hutchins et al. (2012), the authors used horizontal and vertical arrays of sonic anemometers. To investigate transverse gradients, only data with the 10-minute averaged wind direction within the range ±20° from the direction perpendicular to the horizontal array were considered. The same criterion is now used to reject data with large deviations of the wind direction from the azimuthal angle of the LiDAR.

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.

R: The non-stationary index (IST) is a well-established parameter to investigate the statistical stationarity of time-series, see e.g. Foken *et al.,* 2004. It is not a moving standard deviation, rather a quantification of the percentage variability of the variance over sub-periods of the signal with respect to the variance of the entire signal. It is mathematically different from the moving standard deviation, indeed in Eq. 13, CV is not the signal, rather the variance. In Liu *et al.,* 2017, the IST has been successfully used to select stationary velocity signals collected through sonic anemometers at a site very similar to those involved in this work; hence, it is very relevant for our work.

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.

R: This is an interesting suggestion; however, the standard deviation and turbulence intensity already provide information about the variability of the signal in time over the mean.

Point 22

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

R: In Foken et al. (2004) and Liu et al. (2017) a maximum IST of 30% was used. For our work, based on sensitivity analysis, we decided to increase the maximum IST to 40% to enable larger data availability without modifying noticeably the results of our analysis.

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?

R: More details have been added for the gradient-based filter (line 223): "Specifically, the partial derivative in time of the radial velocity is calculated through a second-order central finite-difference scheme. Velocity samples with absolute partial derivative larger than 15 times the respective median value calculated over the entire signal are marked as outliers and replaced through the inpaint_nans function available in Matlab (D'Errico, 2004). The used threshold value is selected based on a sensitivity analysis".

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.

R: The proper name of the function and respective reference are now reported in the manuscript.

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.

R: We are thankful to the Reviewer for bringing up this important comment. The along-beam (radial or LOS) velocity variance $\sigma_{V_r}^2$ can be related to the Reynolds stress components as (Eberhard et al., 1989):

$$\sigma_{V_r}^2 = \sigma_u^2 \cos^2\Phi \cos^2(\theta - \theta_w) + \sigma_v^2 \cos^2\Phi \sin^2(\theta - \theta_w) + \sigma_w^2 \sin^2\Phi$$
$$+\sigma_{uv} \sin[2(\theta - \theta_w)] \sin^2\Phi + \sigma_{uw} \cos(\theta - \theta_w) \sin 2\Phi + \sigma_{vw} \sin 2\Phi \sin(\theta - \theta_w),$$

where $\sigma_u^2, \sigma_v^2, \sigma_w^2$ are the variance of the streamwise, spanwise and vertical velocity components, respectively, and σ_{uv}, σ_{uw} and σ_{vw} are the shear Reynolds stresses. Considering the azimuth angle set in the mean wind direction ($\theta - \theta_w \approx 0$ ensured by constraining the dataset to wind direction variability to $\pm 20°$) and very small elevation angle, the previous equation can be approximated as:

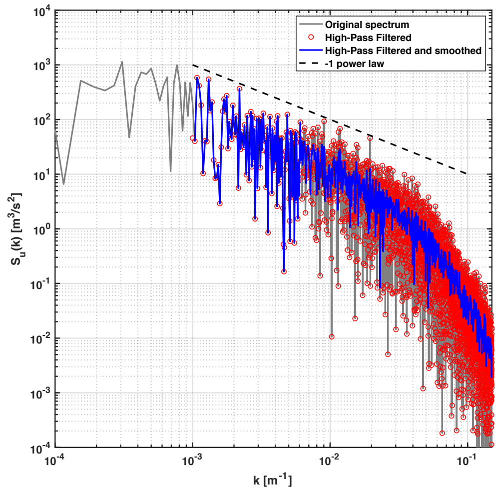
$$\sigma_{V_r}^2 \approx \sigma_u^2 + \sigma_v^2(\theta - \theta_w)^2 + \sigma_w^2\Phi^2 + 2\sigma_{uv}(\theta - \theta_w)\Phi^2 + 2\sigma_{uw}(\theta - \theta_w)\Phi + 2\sigma_{vw}\Phi(\theta - \theta_w).$$

Even assuming all the Reynolds stresses with the same magnitude, it is evident that at the first order of approximation $\sigma_{V_r}^2 \approx \sigma_u^2$. Of course, this approximation is not valid for generic values of θ and Φ. At line 239, it is now added: "Furthermore, the variance of the radial velocity is the first-order approximation of the streamwise-velocity variance given the above-mentioned setup constraints (Eberhard et al., 1989, *Sathe and Mann, 2013*)."

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.

R: For our data analysis the smoothing with the Savitzky-Golay filter worked as good as bin-averaging, see the figure reported below where the original spectrum is initially high-pass filtered, then smoothed. This procedure does not affect the energy content of the spectral peak. For the smoothing, we used a polynomial function of the second order and the windows are calculated as $int[10(160k)^{0.5}]$, with k reported in 1/m, and *int* is rounding to the closest integer number (Balasubramaniam, 2005). At line 267, it is now reported: "For modeling purpose, the velocity spectra are then smoothed in the wavenumber domain following the Savitztky-Golay filter (Savitzky and Golay , 1964), by using a second-order polynomial function and windows with the width equal to *int*[10 (160 *k*)$^{0.5}$],where *k* is in m^{-1}and *int* is rounding to the closest integer number (Balasubramaniam , 2005)".



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.

R: For the datasets collected at the Celina and SLTEST sites, the periodogram method has been substituted with the Welch spectrogram. However, very similar results are obtained with both methods. At line 329, it is now reported: "… the PSD of each velocity signal is then calculated with the pwelch function implemented in Matlab (Welch, 1967) without window overlapping and window width corresponding to k_{co}".

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.

R: We are thankful to the Reviewer for this comment and we agree that the down-sampled velocity spectrum might be affected by aliasing. We are now using the function "decimate" to downsample the data collected with the sonic anemometer. At line 285, it is reported: "The horizontal velocity retrieved from the sonic anemometer is first down-sampled with the sampling frequency of the LiDAR measurements, namely 2 Hz, using the Matlab function "decimate" with a finite-impulse response (FIR) low-pass filter with order equal to 10 (Weinstein, 1979)". Figure 5 has been revised accordingly.

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.

R: The residual noise present in the LiDAR acquisition surely leads to an overestimation of the high-frequency spectrum. However, we think that it is unlikely that such a big overestimation performed by Eqs. 5 and 6 could be related to the sole measurement noise. At line 306, it is now reported: "A possible explanation for the poor performance of these deconvolution models could be the different probe length used for the XPIA campaign (l= 50 m) in contrast to l= 30 m used in the original study of that deconvolution model (Mann *et al.*, 2009), and the presence of measurement noise in the data, which is not accounted for in the models of Eqs. 5 and 6".

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.

R: This figure and related paragraph have been removed.

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.*

R: More details on the used high-pass filter are now reported in the manuscript. The filter cut-off frequency is selected to avoid modifications of the spectral content of the peak. An example of the application of the high-pass filter is reported in the figure of point 26. At line 249, it is now reported: "The LiDAR equivalent velocity, U_{eq}, is then high-pass filtered to remove low-frequency non-turbulent velocity fluctuations, using the following spectral transfer function:

$$G(k; S, k_{co}) = \frac{1 + \tanh\left[\beta \log\left(\frac{k}{k_{co}}\right)\right]}{2}$$

where k_{co} is the cutoff wavenumber, which should be smaller than k_p to avoid effects on the spectral peak. The parameter β is equal to 100 to generate a sufficiently sharp filter across the cutoff wavenumber, k_{co} (Hu *et al.*, 2019)".

Point 32

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

R: We initially produced a similar figure to that mentioned by the Reviewer. However, it resulted to be more confusing without saving too much space in the manuscript. Therefore, we would keep this figure to provide a characterization of the background boundary-layer flows.

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).
R: That discussion has been removed.

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.
R: The inertial sub-range and the spectral correction is generally more evident through the power spectral density rather than through the pre-multiplied spectra. Plotting the spectra as a function of the reduced frequency, n, and wavenumber, k, is relevant for the implementation of the procedure. Indeed, more consistent values of the cut-off wavenumber are observed throughout the ASL height, which is not the case for n.

Point 35
Line 320: "selected dataset" is not specific enough. Maybe you can mention which data from which campaigns?
R: The sentence has been substituted with (line 369): "The proposed correction of the LiDAR measurements is now applied to all the datasets collected at Celina and SLTEST sites (see Table 2)".

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.
R: This figure has been removed.

Point 37
Figure 12 is not necessary either to the paper. The textual description was already enough. I think this figure can be removed.
R: This figure has been removed.

Point 38
Line 330: I am not sure what you mean by a quasi-self-similar behavior. Figure 13 is showing (normalized) transfer functions. I think it may be wise to keep the description as simple as possible.
R: We mean that the empirical corrections practically collapse on the same curve, which corroborates that the filter proposed in Eq. 9 is actually a good model for the spatial averaging. At line 381, it is now reported: "… all the estimated transfer functions practically collapse on the same curve for measurements collected at different heights".

Point 39
Figure 15 could be replaced by a simple table showing the median value and the interquartile range, for example.
R: The results of Figure 15 are now reported in Table 3 and the discussion at lines 404-414 has been revised accordingly.

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.

R: We are greatly thankful to the Reviewer for this highly constructive comment. Sect. 6 has been significantly shortened and the variability of the spatial averaging with friction velocity, aerodynamic roughness length, and sampling height is now reported.

Point 41

The conclusion should be reformulated following the previous comments.

R: Conclusions have been revised accordingly to the Reviewer's comments.

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.

R: In the abstract, it is now reported (line 1): "… pulsed wind LiDAR technology…".

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?

R: In the manuscript, we mention sampling frequency higher than 1 Hz and LiDARs achieving sampling frequency around 10 Hz are now available.

Point 3

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

R: It is now revised to back-scattered LiDAR 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".

R: This sentence has been revised accordingly.

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.

R: That's correct, the range gate is equal to the probe volume only for non-overlapping probe volumes, which is the case for all the datasets under investigation. The term range gate has been substituted with probe volume or length throughout the manuscript.

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.

R: At line 42, it is now reported: "… utilizing a laser beam, whose light is back-scattered…"

Point 7

Line 38: "from the Doppler shift on the back-scattered signal" should be "from the Doppler shift of the back-scattered signal"

R: Revised.

Point 8

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

R: We used more than one LiDAR and this sentence is grammatically correct.

Point 9

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

R: At line 129, the following statement has been added: "The symbol $\tilde{\cdot}$ is used to differentiate the analytical model of the low-pass filter from its empirical estimate through the ratio between the fitted Kaimal spectrum and the PSD of the LiDAR velocity, φ_*^2".

Point 10

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

R: Corrected.

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.

R: This sentence has been removed.

Point 12

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

R: We typically use the present tense for post-processing and past tense for tasks related to the execution of the experiments and results/tasks from previous works.

Point 13
Line 251: The "spectrum of the lidar signal" should be replaced with "spectrum of the LOS velocity"
R: Corrected.

Point 14
Line 276: You may replace "linear regression analysis" by "comparison", which is much simpler.
R: We performed a linear regression, which has a clear mathematical definition to estimate, slope, bias, r-square value, etc.

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.
R: Corrected.

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".
R: Throughout the manuscript, we use "… fitting with the spectral model of Eq. 1".

Point 17
Line 324: "highest LiDAR gate" should be replaced by "highest LiDAR range gate" or "LiDAR range gate furthest from the instrument".
R: Revised.

Point 18
Line 336: " […] always underestimate […]" Do you mean "overestimate" ?
R: The analytical models underestimate, it means they should correct more to generate corrected spectra closer to those estimated through the spectral model.

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.
R: Corrected.

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.
R: This part has been removed.

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