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

Reviewer's comments

## 1 General comment

The revised manuscript "Spectral correction of turbulent energy damping on wind LiDAR measurements due to range-gate averaging" by Puccioni and Iungo has been significantly improved. The fact they take the time to answer my numerous comments is praiseworthy and highlight their dedication to improving the manuscript. Nevertheless, there remain some few crucial aspects that need to be addressed before the manuscript can be accepted for publication in Atmospheric Measurement Techniques. I list them in the section hereafter.

## 2 Specific comments

### Point 1

The authors mentioned that the revised manuscript is significantly shortened but I see that the second version is longer than the first one. I invite the authors to assess which elements are superficial to make the manuscript more compact, if possible.

### Point 2

In the reply to my comment on point 20, the authors state that their stationary test is not a "moving standard deviation" but a "quantification of the percentage variability of the variance over sub-periods of the signal with respect to the variance of the entire signal". I would like to apologize if my original comment was unclear because the second-order stationary test I mentioned in my original point is what the authors described in their reply. More precisely, their test is the application of an archaic moving standard deviation function normalized by the standard deviation of the signal. I recommend the author not to use jargon term (IST) and refer to an older source than Liu et al (2017), for example, Foken and Wichura (1996). Therefore, my original suggestion "The second-order stationarity is assessed based on a moving standard deviation function with a window length of 5 min and zero overlappings" is still adequate. The authors can also complement this description by stating that they assessed the relative error $\varepsilon$ of the moving standard deviation with respect to the standard deviation of the entire signal. This relative error is what they call IST, except it is a longer and more ambiguous term. Note that the test by Foken and Wichura (1996) is also outdated because there is no overlapping and relies on numerical methods developed in the 1980s during which the computational power was limited. I trust the authors regarding the reliability of their test. Nevertheless, it is important to note that contrary to what Foken and Wichura (1996) state, this test should not be used to assess the stationarity of the friction velocity if the window length is only 5 min long with a threshold value $\varepsilon_T$ of 30% (or even 40%) for the relative difference. The reason is that the random error associated with the estimation of the friction velocity using an averaging time of only 5 min is likely above 50% (Kaimal and Finnigan, 1994). In this situation, the stationary the test will fail to distinguish random error from systematic bias. For the variance of the signal, the random error is much lower so the value $\varepsilon_T = 40\%$ chosen by the authors is likely appropriate.

## Point 3

The point 21 in my previous review aimed to highlight that assessing the second-order stationarity without looking at the first-order stationarity makes little sense for spectral analysis. Fortunately for the manuscript, I am not expecting significant changes in the results since the second-order stationarity is more difficult to achieve than the first one. Nevertheless, this step cannot be neglected and has to be addressed. Contrary to what the authors claim, the turbulence intensity and standard deviation cannot inform about the signal stationarity since they are only applicable to stationary random processes.

## Point 4

In your reply to point 27, you wrote that the periodogram method was replaced with Welch's algorithm and that similar results were obtained. In Matlab, using Welch's algorithm without overlapping and a single window is similar to applying the periodogram method but with a Hamming window instead of a rectangular window. It seems that in your data analysis, you have applied a single window (I might be wrong here). Therefore, this might explain the lack of substantial improvement in the power spectral density (PSD) estimates. Nevertheless, the periodogram method is known to be a poor power spectral density estimator. I recommend trying two to three segments with Welch's algorithm and 50% overlapping. This could significantly change the outcome of your fitting algorithm.

## Point 5

In your reply to point 29, the authors wrote that measurement noise in the lidar velocity records is unlikely to produce the big overestimation observed in Fig 5. I understand that my initial suggestion was maybe too vague. Therefore, I have reproduced such an overestimation in fig. 1 by filtering an idealized velocity spectrum using functions which emulates the presence or absence of noise at high frequencies. I have actually applied two low-pass filters with a different order for the sake of simplicity. In the left panel of fig. 1, one could assume that the red curve has a larger amplitude than the blue one because of measurement noise. The correction of the velocity spectrum is done by using the ratio $H(f_r)$ between the black curve and the blue curve. Therefore, the blue curve is properly corrected. However, the noise in the red curve is massively amplified when applying the same correction, because $H(f_r)$ erroneously assumed no measurement noise.

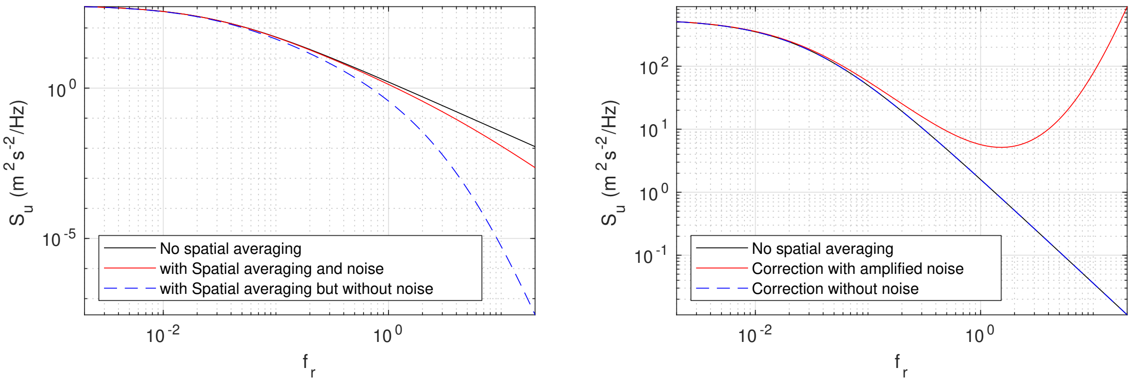


Figure 1: Left: Velocity spectra affected by spatial averaging with and without the presence of noise. Right: Amplification of the noise level by correction the velocity spectra using an idealized spectral transfer function.

The right panel of fig. 1 shows similar behaviour as in the right panel of your Fig. 5. Even if the random noise is small, the application of an idealized spectral correction will likely produce a velocity spectrum with an unacceptable level of noise in the high-frequency range. The misalignment of the scanning beam with the instantaneous wind direction might also contribute to the overestimation of the corrected power spectral density in the high-frequency range. As stated by the authors, the different probe volume length could also affect the high frequencies velocity fluctuations, but I am not sure to what extent. I believe that different probe volume lengths are likely to affect more the low-frequency velocity fluctuations than the high-frequency ones. In this regards, your correction algorithm outperforms those based on an idealized spatial filtering function because it accounts for the random error in the spatially filtered velocity spectrum.

## References

Foken, T., Wichura, B.. Tools for quality assessment of surface-based flux measurements. Agricultural and forest meteorology 1996;78(1-2):83–105.

Kaimal, J.C., Finnigan, J.J.. Atmospheric boundary layer flows: their structure and measurement. Oxford university press; 1994.