Author’s Response

On the second referee comment on "Modelling the Spectral Shape of Continuous-Wave Lidar Measurements in a Turbulent Wind Tunnel" by Marijn Floris van Dooren et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-233-RC2, 2021

26.11.2021

Marijn Floris van Dooren et al.

Dear Sir/Madam,

Thank you very much for taking the time to review our pre-print manuscript and for your helpful feedback and questions. We sincerely appreciate your explicit mention that this work deserves publication in Atmospheric Measurement Techniques. We will rephrase your comments in blue and include our response in black.

General comments

1. The repeated reference to better correlation for TI = 22% than TI = 3%... I believe the RMSE is lower for the TI = 3%, and the better R^2 correlation of the high TI case is just a function of the range of velocities used in the linear regression. The authors’ explanation about relatively higher energy content in the low frequency range (that the lidar can resolve well) for the higher TI case is understood, but the reviewer wonders if this is really a critical piece of the puzzle or not. See my comments further below.

We checked the root-mean-square error (RMSE) for both the 3% and 22% turbulence intensity (TI) cases and confirmed they are 0.22 m/s and 0.65 m/s, respectively. Your assumption that the RMSE would be lower for the 3% TI case is therefore proven right. We agree that the differences in the R^2 values might also be caused by the different range of the measured values and will adjust our explanation accordingly.

2. The discussion and reasoning around Eq. (11) is a little bit sparse and not completely intuitive.

Usually it is assumed that lidar noise is a property of the measurement device itself, and thus uncorrelated to the measured flow. However, our findings show that the noise level significantly increases when going towards more energetic flows, with higher wind speeds. The dependency of σ_η on u_∞ and ε was identified by having the best fit with respect to a manually fitted value. Please see our more elaborate reasoning as an answer to your comment at **L212**.

3. Dual-Doppler vs single-Doppler. It is not entirely clear to me that the dual-Doppler data belongs in this paper, since the main results in this article only use WS2 (because the spectral transfer function is for a single-Doppler only). I would ask that the focus on the dual Doppler reconstruction be removed or justified better. Moreover, the authors state in more than one place that the dual-Doppler reconstruction is better than either of the individual wind scanners for the u component of velocity. I don’t think this is supported by Figure 17/18 or Table 4. If the dual-Doppler is NOT better, this doesn’t overshadow the usefulness of a dual-Doppler technique which can resolve two components of velocity, but the text appears misleading about the u-component results. (I also noticed that the dual-Doppler results are not even mentioned in the abstract, which further makes me wonder: Why even have the dual Doppler results in this paper?)

It is a very fair point of the reviewer that the paper focuses on dual-Doppler too much, although the main results are based on the measurements of a single WindScanner. The research presented in the paper is based on a measurement campaign that was designed for the measurement of the two-dimensional wind speed through the wind tunnel. As this has major implications for the measurement setup, we would like to keep the notion of dual-Doppler in the paper and include it in the comparison of statistics and goodness of fit coefficients. However, the analysis of the v-component (**L369-384**) does not contribute enough to the main objectives of the paper and is therefore omitted.

The strong statement that the dual-Doppler reconstruction is generally performing better than the projected wind speeds based on the single WindScanners is not always correct, as you have rightfully mentioned. That is why statements in both **L338-339** and in **L427-429** have been omitted.

Specific comments

Section 3

L173-174: Does the work of Sjöholm *et al*. and Angelou *et al*. specifically describe the higher frequencies in the lidar spectra as white noise? While it may seem obvious to some, there is not strong justification given for why white noise (i.e. the Gaussian distribution) is chosen. As this is a main advance of the paper, I think a little more information is warranted.

Sjöholm *et al*. (2009) mention '…the noise induced feature at the very highest frequencies…' which implies they suggest that the highest frequencies in the spectrum are affected by noise. The type of noise (e.g. Gaussian, white noise) is not specified in this article. According to Angelou *et al*. (2012), however, the power spectral density function having an increased amplitude for the higher frequency range, is said to be '…probably due to white noise…'. Also, according to our own findings, it is a fair to assume that the noise at the higher frequency range can be classified as Gaussian, white noise, since the spectrum tends towards a horizontal line at the highest frequencies, which is a characteristic of white noise. Such a statement will be added to the paper.

L212: It is not self-evident that σ_η should be a function of energy dissipation rate or wind speed. Is the energy dissipation and wind speed related to the decorrelation time? Is this measurement shot-noise limited? Could you explain this more?

We were not able to identify references that suggest a relationship between the lidar spectral noise level and flow parameters such as the energy dissipation rate and mean wind speed. However, we found very clear indications that such a connection does exist and would like to elaborate on it. First, we started with the assumption that the noise in a lidar measurement should be related to random fluctuations of the backscattered signal only, and that this is a property inherent to the lidar measurement principle and not to the physical properties of the turbulent flow. However, in our analysis we saw a convincing increase of the noise level for more energetic flows with higher wind speeds. We have evaluated various possible dependencies. The following lists the steps describing our empirical analysis of the lidar spectral noise estimate:

1. For each case (1a, 1b and 2a-2e) we manually tuned the lidar noise standard deviation σ_η to the model in Eq. (10) for the best possible match between modelled and measured lidar spectrum.
2. With a linear regression, we then tried to identify a parameter or a combination of parameters that could best match those tuned values for σ_η.
3. In the end the best fit was found for the square root of the product of energy dissipation rate ε and mean wind speed μ_u.

Figure 1 shows the relationship of σ_η with the mean wind speed μ_u and the standard deviation σ_u. The fit is not convincing, although the dimensions match.

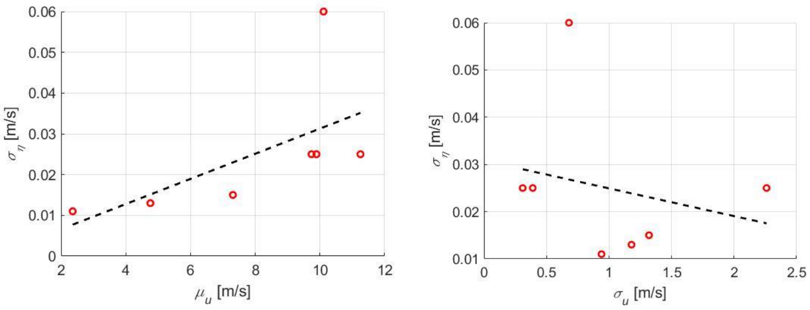

Figure 1: *Plots of the relationship of the lidar spectral noise standard deviation σ_η with the mean wind speed μ_u and the standard deviation σ_u, respectively*.

Figure 2 displays the relationship of σ_η with three different definitions of the coefficient of variance c_v, which is like the standard deviation σ_u, but only considers the small-scale fluctuations, which are most likely to influence the lidar noise. The three plots look similar, although the absolute values are different. The fit is a significant improvement compared to the standard deviation σ_u of the full time series. The unit matches to m/s.

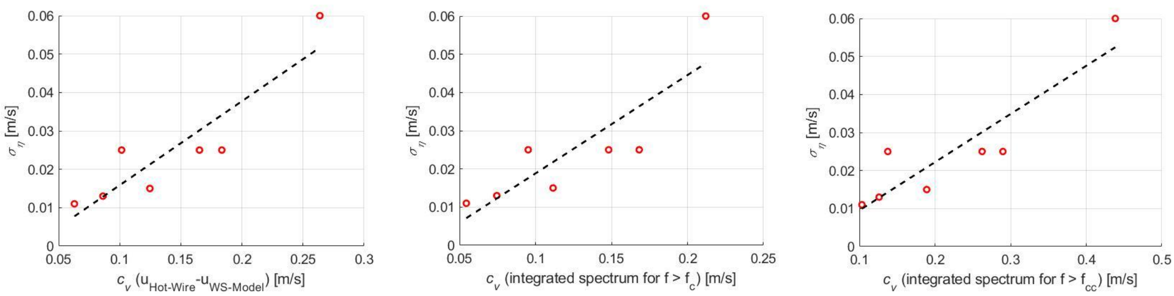

Figure 2: *Plots of the relationship of the lidar spectral noise standard deviation σ_η with the coefficient of variation c_v calculated in different ways; Left: Difference between the modelled lidar time series (only Lorentzian filter without added noise) and the hot-wire time series. Middle: Integrated coefficient of variance of the hot-wire spectrum from f_c to infinity. Right: Integrated coefficient of variance of the hot-wire spectrum from f_{cc} to infinity.*

Finally, Fig. 3 shows the more convincing relationships that include the energy dissipation rate ε (with the unit m^2/s^3), both alone and in combination with the mean wind speed μ_u.

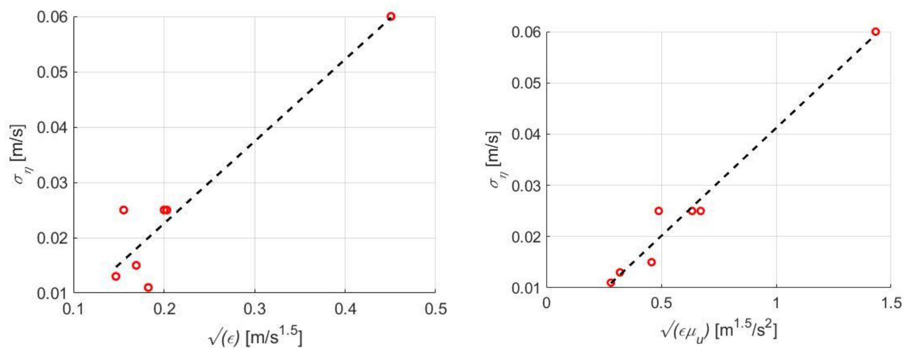

Figure 3: *Plots of the relationship of the lidar spectral noise standard deviation* σ_η *with the square root of the energy dissipation rate and the product of it with the mean wind speed, respectively*.

Based on this thorough empirical analysis, we decided that there is a convincing relationship between the noise standard deviation and the energy dissipation rate in combination with the mean wind speed of the flow. Our interpretation of this phenomenon is that more energetic flows, with a higher energy dissipation rate, inhabit more pronounced fluctuations and gradients in the probe volume of the cw-lidar, which result in a higher uncertainty in the estimated wind speed that influences the small-scale fluctuations in the cw-lidar time series, regardless of the Lorentzian low-pass filter effect.

This hypothesis needs more elaboration. Most likely the white noise is partly due to shot noise of the lidar, and partly related to global flow parameters. Please note that we are not suggesting the white noise is in any way correlated to the flow in the time domain, it is mainly the notion that global flow parameters could influence the absolute value of the lidar noise standard deviation. The model is not yet complete and might rely on further flow parameters, e.g. time and length scales, and lidar parameters.

Since we are convinced that this could be an interesting finding, we would like to keep the adjusted model described by Eq. (11) in the paper. Currently the empirical analysis is not included in the paper itself, but if this is deemed necessary, we will add (part of) this elaboration as an appendix.

Eq. (11): Why is gravity a relevant variable in the dimensional analysis?

We tried to make the units match by including physical constants, of which the gravitational acceleration parameter seemed to be the best candidate. However, we agree with you that it should not physically play a role in this relationship so we left it out and accept a constant with a unit instead, indicating that there might still be unidentified parameters playing a role in the estimation of the lidar spectral noise level.

Section 4.1

L255: I don't know if I would put so much weight on R^2 values here. I wonder how the RMSE compares between figures 9 and 10. The larger spread of u from a turbulent field seems to be giving higher R^2 even though there is clearly more absolute variation between u_p and u over most of the range for the more turbulent case. If you were to run the lower TI case at a freestream velocity of both 5 m/s and 15 m/s (i.e., over the same range as shown for the high TI case), the $R^2$of the combined data for the low TI case would be larger than for the high TI case, right?

The first referee also pointed out that the R^2 values of the instantaneous measurements for the 3% and 22% turbulence cases are not particularly relevant, unless the time series are smoothed with a moving average before, to filter out the larger fluctuations and improve the comparability. We have decided to follow this advice.

We used a window of 20 samples, since this way the effective averaging of the 451.7 Hz time series will yield a smoothed time series where frequencies above ~22.6 Hz are filtered out. This value is just below the lowest cut-off frequency of the lidar measurement modelled by means of the Lorentzian filter full width half maximum among the three presented cases. The effect of the smoothing on the goodness of fit coefficient for the three cases portrayed in the paper is listed in Table 1.

Table 1: *Improvement of the goodness of fit coefficient for the correlation between the smoothed WindScanner 2 and hot wire time series for three cases (1a, 1b and 2c).*

Case	Figure	R^2 (instantaneous)	R^2 (smoothed)
1a	9	0.656	0.790
1b	10	0.931	0.957
2c	16	0.950	0.975

The moving average procedure mostly benefits the goodness of fit coefficient for case 1a, while also slightly increasing the already high values of cases 1b and 2c.

In addition, we checked the root-mean-square error (RMSE) for both the 3% and 22% turbulence intensity (TI) cases and confirmed they are 0.22 m/s and 0.65 m/s, respectively.

L284-285: I understand that you are saying the small scales play a more dominant role for Figure 11 than Figure 12, which seems true based on the low frequency amplitudes of $S(f)$. The energy content at f_{cc} is still more than 10 times lower than at lower frequencies for Fig. 11, though. Why don't you integrate Figures 11 and 12 from 0 to f_{cc} and from f_{cc} to infinity. See what fraction of the turbulence is not fully resolvable by the lidar and report this rather than emphasizing the difference in R^2 values, which doesn't seem as relevant to me.

We followed up on your idea and integrated the power spectral density of the time series of WindScanner 2 to yield an equivalent variance of both the low and high frequency ranges, as a measure of how much energy is contained within the respective scales. We use the fact that the integral of the power spectral density function over frequency yields the variance. We define σ_{u_l} and σ_{u_h} for the standard deviation of the low and high frequency range of the time series u_p, respectively:

$$\sigma_{u_l}^2 = \int_0^{f_{cc}} fS(f)df$$

$$\sigma_{u_h}^2 = \int_{f_{cc}}^{\infty} fS(f)df$$

The resulting quantities, as well as their ratio, can be read off Table 2:

Table 2: *Comparison of the equivalent variance of the low and high frequency ranges of the WindScanner 2 projected wind speed time series.*

Case	TI [%]	$\sigma_{u_l}^2$ [m^2/s^2]	$\sigma_{u_h}^2$ [m^2/s^2]	$\frac{\sigma_{u_h}^2}{\sigma_{u_l}^2+\sigma_{u_h}^2}$ [%]
1a	3	0.074	0.019	20.4
1b	22	4.9	0.19	3.8

As suspected, the contribution to the variance by the scales that are not fully resolvable by the lidar is higher for the case of 3% turbulence, with a ratio of 20.4% to 3.8%. These numbers will be reported in the paper. However, as explained in the response at **L255**, we will not completely omit the correlation plots and the reported R^2 values.

L289: I was expecting this line to say, "A possible reason is the insufficiency of the Full Width at Half Maximum metric to characterize the effective probe length." Don't you agree? What about the implicit assumption that the turbulence is isotropic, could this also be a possible culprit?

Thank you for providing an alternative explanation for the much lower frequencies at which the lidar spectrum deviates from the hot wire spectrum. We agree that a likely reason is indeed the insufficiency of the full width half maximum definition, however we would still like to also mention the misalignment of the probe volume with the x-axis, which could invalidate the assumption of isotropic turbulence along the line-of-sight. We reformulated the sentence as follows: 'Possible reasons for this are the insufficiency of the Full Width at Half Maximum metric to characterise the effective probe length, and the invalidity of the assumption of isotropic turbulence, combined with the misalignment between the line-of-sight and the x-direction'.

Section 4.2

L325: This is the first time you've mentioned 1 Hz averaged time series. Could you please give a brief mention of why you perform this time averaging (I assume to get out of the small eddy range that can't be resolved by the lidar).

You are right about the reason for the 1 Hz averaged time series correlation. The goal is to eliminate the range of small-scale turbulence that cannot be resolved by the cw-lidar. This mention is now included in the paper.

L331: Can you comment on why the green line is not the highest for the 1 Hz data. Is it that at 452 Hz, the two lidars are both filtering small-scale turbulence and thus agree quite closely compared to the unfiltered hot-wire, but at 1 Hz, both the hot-wire and lidar are on more even playing field and can both resolve all the scales?

After applying a moving average window, as explained in the response at **L255**, the green line (comparing the two WindScanners with each other) is neither the best for the 451.7 Hz time series nor for the 1 Hz time series. Although the WindScanners are theoretically identical devices, there are tolerances in the optical system that may cause differences in the measurement. This will be mentioned in the corresponding section of the paper. Your explanation about the hot wire and lidar being on a more even playing field at lower sampling rates could also apply, though.

L338: The lowest errors appear to be found for the blue line not the black line, and a quick subtraction of the columns in Table 4 suggests that the mean difference between WS1 and HW is smaller than between WS and HW. Please revise this statement or justify it. Is the mean error of the dual-Doppler reconstruction related to the fact that the hot-wire only measures one component? Why is validating the dual-Doppler reconstruction given so much weight in this paper?

After reconsidering the plots in Fig. 18 of the paper, we agree with your observation. However, the order of the lines changed slightly after performing the moving average procedure as explained before. On top of that we now replaced the relative mean wind speed difference with both mean average error (MAE) and root-mean-square error (RMSE) plots, which are more common ways to address errors. The paragraph describing the figures is updated accordingly.

It is likely that the mean error of the dual-Doppler reconstructed wind speed with respect to the hot wire is related to the one-dimensional nature of the hot wire measurement. However, considering the usually very low lateral wind speed components in the wind tunnel, the magnitude of the errors found cannot be explained by that exclusively. We believe that a larger contribution is coming from the heterogeneity of the wind conditions over the effective probe volume of the lidars, compared to the single point at which the hot wire is mounted, and over the 7 cm separation between the lidars' focus point and the hot wire location.

The dual-Doppler reconstruction should play a lesser role in the paper, as we have described in the answer to your 3rd general comment. We would like to have this result in the plot as a reference but will put less emphasis on it.

L342: You say there is a "bias between WindScanner 1 and 2, which increases linearly with the mean wind speed". This is not obvious from the plot except moving from $\mu_u \sim 2$ m/s to $\mu_u \sim 5$ m/s where the gap widens between red and blue. Please revise or justify.

The statement of the 'linear increase of the error' can be justified when multiplying the relatively constant percentual increase (over the range between 5 m/s and 11 m/s) with the absolute wind speed values. However, we acknowledge that this is a confusing statement that has been rephrased as a 'relative bias'.

L350: You say, "they do not have physical significance". Please clarify your statement about physical significance as this is clearly a physical phenomenon in the flow that is being resolved by both measurement systems.

We agree that it is incorrect to label the frequency peaks as 'no physical significance'. The first referee also pointed this out. We changed the wording to 'As they result from external gust variations, these peaks are not deemed to be due to turbulence and have a much larger scale than the lowest scales detectable by the WindScanners'.

Table 5: I wonder if the ratios of f_c/f_{cc} in this table are possibly more important in the long run than the 0.5 coherence observation, since in a real application of this technique, you will not have a reference instrument to calculate coherence, right? Would it be appropriate to suggest that the effective probe volume given by the FWHM could be at most an order of magnitude in error based on this data?

We agree with the conclusion that the Full Width at Half Maximum is not sufficient as a length scale for defining the extent of the spatial filtering effect in the probe volume. The noticeable filtering effect indeed occurs over a range that is around an order of magnitude larger than the 'classic' probe length definition. However, we think it would be a too strong statement based on the limited data set to define an 'effective probe length' with an order of magnitude larger than the other definition. We did add a mention in the Conclusion section of the paper about it.

L375: I think this is a good conclusion to draw. It looks like the amplitude of the protocol-induced gust is larger in the u rather than v-direction – could this be a reason why the triple repetition is being lost in Figure 22? Reading further, I see that these differences are quantified in Table 6. If you believe my argument, I think you could comment on how the fact that $\sigma_v/\sigma_u \ll 100\%$ might be related to your conclusion in line 375.

It is indeed true that the amplitude of the induced gust is much larger in the u-component, which is the variable meant to be influenced by the active grid protocol. Although designing active grid protocols with the purpose of simulating the v-component should be possible, it was not applied in this case. However, we disagree that the triple gust is completely lost in Fig. 22, since there is still a visible signature, albeit much less pronounced.

As we have stated in the answer to your 3rd general comment, we decided to omit the analysis of the v-component, since the dual-Doppler reconstruction is not the main objective of this paper.

Section 4.3

L392: You say, “the latter curve is not valid for large-scale structures”. Just to clarify, is this because it is only derived for the inertial subrange?

Your presumption is correct; the analytical formulas for the modelled lidar spectrum are specifically derived for the inertial sub-range. This notion is now included.

L415: The potential application is very interesting and seems worthwhile. You have used the word “atmospheric” twice in the last three paragraphs. From the introduction of the paper, I was under the impression that you want to use the dual lidar technology in wind tunnel studies of wind turbine configurations? Could you clarify here (and in the abstract/introduction) if your aim is for wind tunnel or field measurements (or both)?

Actually, we do not exclusively reserve the word ‘atmospheric’ for wind conditions in the free field, but also for the flow through our wind tunnel. We would like to refer to **L20** where we state that ‘Existing wind tunnels can simulate the atmospheric boundary layer through passive flow manipulation…’. However, we understand that the definition ‘atmospheric’ might be confusing for the general statement made here, so we chose to omit it from this sentence.

Having said this, we believe that it would be an interesting comparison whether the models presented in our paper would also work for atmospheric flow measured in the free field. We have added the following sentence at the end of the Abstract of the paper: ‘Although the models were developed on the basis of wind tunnel measurements, the application on free field measurements should be possible as well.’

Section 5

L427: In reference to 1.1%, you say that the dual-Doppler gives “lower spread”. However, the 1.1% comes from an analysis of mean error, not scatter. Could you clarify this wording?

Thank you for correcting the wording here, where we should have used ‘mean error’ instead of ‘spread’. However, since we have now also included the analysis of the RMSE of the time series, there will be additional lines addressing that as well.

Technical comments

Fig. 1: It might be more useful if it included a zoom in of the nozzle with the active grid.

We have considered providing a close-up of the active grid. However, for the interpretation of the results in this paper, we believe that it is more valuable to know what the inside of the wind tunnel looked like during the measurement. We would prefer to refer to Kröger *et al*. (2018), who described the wind tunnel and the active grid in more detail, and included several photos.

L68: Add “to” before reproduce.

We added the preposition ‘to’ to this sentence.

Table 2: I think it would be appropriate to give the names of the variables in Table 2 and not just the symbols.

Since this table contains variables that are not introduced until later, the names of the variables have been added.

L140: You mention that L is the probe length twice.

The probe length L was introduced twice. We removed the redundancy and merged the two respective sentences into one.

Fig. 11/12: Could you note in the caption that f_{cc} will be defined later in Section X?

We added the notion that the variable f_{cc} will be defined later in Subsect. 4.2 at the end of **L265**, as opposed to in the captions of both Fig. 11 and Fig. 12, to avoid redundancy.

L327-328: No need to describe what the different colored lines mean since it's in the figure.

We agree to remove the redundant lines that describe the meaning of the graph colours, as the figure caption should be sufficient.

L346: No need to describe the line colors in the text.

We agree to the removal of the graph colour definitions here, since at this point in the paper it should have already been clear from Fig. 11 and Fig. 12.

L427: You write "down to -1.1%." Can you say "within 1.1%" instead to be more precise?

We agree with you that changing the wording 'down to -1.1%' to 'within 1.1%' makes sense. However, since we are now using the MAE and the RMSE instead of the straight difference, the paragraph describing these results has changed.

References

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