Response to Anonymous Referee #2

Review of the manuscript AMT-2022-73, entitled "Behavior and Mechanisms of Doppler Wind Lidar Error in Varying Stability Regimes", by R. Robey and J. Lundquist

This study focuses on the errors associated with the measurements performed with a pulsed profiling Lidar when estimating mean wind direction and speed for the atmospheric boundary layer under different atmospheric stability regimes. The problem is tackled from a numerical standpoint through the virtual lidar technique, namely by sampling the wind field simulated with the WRF- LES model through a Doppler Beam Swing (DBS) scan. Two main sources of error are identified, namely the horizontal heterogeneity and range-weighting function (RWF). These errors are investigated through the random-variable theory and convolution integrals, respectively.

The topic is definitely of interest and thoroughly examined in the manuscript. However, a major point that could be improved is the clarity of the language throughout the manuscript. Some statements may result a bit cumbersome and need to be read several times for a thorough understanding (more details in the following). Furthermore, motivations and discussions are qualitatively reported without including details and references, particularly in Sections 3 and 4. The quality of the figures (in terms of labels and panel size) could be improved. Finally, from a technical standpoint, I have noticed some confusion in defining the temporal full-width half- maximum (FWHM) of the lidar pulse, range gate, and accumulation time, which are independent parameters (more details reported in the following). Given their importance for this work, I recommend defining them more clearly.

We would like to thank the reviewer for their thorough reading and constructive comments. The feedback has been helpful in ensuring the technical details are correct and accurately conveyed.

Specific comments

L164: Earlier you referred to τm as "temporal range gate"; please avoid the introduction of new terminology unless strictly necessary. Also, the accumulation time is the time interval when the back-scattered signals are collected from a certain distance and ensemble-averaged in the Fourier space to single out the Doppler shift. Thus, it is independent of what here has been called "temporal range gate" (which is the spatial range gate divided by the speed of light); also, referring to Table 2, τm = 265ns is too small to be an accumulation time (typically ranging from 0.5 s to few seconds). Please address this point.

Thank you for catching this. We made an unfortunate overload of the term 'accumulation time', which should, as you say, refer to time used to collect an ensemble of samples the average of which is used to diagnose the Doppler shift. The discussion of the "accumulation time" was mis-used here to refer to the temporal range gate. We apologize for the confusion the oversight caused and have fixed the wording to remove the mis-used language and clarify our meaning.

> "The choices of pulse and range-gate parameters in a coherent lidar system must balance the desire for spatial locality (reducing the width of the RWF) and the need for adequate accumulation time (τ_m) to accurately resolve frequencies used for the radial velocity measurement."
>
> was changed to
>
> "The range-gate parameter in a coherent lidar system must balance the desire for spatial locality and the need for accurate frequencies used in measuring the radial velocity. The more signal points from the traveling pulse used, i.e. the longer the range gate, the more accurate the diagnosis of the frequencies but the longer the averaging volume along the beam." (LL173-175)

In this section, we wanted to make the distinction between the range gate parameter in time (the 'temporal range gate', τ_m) and the corresponding spatial distance in a range gate (the 'spatial range gate', Δp). The part mis-using the "accumulation time" was intended to make note of how the choice of τ_m is subject to a trade-off: more points used in the FFT, i.e. a longer range gate, gives more accurate frequencies but requires a larger probe volume / averaging over a longer spatial range gate along the beam which loses locality of the measurement.

L166: To my understanding, Fig. 2b, c and d refer to time-averaged velocity profiles, which might be marginally affected by the shape of the RWF. The profiles of the Reynolds stresses may be more relevant for this discussion

The panels in Fig. 2(b, c, d) are "instantaneous" line-of-sight velocity profiles (implicitly averaged over the grid/time step of the LES model) to which we have applied the RWF. We have edited the caption to make this explicit.

Because of limitations in the current virtual lidar model, we do not treat the effects of accumulation time / time averaging in the beam sampling; the measured line-of-sight velocity is taken directly from the RWF acting on a snapshot of the LES wind. Future/improved versions of the model would have to treat this point more carefully with the turbulent stresses as represented by the LES, but for this iteration of the model the raw line-of-sight velocity profile is appropriate to the underlying behavior.

LL 292-294: Using temporal FWHM of 165 ns (as reported in table 2), I obtain a range gate of (Frehlich & Cornman, 2002):
If you used this relationship, please state it in the text with reference. Also, the effective range gate of the pulsed LiDAR system is defined as the temporal FWHM (multiplied by the speed of light) plus the range gate (Frehlich & Cornman, 2002) (265ns from table 2, corresponding to 79.5m). Please clarify this aspect.

For the effective range gate / range resolution along the beam due to the RWF we directly solved for the FWHM of the RWF presented in the paper (Eq 6. with the parameters you give above, resulting in ~43 m). It is our understanding that, as summarized in Cariou and Boquet (2010, p 20), a later estimate of the effective range resolution by Banakh and Smalikho (1994) is indeed about one and a half times shorter than that found using the sum of the FWHM and range gate as in Frehlich & Cornman (2002). This estimate is in line with the FWHM of the RWF arising from the convolution. Hopefully this accounts for the disparity in our numbers.

We don't expect much sensitivity for range resolution values close to 40m. Comparing to preliminary runs using 40m high cylinders for the volume average truth, using the projected ~35m height didn't have an appreciable effect on the results.

L310: In my opinion, an important point missing in this analysis, with respect to a real- scale experiment, is the decreasing of the backscatter coefficient moving away from the lidar, and, thus, the carrier-to-noise ratio (CNR). In other words, the presence of noise in the backscattered signal can severely impair the outcome from the lidar, and, thus, the velocity statistics. I understand that this effect is out of the scope of this analysis, but it could be mentioned when the error analysis is carried out.

The CNR is out of scope, but can be significant to real measurements. We have noted its role and omission along with our disclaimer about the ignored factors in the quality of the radial velocity retrieval..

Added to L 137: "We similarly omit impacts of the carrier-to-noise ratio which can introduce additional uncertainty into the diagnosis of the radial velocity (Cariou and Boquet, 2010; Aitken et al.)."

L323-331: This part would be clearer with a figure reporting vertical profiles of statistical estimators of the error. Please remove Appendix A and put the relative figure here.

(Section 3 presenting the results has changed in rearranging the order of the paper.)

After due consideration, we have decided to leave the full set of moments with the orientation disaggregation in Appendix A. The results section is already quite figure heavy and we did not feel that it was the best use of space to show the skewness and kurtosis in the main text when they feature only briefly in the discussion.

LL336-340: From figure 5 (leftmost panel), I expected the mean error for the volume- averaged case to be lower than that for the 'tower' case considering the lower standard deviation. Can you please add some comments on this feature?

This behavior is related to the mechanics of the additional analysis we have added comparing the scalar-averaged wind speeds of lidar and pointwise (cup anemometer) measurements.

In expanding the wind speed error from which we obtain that the bias is proportional to the u,v error variance, the lidar-derived winds were expanded about the volume-averaged reference wind. This process doesn't really make sense to do in the same way with a point reference which is why the biases using the two references don't directly track from the error standard deviations in both cases. (now noted in L425)

If we think about expanding the pointwise measurement about the volume-average as well (or a Reynolds average) as explored in the added analysis (Section 2.3.5), then the pointwise wind speed will also have a positive bias / inflation relative to that of the volume-averaged wind speed. The biases are proportional to the variance of the fluctuations in the u and v measurements by the lidar or at the point. So part of the bias between the lidar and volume-average is canceled by the additional variability in the point measurement, rather than it being additive to the bias, giving less total bias compared to the point measurement.

L427-428: Is this velocity still a function of time? If so, please add this detail.

Yes; we will clarify here. We are letting the velocity vary in time but assuming the volume-average is about constant over the 5s duration of the scan.

LL444-445: In general, the homogeneity is not violated due to the presence of turbulence. A flow can be homogeneous in a certain direction (i.e. having the same p.d.f. for all the sampled points) even in presence of turbulence. The lack of homogeneity is given by local spatio-temporal variations of the mean flow and turbulence statistics. Please rephrase this sentence accordingly, if you agree.

This is an important point that we want to make sure is clear in the text.

We have used 'homogeneity' in the sense of the 'horizontal homogeneity assumption', meaning the assumption that winds are uniform/constant across the scan volume so that the reconstruction is exact. This phrasing is common in the literature when referring to the reconstruction of the 3D winds (see below). We recognize that our usage may cause confusion, especially if the reader thinks that we are talking about homogeneous turbulence.

We have adjusted our wording here (L324-327) to be explicit that we are referring to the horizontal homogeneity assumption and clarified usage of the phrase when it is introduced. The LES test cases were designed to be quasi-stationary and quasi-homogeneous (in the turbulence sense) so that the assumption of a *constant*/*uniform* wind across the horizontal scan volume is violated even in the homogeneous flow due to the momentary realization of the turbulence.

Quantifying error of lidar and sodar Doppler beam swinging measurements of wind turbine wakes using computational fluid dynamics

Remote Sensing of Complex Flows by Doppler Wind Lidar: Issues and Preliminary Recommendations

Turbulent kinetic energy estimates from profiling wind LiDAR measurements and their potential for wind energy applications - ScienceDirect

L506-507: I am not sure why the RWF should impact the time-, streamwise- and spanwise- averaged velocity components for a homogeneous flow since the time and length scale of the flow are much greater than the probe volume. Please clarify.

In our model, the sampling of the line-of-sight velocity using the RWF acts on a snapshot of LES flow field (which contains some implicit spatial and temporal averaging in the LES-discretized representation of the flow). But the key point with the RWF bias is not the variability it introduces (it's small, Fig. 8) but the repeated action on the underlying, background wind profile. The volume average on a radial velocity profile with curvature is inducing a bias.

The mean bias effect persists even when acting on the time-, streamwise-, and span wise-averaged velocity because of the action of the RWF on curvature in the vertical profile of the velocity. In *Shear and Turbulence effects on Lidar Measurements* (Courtney, 2014) an analysis of this behavior on analytic representation of shear profiles is done, showing the induction of the bias and which our findings support. Some of our analysis generalizes the behavior to any RWF fitting certain symmetry / unit integral properties.

We have placed an additional reference to (Courtney, 2014) and to (Clive, 2008) in the discussion of the RWF results (now Section 3.1, LL597-605) and added Fig. 9 which shows the bias due to the RWF bias in the full model coincides with the action of the RWF on just the mean LES profiles.

Figure 12: If the aim is to highlight the flow heterogeneity, then instantaneous flow visualizations cannot provide this information. Flow visualizations would work better. On the other hand, if you just want to show the positioning of the Windcube V2 within the domain, this figure works fine.

Referring back to our response to the comment on the use of "homogeneity" (**LL444-445**), Fig. 12 (now Fig. 4), is meant to visualize typical turbulent structures alongside the lidar scan geometry. There shouldn't be meaningful *turbulent* heterogeneities in the test case flow fields (they are designed to be quasi-homogeneous), but the character and scales of the turbulence interact with the lidar scan geometry to produce error. While Fig. 4 does not tell us exhaustively about the error, we think it helps to build intuition about the typical scales with respect to the lidar and the resulting interaction, which is then treated more systematically via the model distribution results.

L653: Please provide more details about the trend line.

The trend line is the result of plotting the average of the errors of data binned by wind speed (bins of 0.5 m/s). With closer scrutiny of this plot we have adjusted our statement about the decay of the bias (LL685-692) and removed potentially noisy points in the trend line for bins with fewer than 2500 points. All other things equal, the error expression for the wind speed suggests that a higher wind speed should repress the positive bias ($1/|U_h|$) but separating the trend by stability (and height, not shown), we do not feel we can discern the expected trend from the other factors changing implicitly with wind speed.

LL 668-671: This improvement is not so evident in Figure 15. If you want to highlight it, for instance, you can plot the median Normalized Wind Speed Error (plus-minus 25th percentile) as a function of the misalignment.

Thanks for the feedback on this. Our key point is that we don't believe the orientation has a meaningful effect. We have added mean and standard deviation markings to the plot and amended the statement to consider the changes in the wind speed and direction error negligible with a small visible change in the (wind-direction-weighted) vertical velocity reconstruction.

L681: The term "noise" here is misleading. It can be replaced, for example, with "scattering" or "variability".

We replaced the use of "noise" with "variability" here. (Now in section 2.3.5)

LL 702-704: This sentence is unclear. It might not be correct to mention "mean error" referred to individual measurements. Further, it is unclear what the authors mean by saying "the time average approaches the potentially less biased 'ensemble' error from the selective sub- sample". Please clarify or rephrase it.

"The mean error in an individual measurement may benefit from time averaging, however, due to the time average approaches the potentially less biased ensemble error from the selective sub-sample (e.g. Fig. 7)."

Thanks for pointing this out. We agree about the "mean error" being a poor/incorrect way of referring to the error in this case. We wanted to make the distinction that the "mean" of measurements in the time series from a single instrument, i.e. the time-averaged measurement, can have an (unsystematic) bias and that this “mean” improves with longer time averaging. This behavior really reflected by the variance of the time-averaged measurement error and this sentence only confuses the issue and we have dropped it.

L705: To my understanding, here you refer to the variance (i.e. the uncertainty) associated with the mean of the random process, which is a function both of the variability of the process and the number of samples. Please clarify this aspect.

(Now in Section 2.3.5 discussing the analysis of time-averaging effect. LL474-487)

Yes; the error spread is referring to the variance of the error which was discussed in earlier sections (and we have reworded to call it so explicitly).

This is exactly the analysis of the (time) mean of a random process. The 1-Hz measurement of the winds (the random process) is associated with a random variability/distribution connected to the character of the turbulent variability in the flow. The reduction in the variance of the mean of a random process is dependent on (inversely proportional to) the number of samples, N; however, the correlations in the time series lower the effective sample size. We have introduced (Lumley and Panofsky, 1964) to help cut more directly to our point about the expected scaling of the variance in a time-average.

L708-709: This observation is qualitative and potentially not true. Please either remove this sentence or provide references.

(Now in Section 2.3.5.)

“In the lidar, the pattern of turbulent structures that gave rise to a particular error continue to influence the error in the following samples as well so that the errors are quite similar.”

We agree that the wording of this statement is perhaps too strong and depends on implicit assumptions about the turbulent scales, speed of the scanning cycle, and decorrelation times which may not always hold. Our purpose here was to make clear why the errors in the time-series cannot be assumed to be independent samples. We have deleted this sentence and simply left it at the preceding sentence, “Samples cannot simply be treated as independent since subsequent samples can be highly correlated.” (L477-478)

L717: This assumption is correct, but it relies entirely upon the estimate of the decorrelation time τc. Please provide details about its calculation. Or, as an alternative, if you are interested in a more precise estimate of the standard deviation error, a good reference is Benedict & Gould (1996).

(Now Section 3.3, time averaging results.)

Thanks for the input here. The exact decorrelation time τc was not intended to be a focal point but rather as a more conceptual step to reach the conclusion about the rate of reduction for the error variance/standard deviation. Upon reflection, we have decided to replace Fig. 16 (now Fig. 15) with a plot that we think better illustrates the trends we wanted to convey (i.e. the rate of the reduction in the standard deviation and decay of the wind speed over-estimate bias) and which does not depend on an estimate of the decorrelation time. Showing the reductions over time directly also allows the decorrelation rate to appear implicitly in its effect on the speed of the decay in the different stability cases.

(And thanks for the reference; we regret we won’t have time to incorporate it into this work, but appreciate the recommendation.)

L739: What do you mean with “background signal”? Do you refer to the time or space average?

Yes; we’re referring here to the time- and scan-volume-averaged vertical velocity. We’ve replaced “background signal” with "background, spatio-temporal average” to be clear. (L803)

L900: This equality is unclear. Please clarify and/or rephrase it.

We have reworked this derivation (Appendix B) to make the behavior and assumptions more explicit. We had originally immediately dropped the covariance term on the assumption that $Vu_{err} - Uv_{err}$ and $|\vec{U}_h|^2 + Uu_{err} + Vv_{err}$ were uncorrelated based on the reasoning that the error vector direction and magnitude are fairly uniformly distributed about the circle. We have kept the covariance term in the reworked derivation to treat it explicitly and called out the underlying assumption. We have also introduced a reference (Kendall, 1994) to skip directly to the approximated form for the expectation of a ratio instead of performing the Taylor series expansion ourselves, which keeps the focus instead on the expected behaviors of the numerator and denominator.

Technical comments:

L15: Which velocity components are named u, v? Changed to 'horizontal velocities' here since we do not meaningfully distinguish between the two.

L21: Please add the meaning of the acronym lidar. Done.

L31: Please state clearly that the measured velocity is the along-beam, i.e. radial or line-of-sight, component. Added.

L105: Also mention the conclusion reported in Sec. 5. Added.

L114: For the sake of clarity, please put table 2 at this point of the discussion. Moved up to be introduced with the RWF.

L150: Please mention that Δp is the lidar range gate. Included before its first use in Eq. 4. (L158)

LL194-195: This sentence is unclear, please rephrase it. On recommendation of the other reviewer, we have removed the paragraph with this sentence as scanning lidar instruments are not directly relevant to the current study.

Table 2: Please add one row reporting the azimuth angles used by the DBS scan. Added.

L297: Please revise this sentence as it sounds unclear.

"The error incurred in any individual measurement does not necessarily represent general behaviors; deducing useful, generalizable trends entails focusing instead on distributions of the observation error."

changed to

"The error incurred in any individual measurement depends on the specific realization of turbulence during the measurement and is not necessarily representative of the full variability of possible error behavior. To deduce useful information about bias and typical error magnitudes that can be generalized to other measurements in the same conditions, we focus instead on the distribution of the observation error." (L497)

Figure 5: In the caption, instead of "dotted", you should refer to solid lines to indicate the volume-averaged reference. Thanks for catching this — reference to the solid and dashed lines is fixed and a legend added.

L358: Please add a reference to figure 6 here. This paragraph/sentence was broken up in the rearrangement of the manuscript.

LL391-392: To my understanding, the error metric is the same both for the pointwise and the volume-averaged reference. It would be more correct to say "[...] merging of the two error distribution profiles". We referred to the metrics of the distribution moments (mean/variance), but your wording is clearer and we have adopted it. (L731)

L451: Please provide a reference.

For mean and variance of linear combinations of random variables, we have added a reference:

Zwillinger, D. and Kokoska, S.: CRC Standard Probability and Statistics Tables and Formulae, Chapman & Hall/CRC, Boca Raton, Fla, 2000.

L559: I guess here you refer to figure 12. ~~Yes, fixed.~~ Rearranged.

L644: Please revise this sentence as it sounds unclear.

(Now in section 3.2)

"For measurements made in relatively steady, slow winds it cannot be expected that bias will not emerge as it does in the strong CBL data."

changed to

"Measurements made in conditions of slow winds of fairly consistent direction, as in the strong CBL case, do not benefit from the cancellation expected in an ensemble over all instrument orientations and should take into account the possibility of a persistent bias arising in the wind direction." (LL676-679)

LL659-661: This sentence is unclear, please rephrase it.

"The additional decay is likely accountable to the inverse tangent, which curtails the size of the largest errors compared to the bounding estimate as well as improved correlation at the heights with the strongest winds reducing the component errors."

changed to

"In some cases, the decay in the error magnitudes is greater than the anticipated $1/U_h$ bound. This may be in part because the inverse tangent in the full error expression (Eq. 21) should act to further curtail the size of the largest errors more than is captured in the bounding estimate (Eq. 22) and in part because of implicit correlation effects with height and wind speed." (LL696-698)

L706: Please replace "variables" with "samples".

The technical meaning of random variable was meant here; we have fixed "variable" to "random variable".

L716: Please provide any reference or derivation for this equation. Also, provide a brief description of the terms reported here, which have not been introduced before.

We have replaced our cursory discussion here reaching this inequality with a reference that cuts straight to the variance decay rate behavior (Lumley and Panofsky, 1964). We have fixed the inconsistent notation for the variance of the u velocity error and the remaining terms in the updated equation are defined in the preceding text.

L821: This study offers a relevant comparison for the present work, so it should be discussed at the beginning of the previous Section.

We have incorporated this reference (Teschke and Lehmann, 2017), which analytically treats the minimization of the error and bias with respect to the beam angle under simplifying assumptions, into the initial discussion of the beam angle. (L357)

L**884**: You can replace this Appendix with simple literature references you deem pertinent.

Good point. We have replaced the appendix entry with a general reference for the moments and unbiased estimators:

Zwillinger, D. and Kokoska, S.: CRC Standard Probability and Statistics Tables and Formulae, Chapman & Hall/CRC, Boca Raton, Fla, 2000.

and a reference for the adjusted Fisher-Pearson skewness and kurtosis coefficients used:

Joanes, D. N. and Gill, C. A.: Comparing Measures of Sample Skewness and Kurtosis, Journal of the Royal Statistical Society. Series D (The Statistician), 47, 183–189, 1998.

L897: Do you refer to Equation 22 here? If so, please state it clearly. Yes, fixed.

References:

Benedict, L. H., and R. D. Gould. (1996). Towards better uncertainty estimates for turbulence statistics. *Experiments in Fluids,* 22(2), 129-136.

Frehlich, R., & Cornman, L. (2002). Estimating spatial velocity statistics with coherent Doppler lidar. *Journal of Atmospheric and Oceanic Technology*, *19*(3), 355–366.