Response to Reviewer 1

Manuscript Title: *Dual-lidar profilers for measuring atmospheric turbulence*
Manuscript ID: wes-2025-179
Journal: *Wind Energy Science*

Dear Jacob Mann,

We sincerely thank you for your careful reading of our manuscript and your comments. We have revised the manuscript accordingly. Below, we provide a point-by-point response to all comments. Your comments are shown in black, and our responses are shown in blue. Changes in the manuscript are highlighted or tracked as requested by the journal.

Reviewer 1

General comment

This paper investigates the line-of-sight variance method to extract turbulence from profiling lidars. The authors cleverly use two standard five-beam pulsed Doppler lidars rotated 45o relative to each other to essentially for a nine-beam lidars. This allows of obtaining the variances of the horizontal velocities without combining line-of-sight velocities instantaneously from different beams, similarly to how it is done in Sathe et al. (2015).

The method is tested with relevant offshore data where the turbulence estimations from the lidars are compared to sonic anemometer "ground truth".

The results are promising, but the impact of measurement volume averaging is still not successfully addressed. In general, the paper is publishable, but there are a number of comments that has to be addressed, and the paper is at places too long and textbook-like.

Response: Thank you for the positive overall assessment of the manuscript and for recognizing the novelty of the experimental configuration and the potential of the variance method for turbulence estimation from lidar profilers. We acknowledge your concern regarding the impact of measurement volume (probe-time and probe-length) averaging, which remains a fundamental limitation for turbulence retrievals from pulsed Doppler lidars. This limitation is now more clearly stated and discussed in the revised manuscript, including reference to recent complementary approaches aiming to mitigate probe-volume effects (e.g. Manami et al. (2025)). We also acknowledge your remark that parts of the manuscript were overly textbook-like, particularly in the methodology section. This section has been carefully revised and streamlined to reduce general background material, improve conciseness, and focus more directly on the specific implementation and assumptions relevant to the present study. As a result of these revisions, the total length of the manuscript has been reduced by approximately five pages. All other comments have been addressed in detail below.

Specific comments

Comment 1: l 65 – 69. The considerations here are not entirely correct. If you measure with an instrument with point-like measurement volume and high time-resolution, you will get the real turbulence variance. If you pick only one sample every second, you still get the correct turbulence variance, so you resolve all scales. Similarly, for the six-beam WindScanner setup it is not he 15 s that is important. It is the sample volume and the averaging time of the individual beam that determined what scales contribute to the variance. Had the sampling volume and the averaging time been small, then the variance would have been unbiased, even if the cycle were completed in 15 s.

Response: This passage has been removed in the revised manuscript, as it was not central to the main objectives (and also wrong) of the study and contributed unnecessarily to the length of the paper.

Comment 2: l 93. *Aldernay Race* sounds like a ship race, but it is a geographic location. Maybe you could help the reader realize that.

Response: The text has been revised to clarify that the Alderney Race is a geographic location (a tidal channel), in order to avoid confusion for the reader. p.3, l.79.

Comment 3: l 140. This comment relates to the first. There is no reason to downsample the cup anemometer for a consistent comparison. I think what is most relevant to compare with the the variance of the full cup anemometer signal. It is also unclear what the low-pass filter is doing in this comparison. Please specify the low-pass exactly, and why it is applied.

Response: The text has been revised to remove the physical interpretation associated with the downsampling. The downsampling of the sonic anemometer data is now described only as a practical step for point-by-point temporal comparison with the lidar measurements, without implying that it is required for a consistent variance comparison. The discussion of turbulence scales and low-pass filtering has been removed accordingly. p.5, l.122-124.

Comment 4: l 149. The choice of 30-min versus 10-min averages is very important. I think it is very reasonable, but it is not in the DNV error metrics. This point deserves some more emphasis.

Response: This point has been emphasized in the revised manuscript. The text now explicitly notes that the 30-min averaging window differs from the 10-min intervals used in DNV error metrics and clarifies that this choice was made deliberately to improve the statistical convergence of turbulence measurements. p.6, l.129-132.

Comment 5: l 162. Aerosol fall speed? Do you mean rain?

Response: The text has been revised to clarify that this refers to the motion of aerosol tracers and not to precipitation or rain. p.7, l.142-143.

Comment 6: l 176. A similar technique was applied by Mayor et al. (1997). Cite, if you see fit.

Response: Thank you for pointing out this study. The reference to Mayor et al. (1997) has been added to the manuscript at the relevant location. p.8, l.156.

Comment 7: sec 2.6.1. This section is too long and text book like. Please short considerable. Why is $\mathrm{TI}_u = \sqrt{\sigma_u^2}/U$ and not just $\mathrm{TI}_u = \sigma_u/U$? Same for Eqs 25+26.

Response: Section 2.6.1 has been considerably shortened by removing textbook-style derivations and repeated definitions, while retaining only the equations and descriptions required for the present analysis. In addition, the turbulence intensity notation has been clarified by defining σ_u and σ_v as standard deviations rather than variances. As a result, the turbulence intensities are now consistently expressed as $\mathrm{TI}_u = \sigma_u/U$ and $\mathrm{TI}_v = \sigma_v/U$, and Eqs. 13-14 and

25–26 have been removed. p.8-9, l.170-179, Eq. 3-4.

Comment 8: Eqs 16 + 17. Is this really how the variances are calculated? First you calculate the spectrum and then integrate over frequencies.

Response: The text has been revised to clarify this point. In the present analysis, LOS velocity variances are computed directly from the time series after noise correction. Variances obtained from spectral integration were also evaluated and were found to be equivalent. However, as no explicit spectral integration is required for the results presented in the manuscript, the spectral formulation could be misleading and has therefore been removed for clarity.

Comment 9: Eqs 18 + 19. There is something wrong with the notation (I don't think anything is basically wrong with the math). In 18, a 10 by 3 matrix is multiplied by a 3 by 3 matrix. This gives a 10 by 3 matrix. That is added to a 10 by 3 matrix, giving again a 10 by 3 matrix. It is unclear how that turns into a 10 by 6 matrix in 19. In 19 the LHS is a matrix, but you refer to the elements (e.g $Q_{q,m}$). Are you summing over repeated indices? Please clean up the notation for easier reading.

Response: The notation has been revised for clarity. The transformation matrix $\mathbf{T}$ is now explicitly defined as a 10×3 matrix, and the construction of the 10×6 matrix $\mathbf{Q}$ is clarified by defining it row-wise from quadratic combinations of the elements of $\mathbf{T}$. This makes explicit how the LOS variance vector is related to the six independent components of the Reynolds stress tensor and removes ambiguity regarding matrix dimensions, index usage, and implicit summation, while preserving the original mathematical formulation. p.9-10, Eq. 6-9.

Comment 10: l 319. The abbreviations MRSE and RRMSE were defined in the abstract. Maybe it would be helpful to do it again here.

Response: The abbreviations MRSE and RRMSE are now redefined at their first occurrence in the main text (introduction section) to improve clarity. p.3, l.87-88.

Comment 11: l 345. Why not simply force the fits through zero? (late, in l 371 you actually state that, which I think is good)

Response: All material related to this comment has been removed, as Section 3.1 (Variances) has been deleted in the revised manuscript for conciseness and relevance.

Comment 12: l 346. I disagree that a percentage facilitates interpretation. Could be omitted.

Response: All material related to this comment has been removed following the deletion of Section 3.1 (Variances) in the revised manuscript.

Comment 13: Fig. 4. I'm not sure this figure is necessary. It could be omitted to reduce the length of an already too long paper.

Response: Fig. 4 has been removed together with Section 3.1 (Variances), which has been deleted in the revised manuscript to reduce length and improve focus.

Comment 14: Tab. 2, Fig. 5. It is very difficult to compare experiments that have been performed in different climates, different instruments, and different beam geometry.

Response: The discussion and results associated with this comment have been removed following the deletion of Section 3.1 (Variances). The revised manuscript now avoids such cross-experimental comparisons.

Comment 15: Fig. 6. Excellent, but I insist that you plot premultiplied spectra $f \cdot S(f)$ when you have a logarithmic frequency axis. Explain how the spectra are averaged.

Response: Fig. 6 (now Fig. 5) has been revised to show premultiplied spectra $f \cdot S(f)$. In addition, the description of the spectral averaging procedure has been clarified and is now explicitly stated in the figure title. p.14, Fig. 5.

Comment 16: l 358. It is surprising that the velocity spectra derived with the variance method do not match at low frequencies, while the traditional indeed do match. Can this be explained? In Sathe and Mann (2012) you see good match at low frequencies for the traditional method for the u- and w-components, but not for the v-component. It is also a bit strange that the overestimation of the traditional along-wind spectra differ at high frequencies with stability. Can that be explained?

Response: Thank you for the comment. The different low-frequency behaviour arises from the fundamentally different nature of the two retrieval methods. The variance method is based on LOS velocity variance within the lidar probe volume and is therefore less sensitive to large-scale, spatially coherent motions, which mainly affect the mean LOS velocity and contribute only weakly to its variance. This leads to a systematic underestimation of low-frequency spectral energy when compared with point measurements from the sonic anemometer. In contrast, the traditional method reconstructs instantaneous velocity components and therefore retains sensitivity to large-scale motions, resulting in good low-frequency agreement with the sonic, consistent with the findings of Sathe and Mann (2012). The reduced performance for the v-component reported in that study is attributed to limitations imposed by the scanning geometry. The high-frequency overestimation observed for the traditional method is caused by the wavenumber-dependent response associated with beam separation (Kelberlau et al., 2020). This effect is strongest under neutral and unstable conditions, when the inertial subrange is well developed, and is weaker under stable stratification where small-scale turbulence is suppressed. These explanations have now been clarified in the revised manuscript. p.18-19, l.365-381.

Comment 17: Fig. 7. Not because I want it in the paper, but you do similar analysis of TI_w?

Response: No, a similar analysis of TI_w was not performed, either within this manuscript or as separate side work.

Comment 18: Fig. 8. Please do not show |MRBE|. A bias should be shown with its sign.

Response: Fig. 8 has been revised to remove |MRBE|. The bias is now shown with its sign.

Comment 19: Discussion. A very good discussion in general!

Response: Thank you for this positive comment !

Comment 20: l 410. In Sathe et al. (2015) it is stated that the WindScanner system can have either 400 or 200 ns pulses. Although not entirely clear from the paper, the 200 ns pulse was used. That corresponds to a FWHM probe volume of approximately 30 m, not 100 m, as stated in the text.

Response: The description of the WindScanner pulse duration and the associated probe length has been removed from the revised manuscript, as these details are no longer required for the present analysis.

Comment 21: l 417. Relating to previous comments, the sample rate in it self should not bias the variance. Only the probe volume and the accumulation time should have an impact.

Response: Thank you for this clarification. The passage referring to a potential influence of the sampling rate has been removed from the revised manuscript. The discussion now makes clear that the intra-beam effect is governed by probe-volume averaging, specifically the accumulation time and the probe length, rather than by the sampling frequency itself.

Comment 22: Eq. 36. It is unnecessary to include the σ_r^2 term in the equation. It is completely negligible for a lidar, and only confuses the reader.

Response: Thank you for this comment. Eq. 36 has been removed from the revised manuscript, as it is no longer required for the presentation of the method. Consequently, the σ_r^2 term is no longer included.

Comment 23: l 422. σ_r and σ_l are not weighting factors, but length scales.

Response: The equation has been removed. See previous comment.

Comment 24: l 436. Again, it should be the averaging, not the sample rate that is important.

Response: We have revised the text to emphasize that the relevant controlling parameter is the accumulation (averaging) time at each LOS position, rather than the nominal sampling rate. References to sampling rate have been reformulated or removed where appropriate, and the discussion now consistently focuses on the role of temporal averaging in filtering turbulent fluctuations. p.17, l.339-346.

Comment 25: $\sim$ 411. It think it is worth mentioning Manami et al. (2025) in the discussion. In this paper we try to annihilate the probe volume effect of a pulsed lidar.

Response: Thank you for this very relevant suggestion. We have now explicitly included a discussion of Manami et al. (2025) in the manuscript. Your work is cited in the context of probe-time and probe-volume averaging effects in pulsed Doppler lidars. We clarify that, while the present study investigates how turbulence retrieval is affected by accumulation time, probe length, and scanning configuration, Manami et al. (2025) propose a complementary signal-level approach that aims to mitigate (or "annihilate") probe-volume filtering by exploiting Doppler spectral information. This addition places our results in the broader context of recent efforts to recover turbulence statistics from pulsed lidars and highlights the complementarity between configuration-based and signal-processing-based strategies. p.17, l.347-354.

Comment 26: l 443-452. Again, if the ZXLidar is taking 50 ms acculation time, then that, together with the probe volume, is what is important. Spending only a fraction of the time at one height does not introduce a bias in the variance. That is actually also discussed in Lenschow et al. (1994).

Response: Thank you for the clarification. We acknowledge that the accumulation (averaging) time and probe volume are the parameters controlling variance estimates, and that spending only a fraction of the time at a given height does not, by itself, introduce a bias in the variance, as discussed by Lenschow et al. (1994). The corresponding discussion has therefore been removed from the manuscript.

Comment 27: l 453-457. I don't understand this discussion. Under stable conditions, the standard knowledge is that the length scale is smaller for stable conditions.

Response: Thank you for pointing this out ! We acknowledge that the original wording was incorrect. Under stable stratification, turbulence is indeed characterized by smaller length scales, whereas unstable conditions are associated with larger energetic eddies. We have corrected this mistake in the revised manuscript and reformulated the discussion accordingly to ensure consistency with standard boundary-layer turbulence theory. p.18, l.355-364.

Comment 28: l 465. How can systematic (that is stationary) spatial gradients introduce more variance in the traditional method? I would think that it only introduces a bias in the mean.

Response: We thank the reviewer for this important clarification. We agree that a purely stationary spatial gradient does not, by itself, introduce variance but only a bias in the mean. The additional variance arises because scanning lidars sample different spatial locations sequentially rather than simultaneously. In the presence of spatial gradients, sequential sampling combined with advection causes spatial variability to be mapped onto temporal fluctuations when the LOS measurements are combined, leading to apparent variance. We have clarified this point in the revised manuscript to avoid ambiguity. p.18, l.384-386.

Comment 29: l 473-476. Interesting that the error is significantly larger for the cross-wind component, but can you explain why? I cannot really follow the logic in the explanation. Maybe there is a hint in **?**

Response: We have revised the discussion to clarify that the larger errors in the cross-wind component arise from the sequential nature of scanning lidar measurements, as discussed by Sathe and Mann (2012). Because LOS measurements are acquired at different times, the reconstruction of the cross-wind component relies on combining measurements that decorrelate more rapidly in time and space than those contributing to the along-wind component. Along-wind fluctuations are advected by the mean flow and therefore remain correlated over the scan cycle, whereas cross-wind fluctuations decorrelate more quickly. This leads to a reduced or distorted estimate of cross-wind variance, even when the mean wind direction is accurately known. This explanation has now been made explicit in the revised manuscript. p.19, l.394-405.

Comment 30: l 507. "identified the problematic". I guess you mean "problem", or "research issue".

Response: The term "problematic" has been replaced by "problem" in the revised manuscript. p.20, l.429.

Sincerely,
Maxime Thiébaut

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