Response to Reviewer 2

Manuscript Title: *Dual-lidar profilers for measuring atmospheric turbulence*
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Dear Reviewer,

Thank you for the careful evaluation of our manuscript and for the constructive comments. The manuscript has been revised accordingly. Below we provide a detailed, point-by-point response to all comments. Your comments are shown in black, and our responses are shown in blue. All modifications to the manuscript are highlighted or tracked, in accordance with the journal's guidelines.

Reviewer 2

Thiébaut et al. present an interesting study on turbulence measurements with profiling lidar. The idea to place two lidars with a yaw angle offset and combining them to a lidar with more beams is interesting and innovative. The plots are well prepared and the manuscript is well written. At mutiple points I feel that the description of the results and the methods are a bit unprecise. The comparison to other lidar configurations and techniques on the contrary goes a bit too far in my opinion, because it cannot be justified with the results of the experiment. I thus suggest the manuscript for publication only after major revision. General and specific comments are given below.

General comments

- The database could be better described. A brief statement on wind speed span, median and mean is given, but for example no information about the distribution of wind direction. Is that dataset statistically significant? I think it is, but it is not shown.

- I am not very convinced about the comparison with the 6-beam method. You cannot easily compare the datasets and the lidar parameters are quite different. The comparison of errors and uncertainties on that basis is not sound.

- Some details of the results are not explained in enough detail. For example, the effects for cross-wind variance and how they depend on wind direction, or the differences in the spectra. Why is the low frequency not the same for all methods, why does the variance method have more high frequency noise etc.

Response: We thank the reviewer for these constructive general comments and address them as follows.

(i) Description and statistical representativeness of the database:

We agree that the original manuscript did not sufficiently document the characteristics of the dataset. The revised manuscript now provides additional information on the dataset size, wind speed range, and the distribution across atmospheric stability classes. While wind direction statistics are not explicitly shown, the dataset comprises 1,098 independent 30-min periods spanning a wide range of meteorological conditions, which we consider statistically significant for the purposes of this study. This point has been clarified in the revised text. Sect. 2.1 (p.4) and Fig. 2 (p.5).

(ii) Comparison with the six-beam method:
We agree that a quantitative comparison between the variance method and the six-beam method is not robust given the differences in datasets, scanning strategies, and lidar parameters. This concern was also raised by the first reviewer. Consequently, the comparison of errors and uncertainties with the six-beam method (including Table 2 and Fig. 5) and the associated discussion have been removed from the revised manuscript.

(iii) Interpretation of specific result features:
We acknowledge that some interpretations—particularly regarding cross-wind variance behavior, wind-direction sensitivity, and spectral differences—were not sufficiently supported by the results presented. As these dependencies were not explicitly demonstrated and could not be robustly quantified within the scope of this study, the corresponding interpretations have been removed or substantially revised. The revised manuscript now focuses on results that are directly supported by the data and on mechanisms that can be clearly attributed to probe-time and spatial averaging effects. Regarding the spectra, the apparent differences between the methods do not indicate physical discrepancies or increased noise. In particular, the white-noise plateau is not visible in the traditional method because the high-frequency portion of the spectra is contaminated by inter-beam effects, which distort the spectral shape and mask the underlying noise behavior. This clarification has now been added to the manuscript. p.13, l.290-292.

Specific comments

Comment 1: p.1, l.21. I do not think that you can say that so generally. There are a lot of people who do VAD with pulsed lidars as well. It has advantages, especially for turbulence retrievals, too.

Response: Thank you for the clarification. We agree that the distinction between pulsed and continuous-wave lidars in terms of DBS and VAD operation is not exclusive, and that VAD scanning strategies are also commonly applied to pulsed lidar systems, particularly for turbulence retrievals. We have therefore revised the text to avoid this overly general statement and now emphasize that DBS and VAD represent commonly used, but not exclusive, measurement strategies for pulsed and continuous-wave lidars, respectively. p.1, l.21-24.

Comment 2: p.2, l.48. Eberhard et al. (1989) requires a full VAD at 35.3° and provides TKE and the covariances, not the single component variances. Later studies by Smalikho, Stephan, Wildmann and Päschke showed that this method is very accurate, if the lidar "intra-beam" volume averaging effects are corrected in the retrieval.

Response: Thank you for this important clarification. We agree that Eberhard et al. (1989) is based on a full VAD scan at 35.3° elevation and retrieves turbulence kinetic energy and velocity covariances, rather than individual component variances. Our original wording was therefore imprecise. We have revised the text to correctly describe the scope of the Eberhard et al. (1989) approach and to distinguish it from the variance method applied here. We also now acknowledge subsequent studies demonstrating the high accuracy of VAD-based turbulence retrievals when intra-beam averaging effects are properly accounted for. The manuscript has been corrected accordingly. p.2, l.42-46.

Comment 3: p.3, l.63. There has recently been a release of a commercial 6-beam lidar https://halo-photonics.com/lidar-systems/beam-6x/, https://halo-photonics.com/lidar-systems/beam-6x-windpower/. It also does not take 15s for an instantaneous measurement any more. I saw further down the manuscript that you discuss this instrument, but i think it would be fair to mention here already.

Response: We have revised the manuscript to explicitly acknowledge the recent commercialization of six-beam lidar profilers inspired by the WindScanner concept, in particular the Beam6X WindPower developed by Lumibird. This is now mentioned at the point where the dual-lidar methodology is introduced, and we clarify that the dual-WindCube configuration used in this study provides the minimum number of independent beams required by the variance method while relying on well-established lidar profilers that are already widely used and trusted in industrial applications. This addition places our approach in the context of emerging six-beam technologies while motivating the experimental choices made in the present study. p.2, l.54-56.

Comment 4: p.4, l.112. Despite the fact of the foundation being quite impressive, I am not sure how relevant it is for this study. Information about the wind conditions at the site (wind rose, etc.) could be quite interesting instead.

Response: We agree that the detailed description of the mast foundation and wind farm infrastructure was not directly relevant to the objectives of this study. This information has therefore been removed from the manuscript. In response to the second part of the comment, we have added a characterization of the mean wind conditions at the site, including a wind rose and a wind speed distribution for the analysis period, together with a concise description of the dominant wind directions and wind speed statistics. This revision provides more relevant contextual information for the interpretation of the lidar measurements. p.4, l.99-107 and Fig. 2.

Comment 5: p.7, l.159. Also for the collected dataset, some statistics would be helpful here: wind rose, histograms, of wind, turbulence, stability for example.

Response: We have added a characterization of the mean wind conditions for the collected dataset, including a wind rose and a wind speed histogram for the analysis period. These statistics are now presented in a new figure and accompanying text describing the dominant wind directions and wind speed distribution. Turbulence and stability statistics are treated separately in later sections of the manuscript, where they are directly relevant to the evaluation of the turbulence retrieval methods. p.4, l.103-107, Fig. 2.

Comment 6: p.8, l.185. Please provide the thresholds for the despiking.

Response: We thank the reviewer for this comment. The manuscript has been revised to explicitly state the despiking thresholds. Following Wang et al. (2015), spikes are identified within consecutive 30-min windows when the absolute differences between adjacent velocity samples exceed twice the interquartile range (2×IQR) and exhibit opposite signs. This clarification has been added to the text. p.8, l.162-167.

Comment 7: p.10, Eq. 13-14. U remains the absolute velocity?

Response: Yes, U denotes the wind speed magnitude, defined as the modulus of the horizontal wind velocity vector. p.9, l.178.

Comment 8: p.11, Eq.19. You switch here from vector notation to Einstein notation (I think), without explaining it. That could be confusing for readers and should be explained.

Response: Thank you for pointing this out. The original formulation could indeed be interpreted as relying on implicit index summation. We have revised the manuscript to remove this ambiguity by explicitly defining the construction of the matrix $\mathbf{Q}$ from the transformation matrix $\mathbf{T}$ and by clarifying that no implicit summation over repeated indices is assumed. In addition, we have added a numbered equation that explicitly writes the relationship between the LOS variances and the Reynolds stress components in index form. These changes clarify the notation and ensure consistency between the matrix and index formulations. p.10, Eq. 7.

Comment 9: p.13, l.310. I assume that the "virtual kinematic heat flux" was calculated using the sonic vertical velocity and sonic temperature? Thus not directly the virtual temperature. Could be confusing if you use the same symbol as for the average virtual temperature from the WXT530.

Response: Thank you for highlighting this ambiguity. The kinematic heat flux used in the computation of the Monin–Obukhov length is derived from high-frequency sonic anemometer measurements and is therefore based on sonic temperature fluctuations rather than virtual temperature fluctuations. To reflect this more accurately and avoid confusion with the mean virtual potential temperature derived from the WXT530, we have revised Eq. 36, which is now Eq. 19, and the associated text to use the covariance $\sigma_{w\theta_s}$. The mean virtual potential temperature θ_v is retained in the numerator and is computed independently from WXT530 temperature and humidity measurements. p.12, l.262-264 and Eq. 19.

Comment 10: p.15, Tab.2 and p.16, Fig.5. Comparing the methods with completely different datasets is not sound. You would have to make sure that you have the same amount of data for all sorts of wind bins, wind sectors, stability classes, which I assume is not the case here!?

Response: We agree that comparing the methods using datasets with different sampling distributions across wind speed bins, wind sectors, and stability classes is not statistically sound. This issue was also raised by the first reviewer. In response, we have removed the comparison presented in Table 2 and Fig. 5, along with the associated discussion, from the revised manuscript.

Comment 11: p.16, l.355f. Can you explain why the spectra differ at low frequencies?

Response: This point was already addressed in response to the first reviewer. The differences observed at low frequencies are attributable to differences in sampling strategy and effective averaging between the measurement approaches, which affect the representation of large-scale, low-frequency motions. We have clarified this explanation in the manuscript to make the origin of the low-frequency discrepancies more explicit. p.18, l.365-373.

Comment 12: p.22, ll.434ff. I think the ideas and comparison to other lidar configurations are a bit superficial and not exactly based on results from this study. I recommend to skip them and focus more on the direct findings of the new variance method.

Response: We agree with the reviewer that the previous discussion of specific lidar configurations was speculative and not directly supported by the results of this study. The section has been revised to remove device-specific comparisons and now focuses on probe-time averaging effects and accumulation time, which are directly supported by the findings and discussed in the context of Thiébaut et al. (2025) and Manami et al. (2025), as requested by the first reviewer. p.17, l.347-354.

Comment 13: p.22, l.457. Intermittent turbulence is especially observed in stable boundary layers with strong shear. A violation of homogeneity assumptions cannot be directly associated with neutral and unstable conditions alone. Neutral conditions can be perfectly homogeneous over flat terrain, stable conditions can be non-homogeneous with only slightly complex terrain. I think you should be a bit more precise what you mean here.

Response: We have revised the text to avoid associating intermittency or violations of homogeneity exclusively with neutral and unstable conditions. The discussion now clarifies that such features can also occur in stable boundary layers, particularly under strong shear, while emphasizing that higher turbulence levels and a broader range of energetic scales typically observed under neutral and unstable stratification tend to amplify inter-beam effects in the traditional reconstruction. p.18, l.360-364.

Comment 14: p.23, l.477. I think this dependency on wind direction should be shown explicitly.

Response: We agree that the dependence of the cross-wind turbulence intensity on wind-direction uncertainty should be demonstrated explicitly in order to support the interpretation. As this dependency is not directly quantified or shown in the present study, and the explanation was also not convincing to the first reviewer, we have removed this paragraph and the associated interpretation from the revised manuscript.

Comment 15: p.24, l.497. You should at least mention that a two-lidar setup doubles the cost at this point.

Response: We have added a sentence in the conclusion acknowledging that the dual-lidar configuration entails increased instrumentation costs due to the use of two lidar systems. p.19, l.413-414.

Sincerely,
Maxime Thiébaut

References

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