Review: Rouholahnejad et al.: Beyond the Intersection Point: OffshoreWind and Turbulence Reconstruction from Horizontally Separated Dual-Doppler Scanning Lidar Beams

Summary

The manuscript presents results from offshore DSL measurements and addresses several questions that are of high relevance for practical applications but also research-oriented measurement setups. First, the DSL measurements of mean wind speed and TI at long ranges are investigated. Second, the influence of spatially separated range gates for wind vector reconstruction on mean wind speed and TI is evaluated and the possibilities to measure spatial variability in the wind field with this approach are explored. The results are presented in a way that is oriented towards wind energy applications, which supports the impact that these results will have on advances in wind energy development. I therefore think the results and the content are well suited for publication.

However, I think that, in its current form, the paper has several points in the methodology, discussion, and interpretation of the results that need to be addressed before the paper can be published.

General comments

1. The authors use the term uncertainty as equivalent to the deviation from a reference. This is an oversimplification of the standards by which uncertainty of measurements is usually defined in wind energy and may be misleading. This point is particularly important because the wind conditions between the two measurement periods differ significantly. I would suggest using "deviation from the reference measurement" instead and explaining the connections of the presented statistics to uncertainty.

2. The analysis puts a lot of emphasis on spatial inhomogeneity in the wind field as the main reason for the observed deviations when spatially separated range gates are used for turbulence reconstruction. However, the presented analysis does not fully support attributing the deviations primarily to this mechanism. Besides spatial heterogeneity, scan geome-

try and the spatial structure of (homogeneous) turbulence, as well as their interaction, will influence the errors in TI measurements. While the authors acknowledge this influence to some degree in Section 4.3.2, they do not perform a systematic quantitative analysis. Effects of coherence/decorrelation effects due to spatial separation and their interaction with the scan geometry relative to the wind direction are not sufficiently analysed to draw the presented conclusion. I would suggest rephrasing the relevant sections in the manuscript, especially in the abstract, Sections 4.3–4.4, and conclusions, or adding a systematic quantitative analysis of coherence/decorrelation effects due to spatial separation.

3. A more structured explanation of the different aspects influencing the errors in non-collocated measurements could improve the discussion and interpretation of the results. Turbulent length scales or correlation length scales, and thus spatial decorrelation, will depend on the atmospheric conditions, stability, etc. They are likely to change with direction, i.e. an offshore fetch might be influenced by low-frequency variations Syed and Mann (2024). In addition, turbulence in the atmospheric boundary layer is anisotropic. Coherence and turbulent scales change with wind component and direction relative to the mean flow; streamwise and transverse velocity fluctuations generally have different correlation lengths scales, which also differ by direction (along- vs. cross-wind). This is also not avoided by choosing a fixed coordinate system, as the authors claim. Please provide a clearer, structured discussion of these limitations. Even if not used for modelling, the model suggested by Mann (1994) could provide useful points on typical spatial turbulence structure for the discussion.

4. The LES is used to justify the conclusion that spatial heterogeneity is the main driver of the observed deviations for non-collocated measurements. However, with respect to the explanation of the deviantions, the analysis remains qualitative by comparing observed versus modelled heterogeneity. Modelling a virtual lidar in the LES would greatly improve the robustness of the results and also allow a more quantitative separation of the different factors influencing non-collocated turbulence measurements. Of course with the limitation that the LES only covers a very limited envelope of atmospheric conditions and wind directions.

5. The study uses wind-vector reconstruction from spatially separated measurements to investigate spatial heterogeneity. This approach offers interesting possibilities for exploring spatial heterogeneity. However, I believe that the conclusions regarding the use of spatially separated locations to measure the spatial variability of turbulence are too strong and are not sufficiently supported by the presented evidence. Several limitations and potential pitfalls of this method should be discussed more thoroughly in the manuscript.

    (a) There is a conceptual difficulty: flow information from two locations is combined while the analysis seeks to demonstrate differences between those locations. This blends the wind conditions at the two locations through a weighting function that depends on the beam angles. Consequently, the v-component will be more strongly influenced by the northern lidar, whereas the u-component will be dominated by the southern lidar. The effective representative location of each reconstructed component will therefore be shifted accordingly. For turbulence intensity (TI) and wind speed, the relative influence of each lidar will also depend on the wind direction relative to the beams. Therefore, assigning the measured quantity to a specific physical location is not straightforward and entails significant uncertainty. It would be helpful to add a discussion that explains and justifies the choice made in the manuscript and addresses its assumptions and limitations.

(b) Spatial decorrelation effects can introduce apparent variability, or pseudo-variability.

(c) Finally, the variability in the wind field inferred from the lidar observations is compared with simulations. This comparison indicates that the variability observed by the lidars may be plausible. However, it does not constitute a comparison with ground truth and therefore does not demonstrate that the observed variability arises from real variations in the wind field rather than from measurement artifacts.

Specific comments

- **L15:** "Comparison with LES demonstrated that these deviations reflect physically meaningful spatial variability rather than measurement artifacts." Please moderate this statement. The LES can provide indications, but the analysis presented in the manuscript does not "demonstrate" one or the other reason.

- **L18:** "Under sufficiently homogeneous offshore conditions, moderate beam separations can be tolerated, while the resulting deviations provide a practical measure of spatial uncertainty relevant for wind resource assessment and load analysis." This is a very broad conclusion which is, in this form, not sufficiently supported by the presented evidence. The study showed that under specific conditions and scan geometry the deviations were low. However, the evidence presented does not allow such a generalisation. The term uncertainty is not suitable in this context. Variability would be better. A simple example, for which this conclusion does not hold, would be that a beam perpendicular to the wind component of interest cannot provide spatial information about its variability. Please moderate this statement or relate it specifically to your case study.

- **Section 1:** The authors motivate TI measurements with resource assessments. However, site suitability assessment and load simulations are also important (if not more important) reasons to measure TI and should be added in the motivation.

- **L45–48:** Pauscher et al. (2026) could be a meaningful addition to the literature. They published a comparison of different probe volumes, including a range-gate length that was similar to the one used in this study, and gave similar results for (radial) TI as reported in this study. It would also be useful to use the references from the introduction again when discussing the observed collocated TI underestimation.

- **End of introduction:** The detailed listing of the content of the subsections is a little bit lengthy. I would suggest to shorten this section.

- **Section 2.1.1:** Please provide some additional details on the cup anemometer that was used as the reference.

- **Section 2.1.3:** The proportion of the lidar data in % would be very valuable (for periods when the lidars were technically available). Especially at larger distances, this can be a limiting factor for the application of the DSL technology. If possible, provide the retention rate after the main processing steps.

- **Table 1:** It would also be interesting if any information on the reasons for the limited availability was available, e.g. low visibility due to clouds or low backscatter due to a lack of aerosols.

- **Section 2.2:** At the beginning of the section, information on the recovery rate and CNR filtering is stated, which is later repeated. I would suggest removing this text.

- **Sections 2.2 and 2.2.1:** The authors mention a sensitivity analysis which was performed for their choice of filtering methods and parameterisation. However, no results from this sensitivity study are presented. Instead, the authors review choices of data-filtering methods of other studies. Finally, they proceed to define their own methods and thresholds without giving any evidence of their performance or advantages. I would suggest shortening the discussion of the other methods and, instead, presenting some quantitative evidence on the performance/robustness of their method and the choices for the chosen thresholds. As data preparation for scanning lidars has been identified as challenging by other studies, these results would be very valuable for future experiments.

- **Section 2.3:** I would recommend merging with Section 2.4.

- **Section 2.4:** At the edge of Sector A, there seems to already be a spike due to the wake of the wind turbine. Is there any reason for the choice of the sector boundary other than that the limit of Sector A aligns with the lidar beam? Would it not be better to define a wake-free Sector A and increase the size of Sector B?

- **L278:** The attribution to "increased flow heterogeneity induced by coastal roughness transitions and developing internal boundary-layer effects" appears speculative based on the information presented. Figure 2 shows that these directions are associated with elevated TI, but it does not directly demonstrate flow heterogeneity or the presence and development of an internal boundary layer. I recommend describing the upstream fetch more directly.

- **Figure 5:** Please explain what the thin and thick lines are. In general, the current plot is dominated by the tail ends of the LES_DD spectra. This makes the comparison between the measurements and simulations very hard. It is also hard to judge their similarity and their slope.

- **Section 2.6:** The terminology in this section should be revised. MBE and RMSE relative to a reference dataset quantify deviations from that reference (or reference-relative comparison metrics); they are not equivalent to a complete measurement-uncertainty assessment. This distinction is particularly important because the IEC standard and acceptance criteria are mentioned in the same section. I would recommend using the terms "deviation" and "threshold" here and explain the relation of the presented merics to the uncertainty.

- **Section 3.1:** It is very interesting that a higher TI was observed in AUDRA. However, the reasons for this remain unclear. Could measurement noise be the reason? An analysis of the power spectral density of the LOS velocities could shed some light on this and reveal noise contributions, especially at the high-frequency end.

- **Section 3.1.1:** Consider merging with the previous section.

- **Figure 10:** The main pattern that emerges from the plots is that the deviation is largest when the observed variances and covariances are largest. This is not surprising, as the error in σ_u and σ_v should scale with their magnitude, i.e. a higher absolute value in the observed variance should correspond to higher deviations. I would therefore suggest normalising the observed deviations with the average observed variances in the bin and seeing which patterns remain. In general, the discussion in the corresponding section seems to imply that higher deviations imply inhomogeneity or inhomogeneous turbulence structures. However, high turbulence is not equal to inhomogeneity.

- **Discussion and potentially Introduction:** For profiling lidars, the problem of cross-contamination due to beam separation in wind vector reconstruction was introduced by Sathe et al. (2011)), and more recently analysed by Kelberlau and Mann (2020). They found that cross-contamination between variance components was an important factor. This manuscript deals with a similar problem but a different lidar configuration. Please relate the present configuration explicitly to this literature and clarify whether, and why, cross-contamination is expected to be smaller or qualitatively different than in DBS lidar measurements.

- **L688:** "This suggests that the reconstruction behaves partly as a spatial smoothing operator for the mean flow while still retaining relevant turbulence variability relevant for offshore wind applications." This is based on only 67 concurrent 10-minute periods under very specific atmospheric conditions. Further, LES fields are compared directly with reconstructed lidar products without reproducing LOS sampling in the LES. The conclusion, in contrast, is very general. Please moderate this interpretation and describe the LES result as limited evidence of consistency with resolved spatial TI variability rather than evidence that the method robustly retains turbulence heterogeneity relevant for offshore applications.

- **L693:** "Overall, the results suggest that non-collocated DSL reconstruction can provide representative turbulence measurements and physically meaningful information on spatial flow variability over kilometer-scale domains." Again, this conclusion is very broad. Please be more specific about the limitations (scan geometry, conditions that were found during the experiment) and/or relate it more directly to your case study.

- **Figure B2:** An indication the 1 km max. distance (circle or ellypsis) in Figure B2 would likely be helpful to better understand the spatial separation.

References

Kelberlau, F. and Mann, J. (2020). Cross-contamination effect on turbulence spectra from doppler beam swinging wind lidar. *Wind Energy Science*, 5(2):519–541.

Mann, J. (1994). The spatial structure of neutral atmospheric surface-layer turbulence. *Journal of Fluid Mechanics*, 273:141–168.

Pauscher, L., Popkova, V., Gramitzky, K., Hildebrand, T., Callies, D., and Jäger, F. (2026). On the attenuation of turbulence measurements from scanning lidars. *Journal of Physics: Conference Series*, 3224(2):022034.

Sathe, A., Mann, J., Gottschall, J., and Courtney, M. S. (2011). Can wind lidars measure turbulence? *Journal of Atmospheric and Oceanic Technology*, 28(7):853 – 868.

Syed, A. H. and Mann, J. (2024). A model for low-frequency, anisotropic wind fluctuations and coherences in the marine atmosphere: Ah syed, j. mann. *Boundary-Layer Meteorology*, 190(1):1.