Review of "Beyond the Intersection Point: Offshore Wind and Turbulence Reconstruction from Horizontally Separated Dual-Doppler Scanning LiDAR Beams" by Farkhondeh (Hanie) Rouholahnejad, Tanguy Lunel, and Julia Gottschall

General comments

The authors investigated whether the requirement for intersecting range gates can be relaxed in an offshore environment and whether non-collocated dual-Doppler reconstructions can provide useful information on spatial wind-field variability. The study is based on a four-month offshore measurement campaign and large-eddy simulations (LES). First, using a conventional dual-Doppler scanning Lidar (DSL) configuration, the authors demonstrated that the system provides sufficient accuracy in measurements of 10-min mean wind speed and turbulence intensity (TI) at distances of 5–7 km, based on a comparison with measurements from an offshore meteorological mast. They then showed that non-collocated reconstructions with separation distances of up to 500 m produce uncertainties comparable to those of collocated reconstructions.

The study is well conducted, and the methods employed are generally appropriate. It represents a valuable attempt to extend the applicability of DSL observations by utilizing measurements from non-intersecting beams, which are typically discarded. The findings will be of interest to researchers in Lidar measurements and to engineers involved in offshore wind-resource assessment.

The manuscript is generally well written and presents valuable results. However, several aspects require further clarification and discussion before the manuscript is suitable for publication. My specific comments are provided below.

Major comments

1. The non-collocated approach appears to be essentially the same as the Doppler beam swinging (DBS) method commonly applied to pulsed vertical-profiling Lidars and nacelle-mounted Lidars, except that the measurements from the two lidars are time-synchronized. As the authors repeatedly discuss, the DBS method assumes spatial homogeneity—that is, the wind field is assumed to be homogeneous throughout the measurement volume. Although the results are presented as observations at spatially separated locations, it is unclear which specific point is regarded as the measurement location. For example, is the measurement assigned to one of the two central points, or to some other representative location? Please clarify how the measurement location is defined.
2. Because the horizontal wind speed is reconstructed from two line-of-sight velocities, one would expect the contribution of one lidar to become dominant when the wind direction is aligned with the azimuth angle of that lidar. In the present experiment, the azimuth angle of the southern Lidar is 69.3°. Therefore, when the wind direction is approximately 69° or 249°, along or opposite to this azimuthal direction, the influence of the measurements from the other lidar may become relatively small. For example, Figure. 9(a) presents the differences from the baseline as a function of wind direction; however, the RMSE in TI does not appear to decrease near wind directions of 69° and 249° in Figure 10. Could this indicate a potential issue with the algorithm used to derive TI from the line-of-sight velocities? The authors are encouraged to clarify the directional dependence of the reconstruction and verify the validity of the TI calculation algorithm.

3. The attempt to utilize measurements acquired outside the intersection point of the two scanning-lidar beams is valuable. In practice, even at the scan rate of 1/12 Hz adopted in this study, multiple intersection points could be established by varying the azimuth and elevation angles of the other lidar. It would be helpful if the authors could explain the intended applications of the proposed method and provide a specific example of its practical implementation. This would make the usefulness and advantages of the method easier to understand.
4. Validating the accuracy of scanning-lidar measurements at this range is highly valuable. In addition to the validation results for the 10-min mean wind speed and TI, information on data availability would also be useful to potential users. Although detailed results concerning data availability do not appear to be provided, the authors are encouraged to include and discuss the data acquisition rate or data availability as a function of measurement distance and, if relevant, atmospheric conditions.
5. The Conclusions state that the results agree well with the LES results. However, it was unclear which specific results support this conclusion. Please identify the relevant figures or quantitative comparisons and clarify the basis for stating that good agreement was obtained.

Minor comments

1. **Lines 80–91:** The description of the paper's organization would be easier to read if it were made more concise, without providing a detailed section-by-section overview.
2. **Lines 109–110:** Please provide additional details on the meteorological mast measurements, including whether data affected by tower shadow were excluded. Alternatively, an appropriate reference describing the measurements and data-processing procedure could be added.
3. **Line 123:** It is unclear what quality-control flags 7 and 8 represent. Please define these flags.
4. **Line 135, Table 1:** It would be helpful to include the lidar system version (v2.0 or v2.1) in Table 1.
5. **Lines 174–176:** It remains unclear whether the manufacturer-provided status flags were ultimately used in the quality-control procedure. Please clarify this point.
6. **Lines 179–180:** Was a 10-minute time window used for the rolling (3σ) filter? Please clarify the window length.
7. **Lines 181–182:** These sentences appear to duplicate the description in Lines 156–157.
8. **Line 185:** The functions "sin" and "cos" are italicized. They should normally be set in upright Roman type. In addition, the use of parentheses with these functions is inconsistent throughout the manuscript. Please standardize the notation.
9. **Line 215:** The introduction of the line-of-sight velocity variance for calculating TI seems somewhat abrupt. Consider moving the explanation in Lines 247–249 to the beginning of Section 2.2.4 so that the motivation and underlying concept are introduced before the equations.
10. **Line 222:** An overbar is used to denote the mean in some parts of the manuscript. Please ensure that the notation used for mean values is consistent throughout.
11. **Line 249:** The notation for variance and covariance is inconsistent. Please standardize it

throughout the manuscript.

12. **Line 260:** Equation (19) appears to differ from the corresponding equation in the paper by Archer et al. Please verify the equation and clarify or correct any differences.
13. **Line 267:** Symbols in Equation (20) such as θ_v are not defined. Please define all symbols when they first appear.
14. **Line 272, Figure 2(c):** If the values shown in Fig. 2(c) represent frequencies of occurrence, it may be clearer to display more rounded values.
15. **Lines 303–311:** The model configuration would be easier to understand if the key setup parameters were summarized in a table. In addition, only limited information is provided on the boundary conditions. Were ERA5 data used for both the initial and boundary conditions? Please provide further details.
16. **Line 338, Fig. 5:** The Discussion states that the LES underestimates TI because subgrid-scale fluctuations are not included in the time series. However, the spectrum in Fig. 5 appears to show that the LES resolves higher-frequency components than the dual-scanning-lidar measurements. Please clarify whether these two observations are consistent and explain how the unresolved subgrid-scale fluctuations affect the calculated TI.
17. **Line 375:** Because a probe length of 100 m was used, relatively small-scale fluctuations may have been substantially smoothed. Although the acceptance criteria are satisfied in Fig. 6(e), could this result be specific to the low-turbulence offshore conditions considered here? Please discuss the potential influence of probe-volume averaging and the applicability of the result under conditions with higher turbulence intensity.
18. **Line 466, Table 3:** It is difficult to compare the individual numerical values in Table 3. Consider presenting the results graphically, for example as a bar chart, to make the differences easier to interpret.
19. **Lines 492–496:** The clear difference between the two system versions is somewhat surprising. Could this indicate that the line-of-sight velocity measurements from v2.0 contain more noise, whereas the newer system applies additional internal smoothing? It would be helpful if the authors could discuss possible reasons for the observed difference between the versions.
20. It would be helpful to include a nomenclature or list of variables and symbols used in the manuscript.