Review of wes-2026-39 titled “An experimental campaign to measure turbulence in the marine boundary layer” by J. Mann, A. Patel, M. Sjoholm, G. R. Thorsen, E. I. Simon, L.-Y. Hung, and J. Gottschall

Overview: This study describes the deployment of five scanning lidars at two sites along the western Danish coast to reconstruct horizontal wind components at six offshore locations using a dual-Doppler approach. The authors provide a thorough description of the scan strategy and the experimental procedures adopted to ensure accurate beam alignment and reliable subsequent velocity statistics. While these elements are essential for a successful observational campaign and represent a valuable step-by-step methodology, a scientific publication should not rely primarily on technical description and pre-campaign validation against in situ instruments, particularly for an established technique such as dual Doppler. In my view, the principal scientific value of this work would be the one- and two-point spectral statistics obtained at unprecedented heights. These quantities are presented in the Abstract and Introduction as a central objective, but the Results section does not currently substantiate this claim. Instead, the results are largely limited to the pre-campaign phase and cross-validation outcomes. For example, the manuscript repeatedly cites lateral coherence as a key outcome of the campaign, yet no corresponding results are shown. Likewise, substantial attention is given to the quality-control process intended to ensure high-accuracy turbulence spectra, but no results are presented that demonstrate its impact on the spectral estimates.

Before this manuscript can be recommended for acceptance, I encourage the authors to expand the Results section and demonstrate that the scan technique and analysis workflow enable scientific findings beyond current limitations, with particular emphasis on spectral analyses of the retrieved velocity components. Additional comments are provided in the remainder of this review.

Technical comments

Line 22: A assumption of the Kaimal model which limits its vertical range of applicability, besides those already mentioned, is the use of the vertical coordinate as length scale. It may be worth mentioning this aspect here.

Table 1. In the third row, please correct “starring” with “staring”.

Line 85: Were Streamline XR Lidars used in stare mode or was a User-type scan defined for it?

Table 3: Please add the site for each Lidar system reported in Table 3 or add each Lidar label to Figure 3. This could help increasing the clarity of the experimental setup when different Lidar systems are mentioned through the rest of the manuscript.

Line 93-94: These lines are a bit unclear. Were the authors focusing on calibrating pitch and roll angle of each Lidar unit, the height or anything else? Please provide further clarifications.

Lines 109-114: The discussion related to the pre-campaign calibration is a bit confusing in these lines. In particular, it is unclear whether the Streamline systems were compared against the met tower; in the text, it is stated that their measurement range did not reach the met mast, yet the caption of Figure 5 states that the considered LOS is close to the met tower.

Additionally, even though each Lidar system was synchronized, I expect to find inevitable temporal misalignments for time series recorded by different systems in dual-Doppler configuration. How did the authors deal with this problem?

Line 110: Based on the high agreement between the 10-minute average wind speed in Fig. 5, I assume that the authors adopted a quality control procedure to reject LOS records characterized by low CNR, or analogous flaws. More details on this aspect should be provided at this stage of the manuscript.

Line 111: Figure 4 depicts two Windcube systems installed by the Streamline Lidars, as previously described by the authors. In contrast to the Streamline units, did Windcube LOS reach the met-mast location with high accuracy? If so, did the authors run a direct assessment against the met-mast sensor?

Line 199: The data in Figure 11 show one point every 100 m or so. If the radial resolution is 1.5 m (as reported in Table 3), it is unclear why one point every 100 m is reported here. Please clarify.

Figure 10b: To my understanding, blue dots report the V2 time series collected at Storm, which are compared against the analogous measurements at Brise. What do red dots stand for in Figure 10b?

Figure 11: Please add *x*-labels to the bottom-right panel and label each panel.

Line 208: Please add the Table number.

Equations 9 and 10: Do these equations refer to time-averaged or instantaneous quantities?

Eq. 10: The arc-tangent function is bound between -90° and +90°, whereas the wind direction should be reported between 0 and 360deg. How did the authors resolve this ambiguity?