# Review of paper titled ”An experimental campaign to measure turbulence in the marine boundary layer” by Jakob Mann et al. (wes-2026-39)

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## 1 Overall paper summary

This manuscript for a data description article presents a comprehensive field measurement campaign involving several Doppler lidars, with the aim of validating turbulence models in the marine atmospheric boundary layer. First, the authors provide a layout overview of the measurements, followed by a description of comprehensive pre-campaign calibration and testing. These include pointing verifications using drones, hard target mapping, comparisons against met mast-based anemometry, and elevation offset determination using sea surface leveling. Post-verification of the line-of-sight (LOS) measurements was also presented by cross-comparison of the lidar measurements. The manuscript further documents the published dataset, including a quality control section intended to guide end users. Finally, the dataset is evaluated through comparisons of vertical wind profiles with mesoscale simulations from the WRF model.

Overall, the manuscript is well structured and presents a clear and thorough description of the dataset, including limitations of single lidars, which affect the final data availability. The analysis of radial wind speed offsets and pointing accuracy, including elevation angle offsets, is particularly comprehensive. However, several issues arise that do not fully align with the stated objectives of the manuscript, particularly regarding its intended data use by other scientists for lateral coherence analyses and validation against turbulence models. There are also multiple instances of missing/unclear information or uncertainty analyses that are crucial for the intended use of the data. In its current form, we therefore recommend that the manuscript be significantly revised before acceptance.

## General comments

- Lidar measurements are sensible to uncertainties in pointing. Associated uncertainties should be provided with this documentation, or at least the basis to derive them (uncertainties in elevation, azimuth, range, possibly radial wind speed). An associated uncertainty model that could be derived, (e.g.,YADDUM, https://zenodo.org/records/3551577) would be helpful here and reasonable to implement.
- The manuscript lacks essential information on probe volumes or pulse lengths, a critical parameter for turbulence measurements. Given their direct impact on spatial resolution and data accuracy, probe volume specifications must be included in the data description including a discussion of their impact on the measurements.
- Section 2.3 (pre-calibration and testing) is comprehensive but could be streamlined. Some details, such as operating system update issues and water ingress, appear excessive for the scope of the

manuscript. Since one lidar was ultimately replaced and not used in the analysis, the section could be condensed to focus on the instruments included in the final dataset, along with key aspects such as LOS calibration and regression results. Issues such as windows updates, laser errors and weatherproofing (reconsider the use of the word improper here) could be moved to the appendix or even omitted.

- The drone-based calibration approach is interesting and reflects current state-of-the-art, but its motivation and advantages are not sufficiently emphasized. It would be helpful to more clearly explain why drone-based pointing verification is necessary and how it improves upon traditional hard-target mapping described earlier. In addition, Tables 7 and 8 would benefit from more detailed explanation and interpretation within this section. Also, why is the elevation precision for Sterenn so much larger for the February verification?

- While the paper provides a thorough analysis of offset sources, the multiplicity of methods complicates the interpretation of the offsets for the angles and radial wind speeds. Table 9 reports final offset values, but does not address potential temporal drifts over the campaign. To improve clarity and reproducibility, a supplementary table should be included showing the evolution of all offsets across time, with corresponding instruments and measurement periods. If drift is observed, a time-dependent correction function (e.g., linear or polynomial fit) should be provided to enable accurate reconstruction of measurement positions from the raw dataset. If no evidence on the temporal development of the drift is feasible, all absolute wind speed values should be labeled with an uncertainty range of at least the maximum absolute difference of the drift. Furthermore, a technical reasoning of the drift should be provided to identify any systematic or random impact.

- The use of WRF simulation data as a validation reference for in-situ measurements raises some questions. Given the potentially high quality of the observational dataset, comparison with simulations, particularly under varying atmospheric stratification introduces additional bias and uncertainty. Unless the authors provide a stronger justification for this approach, the plots involving the vertical wind profile comparison can be removed. Instead, a lateral coherence analysis example would better align with the stated objectives and should be included as a demonstration of the applicability of the dataset.

## Specific comments

- Consider adding ”data description” to the title of the manuscript so that readers will be aware of this special manuscript type in the Wind Energy Science journal.

- Line 58: Is the information about the landowners’ attitude on wind and nearby availability of technicians appropriate for a scientific paper?

- Line 87: Please specify which quantities were calibrated (e.g., $V_{\mathrm{LOS}}$, elevation, azimuth, range) and report the corresponding uncertainties.

- Line 97: What do you mean by ”correct” stick positions for the lidar? We understand that you have the same elevation error for all azimuth angles, meaning the elevation error equals the elevation motor offset?

- We recommend including a Gantt chart (or similar visualization) summarizing the operational timeline of all lidars, including observation periods and any downtime or failures. This could be placed in an appendix, with detailed discussion of instrument issues also moved there to better focus the main text.

- Table 1: In general, the event column should be detailed more and the table could be arranged in a manner to have start and end dates for a specific events. Pre-calibration is mentioned the first time here, it should be mentioned in Section 2.1 (general description) before. Also, the row for RHI scans is left without any description, please elaborate. LOS calibrations is also not specified against what device. 21st of June - hard target mapping with drones or building?

- Table 2: It is unclear whether the WindScanner 200S systems (Brise and Sterenn) are proprietary DTU-manufactured lidars. This ambiguity carries over to Line 200, where the terms "Streamline scanners" and "Streamline units" are used interchangeably. Please revise for clarity and consistency in terminology.

- Line 111: First mention of the post-campaign calibration. This should be in Table 1 and mentioned in the initial description in the introduction also.

- Line 134: Please specify the resolution (horizontal and vertical) of the DEM, as well as uncertainties (if possible).

- Table 4: Also, why is (Brise-Sterenn) + (Sterenn-Cup) != (Brise-Cup)? Were the offsets (except Chinook-storm) derived for the same period and if not, indicate it in the table with a separate column.

- Figure 5: Why are there significantly more data points for the Streamline systems (1513 in panel (b) vs 980 in panel (a)), despite the technical problems? This contradicts the information stated in line 126.

- Line 155: Please specify, at which ranges the drones were hovering during the pointing verification. Please also derive uncertainties from Thorsen et al. and provide them here. A drone at 20 m range will have significantly higher uncertainty compared to a range of 1 km.

- Line 157: While the reported errors appear negligible for the present analysis, it would be helpful to comment on whether they remain negligible for the intended applications of the dataset.

- line 163: Please refer to the recently published journal publication by Gramitzky et al. (2026) https://wes.copernicus.org/articles/11/861/2026/, who provide a correction to the obtained lidar-sea surface range of half the probe volume length. They observed, that potentially the lidar-sea surface range is biased and is overestimated with the standard sigmoid function. They highlight that errors in the lidar-sea surface range have a major contribution to errors in the obtained elevation offset. Further, they also provide a correction for a given probe length of 75 m. Still, they mention that this range determination potentially has errors and an improvement is necessary. Please discuss how this would impact the sea surface leveling and hard target based elevation offsets.

- Table 9: Please clarify why no data is reported for Sterenn. Please specify in the text, why there are such big differences to the results obtained with the drone base pointing verification to the sea surface leveling based results. Further, please explain why the elevation offsets from the RHIs have been utilized to correct the elevation offset and not the drone based results. What is the reference, that is trusted? It would be interesting (but not necessary) to provide uncertainties obtained with the both methods.

- Table 11: The LOS velocity offsets for Sterenn and Brise show notable changes (e.g., Sterenn: $-0.3$ ms$^{-1}$ pre-campaign vs. cup, compared to $-0.7$ ms$^{-1}$ in the final values; Brise: $-0.2$ ms$^{-1}$ vs. cup, compared to $-0.4$ ms$^{-1}$ final). Please comment on the cause of these variations. Do these offsets evolve over the course of the campaign relative to the cup-based reference? How would a user downloading the data process the wind speeds for example in the middle of the campaign (since a fixed final offset is specified)? Additionally, for consistency with the text and other tables, values could be reported with two decimal places.

- Line 188: Please clarify this - the beams have been pointed at the hard targets and the radial wind speeds have been used at ranges, where there is no aerosol backscatter and the CNR is in the noise range due to the blocked beam? Or is the radial wind speed used just before the hard target? Or from the range, where the probe volume hits the hard target and the CNR is very high ($> 5$ dB)? If so, the wind speeds could be incorrectly derived, as the manufacturers typically use a maximum likelihood estimate, which is valid for aerosol backscatter but might not be valid for hard target backscatter. This is very unclear and must be specified in more detail.
- Line 202: The streamline systems were tested against a reference lidar, which, however, could also have significant offsets. Why do you assume, that the streamline systems do not provide radial wind speed offsets? Especially, because they have not been tested against the met mast in the pre-campaign verification? There has basically been no verification at all against a met mast based reference, right? Also, Zonda was not part of the pre-campaign calibration.
- Line 205: To the reader, it is not clear, where to apply the radial wind speed offsets, for which periods and for which reason. Whereas the pre-campaign results (from an accurate comparison against a met mast) indicate, that there are significantly smaller radial wind speed offsets, we would reconsider the assumption, that the streamline systems and Brise have no significant offsets.
- Line 217: Here the term signal to noise ratio (SNR) is used, but there are multiple instances throughout the paper where carrier to noise ratio (CNR) is also present. They are not the same, but are often mixed in literature. Please use only one terminology. Also in Line 236, the abbreviation CNR is elaborated, but this has already been used multiple times before (e.g.: "CNR mapper").
- Line 218: Is the term streamline intensity referring to a physical stream-line intensity of the lidar beam or rather the signal intensity, with a modified name to match the manufacturer (or is it the same)? Please clarify this.
- Line 261: For staring configurations at $5^\circ$ and $11.1^\circ$ elevation angles, the vertical wind component is assumed negligible. It would be useful to provide an estimate of the expected magnitude of this contribution to justify this assumption.
- Table 12: Consider adding a column describing atmospheric stratification, quantified using a metric such as the Monin–Obukhov length.
- Lines 290–291: Stratification is referenced without prior definition. Please clearly define how stratification is estimated (e.g., from WRF or measurements), how it is quantified, and how stability classes are determined.
- line 296: One could argue that $11.1^\circ$ is not close to horizontal, see comment before.
- The time synchronization is not referred to outside of Lines 105-107, but this refers to the pre-campaign validation. The authors should describe how the systems were synchronized and what the expected deviations are from the synchronization. This might be important especially for turbulence analysis.

# Technical comments

- Table 1: 1st of February, typo for staring
- Line 126: Period - have, check grammar
- Line 208: Missing table reference
- Line 280: are affected - split word

## Data accessibility

The data is cataloged properly and the .ipynb script is organized well for other users to process the dataset. This section in the script: "From Influx - cannot be accessed from outside DTU" - can be removed, or it has to be mentioned in the data availability section if a part of the script cannot be executed outside the DTU campus. It can also be removed if it is not essential to process raw data. There could also be a few lines to obtain a corrected radial wind speed along with offsets also for azimuths and elevation for a test case.