<u>Referee 1</u>

Review of wes-2026-3G titled "An experimental campaign to measure turbulence in the marine boundary layer" by J. Mann, A. Patel, M. Sjöholm,

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.

We thank the reviewer for their insightful comments which have helped improve the manuscript. We agree with their main comment that the principal scientific impact of this work lies in the turbulence spectra and coherences. However, the reason for not mentioning them in the Results section is because we have published another article (<u>Lidar observations of turbulence for tall offshore wind turbines | Journal of Fluid Mechanics | Cambridge Core</u>) that provides detailed analysis. Thus, we have expanded the Results section only with a brief example of the spectra and coherences to illustrate how the measurements can be used. We would also like to stress that this is a paper in the newly introduced data description paper category and not a research article. Thus, an analysis of the characteristics of turbulence would be out of context. Please find our replies to the reviewer's comments in green below.

Technical comments

Line 22: An 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.

A sentence has been added between line 20-22: "Moreover, the Kaimal model uses the vertical coordinate as length scale which also limits its vertical range of applicability."

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

Corrected

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

We simply used the stare mode. It is clear already in the text.

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.

The sites have been added to Table 3

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.

The clarity of the text was improved and references have been added describing the inclinometer calibration method in greater detail. The focus here was on finding the pitch and roll readouts of the inclinometer sensor where the lidar is in a precisely levelled position. Such that the elevation error as a function of azimuth position is minimized. The calibrated values are then used as a starting reference when installing the lidars in the subsequent field campaign.

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.

It was indeed confusing. Thank you for pointing this out.

In the pre-campaign phase, our primary objectives were (1) to ensure consistency between the lidar systems that would be used in the dual-Doppler setup, which in the main campaign does not rely on the met mast as a reference. (2) evaluate the desired scan configuration tailored for turbulence measurement would reach sufficient distance in the main campaign.

For the reconstruction of the horizontal wind field, the requirement is that the different lidar systems are consistent (i.e. no significant systematic deviation across systems), rather than that each system is calibrated against an absolute reference, such as a met mast.

There is a labelling error as the reviewer pointed out. At the location of met mast, we got only x sample points. Also, it is to be noted that the data shown in the met mast vs. Streamline system comparison was collected before the repair and heavy maintenance. Therefore, it is especially important to note that they are not sensible to be treated as calibration ground truth.

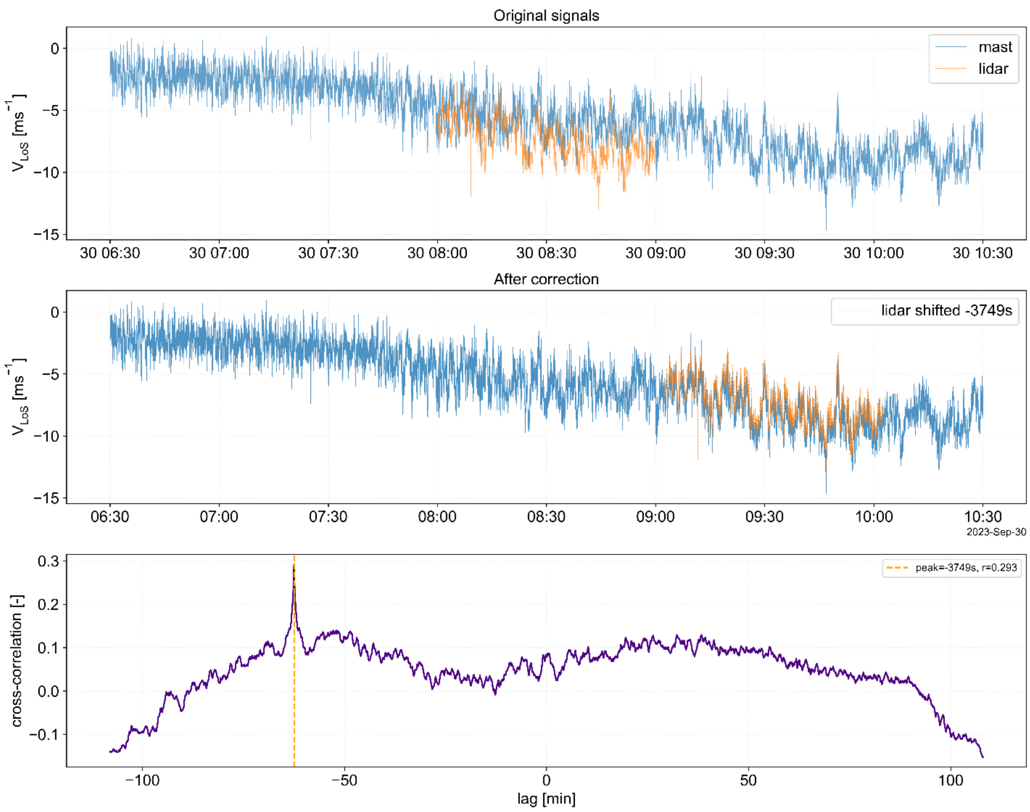
We have significantly adapted the text in Lines 109–114, Fig.5 and the caption of Fig. 5 to explicitly addressed the points mentioned.

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?

There were no dual-Doppler system setting during the pre-calibration campaign. Nevertheless, the time shift is always properly checked against the two systems performed regression analysis.

We appended a new paragraph in the manuscript to describe the time offsets found, especially during the first half measurement period of the WindScanner systems: average corrections of 3750 s for Brise and 3620 s for Sterenn against the metmast which was stamped in UTC+1.

An example using one second time series of lidar LOS and met mast projected LOS to determine the time shift of the system "Brise" is attached as a verification of the proposed offsets.



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.

Thank you for the comment. You are correct that we applied a filtering procedure to the LOS records prior to computing the 10-minute averages shown in Fig. 5. Specifically, we discarded all LoS measurements that did not meet the following criteria:

- Windscanner:
  - CNR > -22 dB
  - -40 m/s < LoS wind speed < 40 m/s
  - Additionally, removed records affected by obvious outliers associated with certain time period.
- Lumibird:
  - intensity > 1.009
  - -40 m/s < LoS wind speed < 40 m/s
  - Additionally, removed records affected by obvious outliers associated with certain time period.

Only LoS data that passed this quality control were included in the calculation of the 10-minute average wind speed. Apart from the LoS wind speed filter, we have also applied a

10-minute based data availability filter to prevent 10-minute bin with insufficient measurements to be used in the regression analysis. Only 10-minute data availability higher than 80% is considered in the regression analysis.

We add an additional paragraph in the manuscript to describe this procedure in more detail.

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?

Yes, for the two WindScanner systems the line-of-sight was configured such that one range gate intersected a met mast cup anemometer at around 1 km range.

We performed a direct comparison between the lidar LOS wind speed and the met-mast cup and wind vane projection, which is shown in Fig. 6. As seen in that figure, the number of data points is considerably smaller than in the lidar–lidar LOS comparison because, for met mast-based analysis, we only keep data when the wind direction is within ±40° of the LOS (or its 180° opposite direction) and we apply the same quality-control filters as described for Fig. 5.

In addition, not all systems consistently reached the met-mast distance concurrently, the authors used the met-mast as a complementary check and did not wish to interpret this specific analysis as a full calibration. We have clarified this in the revised manuscript.

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.

It is correct that the data was taken with more ranges than every 100 m. However,since we don't expect the result from the regression plot to change significantly, we reduce the result to only be every 100 m.

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?

It is the binned data which we fit to. This is now added in the text.

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

X-axis added.

Line 208: Please add the Table number. Table number added.

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

They are applicable to both time-averaged and instantaneous quantities but in this context, they refer to instantaneous ones.

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?

We use the two-argument arc tan function (often called arctan2) which is bound between -180° and +180°. Then a simple conversion from geometrical to meteorological convention provides the wind direction.

Referee 2

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

April 15, 2026

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 ver- ifications 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.

We thank the reviewer for their comments that have helped improve the readability of the manuscript. We have tried to address the comments especially with regards to the lack of/unclear information. Please find them below

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.

  In the section Drone-based verification, the pointing error at the point of intersection is estimated using the result from the drone-based verification.

- The manuscript lacks essential information on probe volumes or pulse lengths, a critical pa-rameter 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.

  Probe length and pulse length added in table 3 together with references.

- 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.

  We agree that there are many details that could be omitted, but the point is for this to be an extensive reporting on the planning and execution of the experiment. So, our take is that all of these details are relevant. We don't see much see published documentation of such issues with lidar hardware for example, as that sort of info doesn't make it into journal papers, and the manufacturers aren't forthcoming for obvious reasons.

- 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?

  The drone-based verification precision depends on multiple things, one being the distance from the drone to the scanner head. For windy conditions, as was the case in February for the church site, it is more difficult to steer the drone into the beam and therefore events where the beam is hitting the drone at large distances are hard to obtain. Another thing that decreases the pointing direction accuracy is the size of the drone. For the windy conditions, we set the leg configuration of the drone to be down, increasing the drone size and thereby increasing the chances of hitting the drone with the beam. However, this also decreased the accuracy of the elevation angle estimation. Max distance and drone leg configuration have been added to the tables and a description of why these matters for the precision are added to the manuscript.

- 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.

  From Patel et al. (2026), equation B7:

  $$\boldsymbol{R}_{\tilde{u}\tilde{u}}(x_1') = \boldsymbol{T}\boldsymbol{R}_{v_r v_r}(x_1')\boldsymbol{T}^{T}.$$

  If we add an offset of

δv_1

and

δv_2,

the correlation matrix for the radial velocities becomes:

$$R'_{v_{r_1}v_{r_1}}(\tau) = \langle (v_{r_1}(t) + \delta v_{r_1})(v_{r_1}(t+\tau) + \delta v_{r_1}) \rangle$$

$$R'_{v_{r_1}v_{r_1}}(\tau) = R_{v_{r_1}v_{r_1}} + 2\delta v_{r_1} \langle v_{r_1} \rangle + \delta v_{r_1}^2$$

And,

$$R'_{v_{r_1}v_{r_2}}(\tau) = R_{v_{r_1}v_{r_2}} + \delta v_{r_1} \langle v_{r_2} \rangle + \delta v_{r_2} \langle v_{r_1} \rangle + \delta v_{r_1} \delta v_{r_2}$$

If $\delta v_{r_1} < 1$, $\delta v_{r_1}^2 \ll 1$. Moreover, $R_{v_{r_1}v_{r_1}} \sim O\left(v_{r_1}^2\right)$ while the middle term is $O(\delta_{v_{r_1}} v_{r_1})$. So, unless the offsets are of the same order as the radial velocity measurements themselves, the effect of $\delta_{v_{r_1}}$ on the spectra is small (as the spectra are the Fourier transform of the correlation function)

As the verification campaigns show that the offsets are $O(10^{-1})$, their impact on the spectra and coherence will be low. Thus, we do not believe that in-depth treatment of the drift is necessary. Indeed, the user can use the values of the offsets from Table 9 even during the initial stages of the campaign.

- 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.

  As per the reviewer's recommendation, the part concerning comparison with WRF has been replaced by an example of turbulence auto-spectrum and coherence.

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.

The journal marks the paper as a data description article, therefore we will not add “Data Description: “ to the title.

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

  We are following the data description manuscript format, which WES describes as:
  Data description articles extend and advance beyond the technical documentation in a repository by describing original and FAIR research data, and the planning, instrumentation, and execution of experiments, collection or generation of data.
  https://www.wind-energy-science.net/about/manuscript_types.html

  We believe that the local attitudes (positive: support, negative: obstruction), proximity and ability to quickly investigate and repair instrumentation issues, and having prior experience at the test site are all highly relevant to the experiment planning process. We agree that we would most likely not include this in a traditional research article however.

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

  Thank you for highlighting this. This work is better described as testing and verification rather than a rigorous calibration procedure. We have changed the heading and text to reflect this.

- 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?

  The text has been edited for clarity. It was intended to mean that the survey stick positions found by hard targeting in the lidar coordinate system match the calculated azimuth and elevation angle in global coordinates calculated using the measured GPS positions of the lidar scan head and sticks.

- 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.

  A Gannt-like visualization has now replaced the timeline table showing the different stages of the campaign. A short description of the timeline is added with reference to section that describes in more detail the different stages and event in the campaign.

- 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?

  This is replaced with a Gantt chart and references for the different stages and events are added. The pre-campaign is now mentioned in section 2.1 with a reference to the pre-campaign section.

- **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 "Stream- line scanners" and "Streamline units" are used interchangeably. Please revise for clarity and consistency in terminology.**

  The ambiguity regarding the streamline systems has been fixed. Regarding the WindScanners, we follow the convention used in this study: https://www.mdpi.com/2072-4292/8/11/896

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

  The post-campaign calibration is now mentioned in section 2.1 with reference to the post-campaign calibration section.

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

  The estimated precision of the point-cloud is stated, and the angular resolution is estimated.

- 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.

The WindScanners Brise and Sterenn were deployed simultaneously in the test. However, Sterenn was less sensitive than Brise, so the measurements are not completely concurrent. The general uncertainty of the wind speed determination is of the order of 2% which may explain the apparent inconsistency.

- 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.

There is a labelling error as the reviewer pointed out. We intended to show the regression results at the range of 450 m, where both systems could still provide sufficient signal quality for such comparison. The Fig.5 caption and Fig.5 are updated. Thank you for pointing out the error.

At the location of the met mast, we obtained only 69 sample points for Chinook. In the meantime, the data quality is questionable for Storm, as the failure evolved over time and did not immediately degrade the system performance after one single incident.

- Line 155: Please specify, at which ranges the drones were hovering during the pointing verifica- tion. 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.

  The max distance has been added to the table since this sets the upper limit for the precision and the uncertainties are given in the table as precision.

- 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.

  See the answer about the influence of the uncertainty on coherence above.

- 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 ele- vation 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.

  A reference for Gramitzky et al. (2026) is added and a discussion of the impact on the Sea surface levelling results is added. The Gramitzky et al. (2026) does not claim that the hard target-based method is subjected to an offset and therefore it is not relevant to discuss in this situation.

- 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.

The reason why the drone-based results and the Sea Surface levelling-based results differs is for the second drone-based point verification that the drone result is the scanner angle – the drone angle, this includes the correction done after the midcampaign sea surface levelling. Whereas the sea surface levelling results do not include the correction of the elevation but give the elevation misalignment in that azimuth direction. A description of this is added under the Drone-based pointing verifications section.

- 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.

We do not use the radial wind speeds from the hard target scans and any mention of this has been removed from the manuscript. Indeed, we use the hard target scans only to assist in pointing the beams in the right direction.

- 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.

The StreamLine scanners were sent for repairs as described in the Pre-campaign section, and there was therefore not time before the campaign start to do a verification against the mast. However the manufacture made a verification at the factory which showed not significant offset. We therefore as not reason to believe that there is an offset on the streamline scanner. It is correct that Zonda did not take part in the pre-campaign tests. The reason for this is that it was not intended to be used from the beginning, but was used as an replacement of the scanner that failed during the pre-campaign.

- 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.

  Please refer to our previous answer on this topic.

- 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").

  We now use SNR throughout the manuscript

- 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.

  The streamline intensity is the terminology used by the manufacturer as now stated in the manuscript.

- Line 261: For staring configurations at 5° and 11.1° 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.

  If the vertical component, w, is 1 ms −1 then its projection along the lidar beam with an elevation angle of 10, is 1sin10 ≈0.17 m/s. This is about 1.7 % of the radial velocity measurement, if vr =10 m/s. Indeed, the projection of the vertical component along the lidar beam is between 1 %-5 % of the radial wind velocity measurements for the range of elevation angle analyzed in this study.

- Table 12: Consider adding a column describing atmospheric stratification, quantified using a metric such as the Monin–Obukhov length.

We have removed the part related to the comparison between the WRF and measurements, so we do not provide 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.

  Since we have removed the part related to the comparison between the WRF and measurements, we do not define stratification.

- line 296: One could argue that 11.1° is not close to horizontal, see comment before.

  Please see the reply to the comment above.

- 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.

  The data are time-stamped in UTC timezone. Thus, time synchronization is trivially achieved.

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

  Corrected

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.

We have updated the code availability section to highlight that part of the script cannot be executed without DTU credentials. Since we argue that the radial wind speed correction only involves addition of a constant offset, we leave it to the user to implement it.