Responses to Reviewers' Comments for Manuscript

The LOLland offshore Lidar EXperiment (LOLLEX): A novel observational approach for the study of wind farm flow and entrainment

Addressed Comments for Publication to

Atmospheric measurement techniques

by

Malekmohammadi et al.

1 Authors' Response to Reviewer 1

1.1 General comment

General Comments. The paper presents an interesting dataset collected around an offshore wind farm with a combination of three lidars. The results are mostly qualitative and not novel (i.e., atmospheric waves occur over the ocean) and present some limitations. The authors create high expectations in the abstract and introduction by referring to a novel "strategy to detect vertical momentum entrainment", but it falls a bit short in the content. First, for momentum entrainment one usually refers to the $\overline{w'u'}$ Reynolds stress, while only w' is shown. Then, the only novelty in the strategy seems to be the fact that the lidars are on a ship yet performing well-known scans. The potential of the colocation of a V2 and a 100S is not explored as only measurements from individual instruments are shown. This brings to the second point: none of the case studies show profiles of wind speed and direction, which would be interesting to see after all the efforts in motion correction and comparison with NORA3. Third, it sounded like the wind profiles from the 100s were not motion-corrected. This is hard to understand and the fact that the RMSE of the 100S winds have lower RMSE than the V2 ones when compared to NORA3 creates additional confusion in the reader. Fourth: another singular approach is the execution of RHIs trying to capture the wake velocity in the cross-stream plane. Although the lidar is positioned favorably to scan the y-z plane for E-W wind, the line-of-sight projection of the wake velocity field is minimal. This counters almost all the lidar wake campaign (ground- or nacelle-based) where indeed one tries to align the lidar with the wind as much as possible. Acknowledging these limitations, major revisions are recommended. A revised version of the paper should address these four points:

- Either estimate $\overline{w'u'}$ with a new approach or mitigate the claims of a novel strategy.
- Show wind speed and direction from both lidars for the case studies.
- Motion-correct the 100S profiles or justify why this is not done.
- Explain better the quite counter-intuitive RHI for cross-stream wake detection strategy or remove that section (it adds little information on the wake physics and model accuracy).

Response:

We would like to thank the reviewer for their thorough review. We have revised the manuscript accordingly and hope it addresses their concerns.

First, we acknowledge that vertical momentum entrainment is formally linked to the divergence of the Reynolds stress $\overline{u'w'}$. We have clarified this definition and added a dedicated section proposing an estimate of the vertical momentum entrainment as

$$E_u(z) \equiv -\frac{\partial}{\partial z}\left(\overline{u'w'}\right), \tag{1}$$

Case studies 1–3 now explicitly include an analysis of $E_u(z)$. The contribution is therefore framed as a ship-based measurement strategy with an applied entrainment analysis rather than as a novel turbulence retrieval technique.

Second, the case studies have been revised to include measurements from both lidars wherever data quality allows, enabling clearer assessment of atmospheric conditions, motion correction, and inter-instrument consistency.

Third, we have expanded the discussion of motion correction for the WindCubeV2 and WindCube100S. We now explicitly include a comparison between uncorrected and corrected wind speeds in Section 5.1 Motion correction results. We clarify that translational vessel motion and intermittent data loss during scanning limit the robustness and impact of motion correction for the 100S in some cases.

Fourth, the RHI scan was selected to support the campaign's focus on vertical momentum entrainment. The choice of only one RHI scan was made with the purpose of providing sufficient statistics of continuous data in the targeted plane. Nevertheless, since a DWL only measures the LOS speed component, this configuration imposes some limitations on the wind directions for which the wakes can be observed. In most cases, wake signatures are detectable, except when the wake's lateral cross-section is exactly aligned with the RHI plane. In that case, the lidar cannot detect any component of the axial wake flow. We have revised the manuscript to clarify this observational constraint and better explain the rationale for the chosen RHI orientation. Also, section 5.4 (case study 4) has been rewritten to better highlight the value of the RHI scan for wake analysis by the lidar on the transformer platform.

1.2 Specific comments

Comment 1

L11: which scanning lidar was doing profiling + stares? Please specify.

Response:

We have added the information in the abstract as follows :

During this campaign, two pulsed Doppler wind lidars, a scanning lidar (WindCube100S) and a lidar wind profiler (WindCubeV2), were deployed onboard a crew transfer vessel (CTV) commuting daily between the harbour and the offshore wind farm Rødsand II. Additionally, a scanning pulsed Doppler wind lidar (Halo Photonics) was mounted on a transformer platform north of the wind farm to perform range height indicator scans across the farm. Horizontal wind speed data were collected up to 300 m above the sea surface by the lidar wind profiler. The scanning lidar on the CTV collected data up to 2.5 km alternating between the wind profiling mode and vertical stare mode.

Comment 2

L36: I would replace "beyond certain scales" with "for large wind farms".

Response:

Thank you for the suggestion, we have revised the manuscript accordingly.

Comment 3

L53: What does "most offshore DWLs rely on fixed platform"? For installation or for validation? Please rephrase

Response:

We have revised the manuscript as:

Most offshore DWLs are deployed on fixed offshore infrastructure such as substations or platforms supporting bottom-fixed meteorological masts [...]

Comment 4

L101: Please specify that the WindCube are the lidars on the ship or just avoid mentioning the lidar model at this point.

Response:

Thank you for your feedback. This sentence has been moved to acknowledgments.

The lidars deployed on CTV were provided by the national Norwegian research infrastructure OBLO (Offshore Boundary Layer Observatory).

Comment 5

L129: IMU-derived location can drift over time, was it corrected with the GPS? Please expand.

Response:

Thank you for raising this point. IMU-derived position and attitude are subject to drift over time due to integration errors inherent to inertial sensors. However, in the WindCube V2, the IMU is part of an integrated unit that is linked to a differential GPS (GNSS), as described in the instrument documentation. This linkage indicates that the reported position (latitude, longitude, altitude) and attitude are not based on a purely inertial solution. Instead, the inertial measurements are constrained by the GPS, which limits the accumulation of low-frequency drift over time.

Comment 6

L133: 5 minutes is shorter than the typical 10 minutes used by the wind industry, please justify this choice.

Response:

The 5-min averaging period was selected to maximize the duration of the vertical stare scans within each hour. The measurement cycle consisted of 2 × 25 min vertical stare scans and 2 × 5 min DBS scans per hour. Extending the DBS duration to 10 min (e.g., 2 × 20 min vertical stare + 2 × 10 min DBS, or other similar combinations) would have reduced the temporal coverage of the vertical stare mode, which was prioritized for turbulence and entrainment analysis. We evaluated the impact of averaging length by comparing 5-min and 10-min averages from the vertical lidar and found only minor differences in mean wind speed, with the expected increase in statistical uncertainty for the 5-min averages. The selected configuration therefore represents a practical compromise between reliable wind profile estimation and maximizing vertical stare sampling time, and this rationale has been clarified in the manuscript.

We have rephrased the paragraph as

"The instrument operated on a 30–min scan cycle alternating between two modes: 5 minutes of Doppler Beam Swinging (DBS) for wind profiling (left panel in Fig. 4) and 25 minutes of vertical velocity measurements in vertical stare mode (right panel in Fig. 4). The 5-min DBS duration, shorter than the 10-min averaging period commonly used in the wind industry, was selected to maximize the duration of vertical stare measurements used for turbulence and entrainment analysis. Extending the DBS duration to 10 minutes would have reduced the temporal coverage of the vertical stare mode. A comparison between 5-min and 10-min averages derived from the WindCubeV2 showed only minor differences in mean wind speed, apart from the expected increase in statistical uncertainty for the shorter averaging period."

Comment 7

Fig.4: Please keep same font size across the figure.

Response:

Thank you for your feedback. The figure has been updated.

Comment 8

Section 3.1: “filtering” and “outlier removal” are generally synonyms. Please clarify if this is simple a 2-step quality control using first SNR and then median filter.

Response:

We thank the reviewer for this comment.

Section 3.1 has been revised to clearly describe these two sequential quality-control steps (1) CNR-based threshold filtering and (2) MAD-based statistical outlier removal, followed by motion correction.

First, CNR-based threshold filtering applies a fixed CNR threshold at each time and range gate (-24 dB for the WindCubeV2 and -27 dB for the WindCube100S) to exclude low Carrier-to-Noise Ratio measurements. Second, MAD-based statistical outlier removal uses a moving median absolute deviation (MAD) filter to remove spurious values that pass the CNR threshold but are inconsistent with local temporal variability.

Comment 9

Section 3.2: Please provide information on the timestep used in the correction.

Response:

The motion correction is applied at each synchronized time step. For the WindCubeV2, one beam is recorded per second (5 s per full scan), and for the WindCube100S, one beam is recorded every 3 s (15 s per full scan).

To ensure temporal consistency between beams and motion data, both radial velocities and motion signals (translational velocities and attitude angles) are interpolated onto a common 1 Hz time base. This enables temporal collocation and synchronization of the five beams forming each scan. At each time step, the correction is applied simultaneously to the synchronized beam set using the corresponding rotation matrix and translational velocity vector, and the wind vector is retrieved via least-squares pseudo-inversion as described in Eq. 5.

Although wind vectors are computed at 1 Hz after interpolation, this upsampling does not introduce additional physical information and is used only for subsequent statistical aggregation (e.g., mean estimates). This clarification has been added to Section 3.2.

Comment 10

Eq 6: This equation uses only the tilt angle while for sonic anemometers and in reality also pitch and yaw play a role in defining the projection of the horizontal wind speed onto the beam direction. It other words, this seem to apply to the special case where the tilt is plane defined by the wind direction. Please justify this simplification.

Response:

In Eq. 10 (Eq. 6 in previous version), $\overline{u}$ is expressed in a coordinate system aligned with the mean horizontal wind direction after an initial horizontal rotation, such that yaw misalignment is removed. In this wind-aligned frame, the projection of the mean flow onto the vertical axis depends only on the inclination angle θ, and pitch and roll do not appear explicitly.

The equation corrects the mean vertical velocity for the measured (static) tilt angle θ, applied directly in the transformation. This approach differs from the classical double-rotation method used for sonic anemometers, where tilt is adjusted statistically to enforce $\overline{w} = 0$. Therefore, Eq. 10 represents a physically prescribed tilt correction in the wind-aligned frame, which is a valid simplification for the flow conditions considered. This clarification has been added to the manuscript.

Comment 11

L243: Vertical wind speed is never "removed" in turbulence analysis, please omit.

Response:

We have revised the sentence as

It should be noted that in turbulence analysis, velocity components are decomposed into mean and fluctuating parts using Reynolds decomposition

$$i = \bar{i} + i', \tag{2}$$

where $i = \{u, v, w\}$. *The static tilt correction is therefore applied to obtain an unbiased estimate of the mean vertical velocity without assuming it to be zero. This correction does not affect the turbulence statistics, as these are computed* *from the fluctuating component, which by definition has zero mean. Hereinafter, the scanning lidar data are shown with tilt correction using Eq. 10.*

Comment 12

Fig. 11: Please make time format consistent between the two panels.

Response:

We appreciate your suggestion. We have updated the figure as fig. 1. However, following the recommendation of Reviewer1, we have replaced the time series with a scatter plot comparing the lidar data and NORA3 before and after motion correction, as this provides a clearer overall assessment. The revised figure is presented as Fig. 10 in the manuscript.

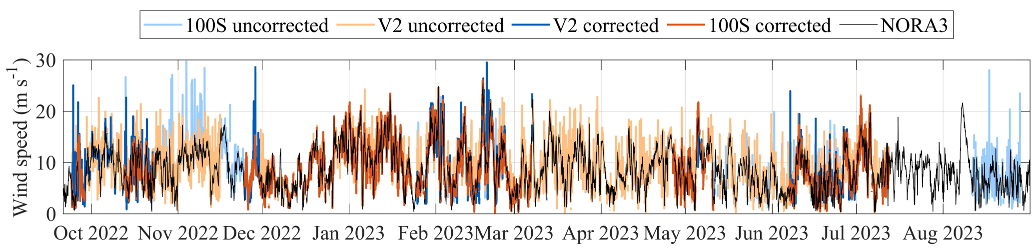

Figure 1: Time series of wind speed at 100 m above the surface during the LOLLEX campaign, comparing the ship–based lidar measurements with the NORA3 reanalysis data.

Comment 13

L298: Please mention that NORA may not be capturing the physics of the near-surface winds (low-level jet, sea surface temperature, wave height, etc.), on top of the wake effects. It is not uncommon for NWP to show biases in the lower ABL.

Response:

We thank the reviewer for this comment. We have added completed the paragraph as

[...] The WindCubeV2 and WindCube100S show a positive bias below 100 m, indicating that NORA3 overestimates wind speed at lower heights. The NORA3 hindcast does not consider wind-farm wakes, while the lidar measurements are affected by the flow deceleration caused by the farm, a phenomenon also documented at the FINO1 site (Podein et al., 2022; Cheynet et al., 2025). It is worth mentioning that NORA3 has a horizontal resolution of 3 km and represents boundary-layer flows more realistically than the earlier 10 km NORA10 hindcast (Haakenstad et al., 2021). Nevertheless, uncertainties in the representation of near-surface wind processes may remain. Evaluations of similar high-resolution wind atlas datasets (3 km) show that they reproduce near-surface wind characteristics fairly well, although certain aspects of boundary-layer dynamics, such as the structure of low-level jets, may still be imperfectly represented (Kalverla et al., 2020; Rubio et al., 2022).

Comment 14

L315: Please add the date and the time zone, not just the hour.

Response:

We have added the information to L315 as:

... the analysis was limited to the period between 12:35 and 13:00 UTC on 22 February 2023.

Comment 15

L334: It is unclear why tip vortices would have a persistent positive velocity instead of a +/- pattern. Also, it is questionable that the resolution of the lidar would allow to resolve a tip vortex. Please either omit or add more references showing the same positive w over such a large vertical range.

Response:

We thank the reviewer for raising this important point. We agree that suggesting a signature of blade tip vortices was not correct. Given the lidar measurement configuration and spatial resolution, individual tip vortices cannot be resolved.

In our case, the observed vertical velocity band extends from approximately 100 m to 200 m height and persists for about 37 minutes. We interpret this as a deflection of the streamlines when passing the rotor plane. We have reformulated the manuscript as:

Coincidentally, the lidar measurements, located approximately 37 m downstream of the turbine, captured the effect of the turbine on the local flow field. This appears in the vertical velocity time series as a band of enhanced positive vertical velocity between 100 m and 200 m height, visible between 12:35 and 13:12. The spatial extent of the enhanced vertical velocity band indicates a rotor-induced streamline deflection in the near-wake region. This interpretation is consistent with previous studies of near-wake dynamics (Whale et al., 2000; Hong et al., 2014; Dasari et al., 2019; Lanzilao & Meyers, 2024, e.g.,), showing comparable vertical velocity disturbances downstream of turbines.

Comment 16

L339: The statement about the need for horizontal homogeneity to derive wind profiles is correct, but horizontal wind is not shown in Fig. 13 (only w, which does not require this assumption). Please clarify.

Response:

Thank you for raising this point. We have removed this paragraph.

Comment 17

L367: Please clarify if the vessel was moving during the case study 2.

Response:

The CTV was not moving at the beginning of the case study. It began moving at 07:12 UTC the last two minutes of the case study (fig. 2). This information was added to the manuscript.

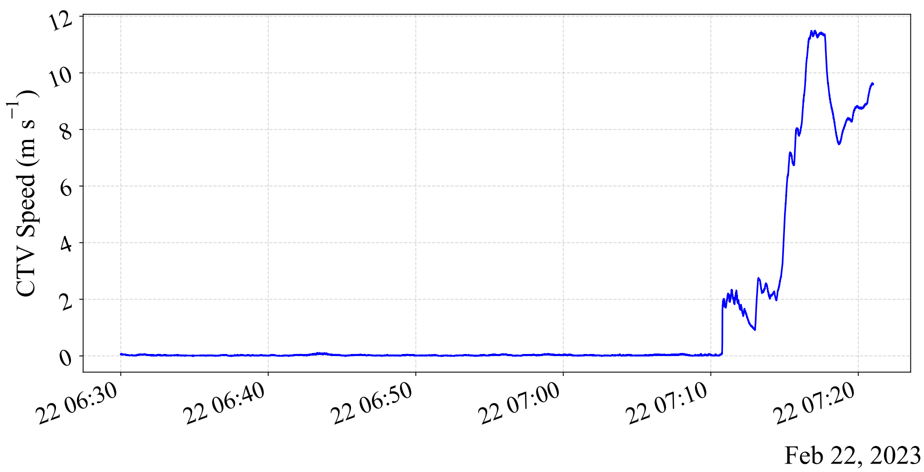

Figure 2: CTV speed during the case study 2. The CTV started moving from 07:12 onward.

Comment 18

Section 5.2: The KHB hypothesis seems to be ruled out too quickly without a clear justification. In fact, at L388 the "shear instability" is mentioned. Please reconcile.

Response:

We agree that the initial draft did not sufficiently justify why Kelvin–Helmholtz billows (KHB) are considered unlikely in this case, and we have revised Section 5.2 accordingly. Although strong vertical shear is present and may contribute to shear-driven instability, this does not by itself demonstrate the presence of KHB. In our previous KHB observations (Malekmohammadi et al., 2025), the billows were characterized by a clear narrow-banded spectral peak, a well-organized periodic pattern in the time series, and a persistent kurtosis peak. In the present case, by contrast, the power spectral density of w shows a broader increase in energy across a range of frequencies rather than a narrow-banded peak, and the time series of w and CNR do not display similarly organized periodic structures. In case study 2, the negative skewness peak around 300 m was used as an indicator consistent with turbulence generated by cloud-top radiative cooling; however, a negative skewness peak was also present in the KHB case of Malekmohammadi et al. (2025), indicating that this feature is not specific to one mechanism. We also examined the kurtosis of *w*: a local peak occurs between 06:45 and 07:00, which could suggest intermittent coherent structures consistent with KHB, but it disappears during 07:05–07:12 when the local turbulence intensity continues to increase. This lack of a persistent kurtosis signature differs from the KHB case. Taken together, these observations indicate that KHB cannot be fully excluded, but the measurements are more consistent with intermittent entrainment and mixing near the ABL top, potentially associated with cloud-top radiative cooling, than with a classical KHB event.

Comment 19

L505: it is unclear where the robustness of the motion correction of the V2 as shown. The V2 showed instead higher REMS when compared to NORA3. Please clarify or omit.

Response:

As correctly mentioned by both reviewers, some discrepancies were observed in the results after applying motion correction. After carefully reviewing the algorithm, we detected a bug in our translational motion correction, which caused such increase in RMSE. We fixed the bug in the motion correction algorithm and applied it to the data again. Then the results were compared to the NORA3 data. Please see the updated text and figures in section 5.1.

Comment 20

Discussion: this section could be combined with the Conclusion since it repeats many concepts and makes the paper unnecessarily lengthy.

Response:

We thank the reviewer for this suggestion. We chose to keep the Discussion and Conclusion as separate sections because they serve different purposes in our paper. In the Discussion, we provide a critical reflection on the measurement campaign, including its limitations, uncertainties, and implications and our suggestions for future works. The conclusion summarizes the main findings of our study. Nevertheless, we have revised both sections to reduce redundancy.

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