

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.

Authors' Response to Reviewer

Reviewer 1

0.1 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

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

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.

0.2 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 off-shore 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. In other words, this seems 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), $\bar{u}$ 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 $\bar{w} = 0$. 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

$$i = \bar{i} + i', \quad (2)$$

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.

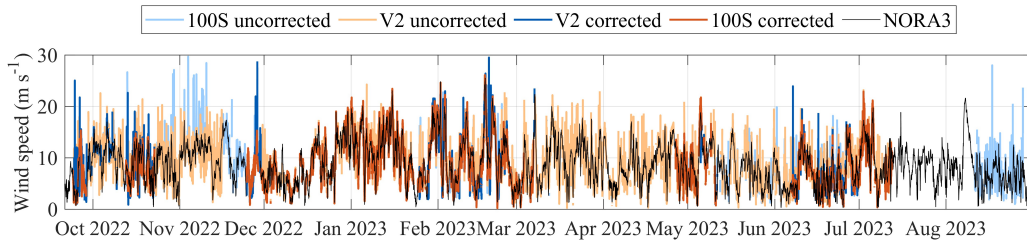


Figure 1: Time series of wind speed at 100 m above the surface during the LOLLEX campaign, comparing the ship-based lidar measurements with the NORA3 reanalysis data.

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.

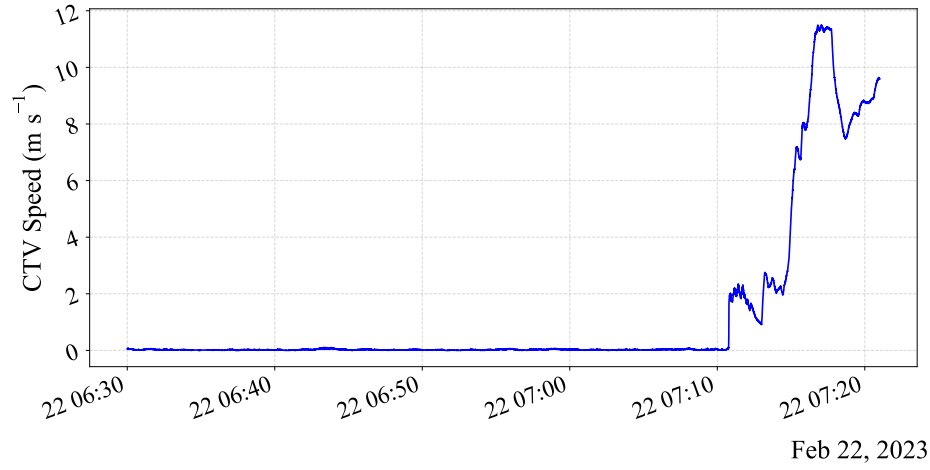


Figure 2: CTV speed during the case study 2. The CTV started moving from 07:12 onward.

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.

Authors' Response to Reviewer

Reviewer 2

0.3 General comment

General Comments. Overall, the paper presents a broad overview of the LOLLEX campaign but fails to provide important details. The paper could be improved by incorporating an expanded joint analysis of the mean wind observations conducted with two lidars and the turbulence data from the vertical stares.

Response:

We would like to thank the reviewer for their thorough review. We acknowledge that the paper falls short of an in-depth joint comparison between the measurements of the two lidars and also the turbulence data for the entire campaign. We have attempted to include this analysis in the manuscript. In addition, we have carefully revised the manuscript based on the specific comments below. We hope that this new version addresses the comments of the reviewer.

0.4 Specific comments

Comment 1

In Fig. 4, it would be good to add the elevation angle for the DBS scan. Also, what was the motivation for using a 4-beam rather than 5-beam (with the 5th beam pointing vertical) DBS scan with the WindCube100s similar to the WindCubeV2 DBS scan? Please clarify if the WindCube100s DBS scans runs for 5-min how many estimates of u , v , w are retrieved, and if they are then averaged into 5-min samples.

Response:

Thank you for your comment, the figure is updated accordingly. After re-checking the instrument logs, we confirm that the WindCube 100S was configured to perform a 5-beam DBS scan rather than a 4-beam scan as initially stated. During

each 5-minute DBS period, approximately 80 individual measurements were collected, which corresponds to an effective sampling rate of about 0.3 Hz. From these measurements, instantaneous values of wind velocity and wind direction were retrieved; these were then averaged in post-processing to form 5-minute mean wind components.

We have updated figure 4 and added the following to the section 2.2.1:

In vertical stare mode, which corresponds to a line-of-sight (LOS) configuration with an elevation angle of $\varphi = 90^\circ$, the lidar operated at a sampling frequency of 1 Hz at ranges between 50 m and about 2500 m. In DBS mode, the lidar operated using a five-beam scanning configuration. Four oblique beams were sequentially emitted at an elevation angle of $\varphi = 75^\circ$ and azimuth angles of $\alpha = (0^\circ, 90^\circ, 180^\circ, 270^\circ)$, followed by a vertically pointing beam with an elevation angle of $\varphi = 90^\circ$ at ranges between 50 m and about 2500 m (Fig. 3). During each 5-min DBS period, approximately 80 individual samples were collected, corresponding to an effective sampling frequency of 0.3 Hz. In wind profiling mode, the lidar retrieves wind speed and wind direction. Both instantaneous estimates and 5-min time-averaged products were computed in post-processing (see Section 3.3).

Comment 2

The caption of Fig. 5 mentions a top and bottom panel, but the figure shows a left and right panel.

Response:

Thank you for the feedback. This oversight is now corrected.

Comment 3

The paper states: "When deployed on the vessel, the WindCube100S occasionally produced short or incomplete scans, likely due to its sensitivity to rapid translation and acceleration. Unlike the WindCubeV2, which is designed for dynamic conditions on buoys, the WindCube100S is not optimised for mobile offshore deployments. Despite these limitations, a large number of high-quality scans were obtained, providing valuable measurements within the wind farm. Outlier removal was accomplished by a moving median absolute deviation filter, following the recommendation by Starkenburg et al. (2016) and Leys et al. (2013)." For a campaign overview paper, it would be good to add quantitative information about the percentage of datasets flagged and removed.

Response:

We have revised the section 3.1:

The WindCube100S occasionally produced short or incomplete scans when deployed on the vessel, likely due to its sensitivity to rapid translation and acceleration. This can be attributed to its mechanically rotating scanning head for beam steering, in contrast to the WindCubeV2 Offshore, which operates without moving parts and is designed for dynamic buoy deployments. For the vertical staring mode conducted between 2023-01-25 and 2023-08-28 (approximately 9,900 scans), 65% of the WindCube100S scans were flagged as incomplete (scan duration < 10 min) and excluded, while 30% achieved the full 25-min duration.[...]

Comment 4

The description of the filtering applied to the Halo lidar at the top of page 10 of the paper is not very clear - what motivated this particular method? Is the filtering done for each individual RHI scan or over a certain period? What is the percentage of data removed?

Response:

We have rephrased the corresponding paragraphs and refined the description of the data filtering as follows:

Following [Päschke et al. \(2015\)](#), a signal-to-noise ratio (SNR) threshold of -20 dB is commonly recommended for radial velocity data from the Halo Photonics Stream-Line XR+. In the present case, this threshold appeared overly conservative. The lidar data were used solely for mean wind speed estimation, and the beam was frequently oriented at angles close to 90° relative to the mean wind direction, resulting in generally low SNR values. In addition, the SNR was originally provided on a linear scale, with a substantial fraction of values being negative, preventing a consistent conversion to dB.

For the case study 4, a moderate correlation ($\rho \approx -0.63$) was found between the SNR and $|v_r - v_0|$, where v_r is the along-beam (radial) velocity and v_0 denotes the most probable radial velocity. However, substantial overlap between low- and high-deviation data across the SNR range indicates that SNR alone is not a reliable indicator of data quality. Consequently, no SNR-based filtering was applied. Radial velocity data were first filtered to remove implausible values by discarding samples with $|v_r| > 30 \text{ ms}^{-1}$. The most probable radial velocity v_0 was then estimated as the mode of the v_r distribution. Only samples satisfying $|v_r - v_0| < k \text{MoAD}$ were retained, where $\text{MoAD} = \text{median}(|v_r - v_0|)$ denotes the median absolute deviation about the modal radial velocity v_0 , and $k = 3$ is a dimensionless threshold parameter.

In summary, the filtering does not rely on SNR but instead on the statistical structure of the radial velocity distribution, which was found to be more robust for the present dataset due to the large number of undefined values (SNR < 0 in linear scale). The rationale for this approach is illustrated in Fig. 3, which shows the distribution of v_r , the estimated mode, and the retained interval. The filtering was applied independently to each RHI scan.

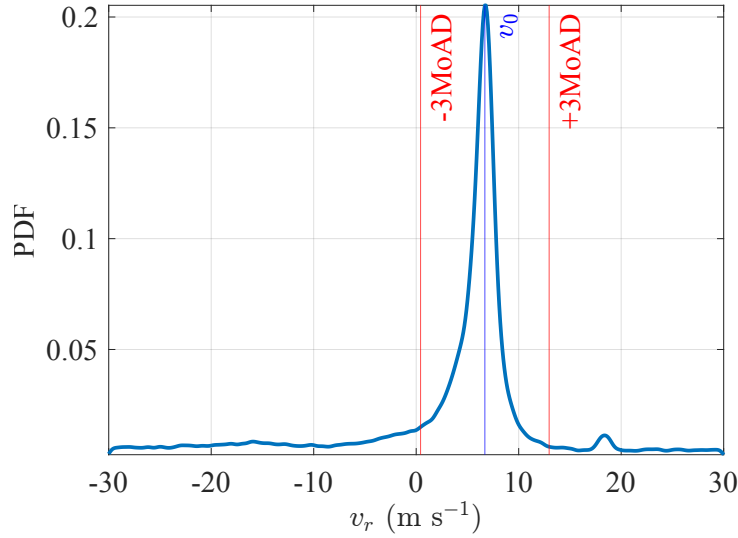


Figure 3: Probability density function of the along-beam velocity for the scanning lidar on the transformer platform (case study 4). The modal velocity v_0 is indicated, and the retained data correspond to the interval $|v_r - v_0| < 3\text{MoAD}$.

Comment 5

The paper introduces the motion correction and states: "The efficacy of this algorithm has been validated under controlled conditions using a six-DOF motion platform (Malekmohammadi et al., 2024). Thus, this motion correction approach is well-suited for the deployment of DWLs on CTV or other vessels." In the cited paper, the algorithm was only validated for one test with pitch motion, the performance for other motions is not documented and it thus remains unclear how well it performs in a complicated ship-based deployment with different types of motions. No comparisons between uncorrected and corrected data are provided and it is unclear how critical the corrections are and if the corrected data look reasonable. In fact, I am not even sure they were applied in this paper.

Response:

We thank the reviewer for raising this important point. Motion correction was applied to the dataset used in the manuscript. To clarify its impact, we now explicitly include a comparison between uncorrected and corrected wind speeds in Section 5.1 Motion correction results.

During revision, and following comment 14, we carefully re-examined the motion correction algorithm and identified a bug in the translational motion correction, which led to an artificial increase in RMSE. In Malekmohammadi et al. (2024), rotational motion dominated and translational motion was limited. In the present campaign, however, the CTV’s translational motion represents the primary source of motion-induced error, which helped to detect this issue.

After correcting the algorithm, we re-applied the motion correction to the entire dataset and compared the results against NORA3, which are presented in Section 5.1.

Comment 6

A tilt correction introduced in a previous publication by the lead author (Malekmohammadi et al., 2025) is discussed in section 3.3 but open questions remain. In one of the methods, the vertical component w from the WindCubeV2 is used as target to be matched when evaluating the tilt angle. Fig. 7 shows that the WindCubeV2 w -values at 150-m are non-zero (~ 0.3 m/s) at the beginning of the time period plotted but then approach zero. How is it known that this trend in w is not caused by motions of the lidar but is in fact the true value that should be matched but the scanning lidar?

Response:

We thank the reviewer for raising this point. The decrease in $\bar{w}$ is consistent with the evolution of the mean wind speed during that day. From the early hours toward the evening, the mean horizontal wind speed at 150 m ($\bar{u}$) decreased substantially, from approximately 20 m s^{-1} to about 5 m s^{-1} . If the flow has a small but approximately constant tilt angle α , the mean vertical velocity scales with the horizontal wind speed according to $\bar{w} \approx \bar{u} \tan(\alpha)$; thus $\bar{w}/\bar{u}$ remains approximately constant while $\bar{w}$ varies proportionally with $\bar{u}$. Consequently, a decrease in $\bar{u}$ should lead to a corresponding decrease in the observed $\bar{w}$. This behaviour is consistent with Fig. 7, where $\bar{w}$ is around 0.3 m s^{-1} when $\bar{u} > 15 \text{ m s}^{-1}$ and approaches zero as $\bar{u}$ further decreases. While strong winds could in principle introduce small platform tilts even in harbour, the observed scaling of $\bar{w}$ with $\bar{u}$ is consistent with a tilted mean flow rather than an instrumental artifact.

To avoid focusing on a single day and to provide a more robust assessment of the static tilt correction algorithm, we removed the figure showing one day of data and

instead present the probability density function (PDF) of the vertical velocity difference between the WindCube100S and the WindCubeV2 using data collected between 2023-01-01 and 2023-07-12 (more than 6000 samples). This analysis demonstrates that the proposed static tilt correction performs well and outperforms both the non-corrected approach and a simple bias correction in which the bias is assumed to be the value of $\bar{w}$ measured at height 400 m, far from ground obstacles.

Comment 7

Applying, the tilt correction to the WindCube100S shows strange results: in the afternoon the black line (scanning lidar) suddenly does not agree with the WindCubeV2 w -values (blue line) and no explanation is given about what is causing this sudden discrepancy.

Response:

We thank the reviewer for pointing this out. The discrepancy observed in the afternoon in the former Fig. 7 coincided with a short rain shower, as indicated by the CNR signal and the temporal pattern of w . Precipitation can occasionally introduce spurious vertical velocity signals in Doppler lidar measurements when falling hydrometeors contribute to the backscatter. In this case, the rain was not intense enough to cause data loss but likely led to artificially elevated w values for a short period. Since precipitation effects on Doppler lidar measurements are beyond the scope of this study and such events were rare in the dataset, we did not analyse them further. To avoid drawing conclusions from a single day, the figure has been removed and replaced with a probability density function (PDF) of the vertical velocity difference between the WindCube100S and the WindCubeV2 using data collected between 2023-01-01 and 2023-07-12. We also added a brief statement in the manuscript noting that precipitation can occasionally introduce spurious vertical velocity signals in Doppler lidar measurements and that such events were not specifically analyzed here.

Comment 8

Also, what tilt angle is determined when using the alternative method of forcing mean w at 400 m to be zero??

Response:

The alternative method does not explicitly determine a tilt angle. Instead, it assumes that the long-term mean vertical velocity at 400 m should be zero and removes any constant bias in w at that height. The removed offset is implicitly interpreted as the effect of a static tilt of the lidar. Thus, this approach applies a constant correction to the vertical velocity component rather than estimating a geometric tilt angle. Our analysis showed that this method tends to over-correct the data, particularly under conditions with non-zero large-scale vertical motion. For this reason, we developed and applied the revised approach described in [Malek-mohammadi et al. \(2025\)](#), which provides a more physical estimation of the static tilt effect.

Comment 9

If the tilt angle is known from visual inspections, why are any of the other methods needed?

Response:

The tilt angle inferred from installation photographs represented only the initial geometric alignment and did not account for quasi-static changes in vessel attitude during operation. The equilibrium attitude of the CTV can vary due to changes in mass distribution (e.g. fuel consumption or freshwater usage), producing a slowly varying static tilt. Such a tilt introduces a vertical velocity bias approximately given by

$$w_{\text{bias}} \approx \bar{U} \sin \theta.$$

Under high wind speed conditions (e.g. $\bar{U} \approx 20 \text{ m s}^{-1}$), even small tilt angles can therefore generate a great artificial vertical velocities.

Nonetheless, we have used a constant value of 2.7° for simplicity, since this could lead to greater data loss when DBS data from the 100S are unavailable. A more detailed analysis could assess how the tilt angle depends on the vessel's mass distribution (e.g., fuel, freshwater, and payload), but this is beyond the scope of the present study.

We have clarified this in the manuscript as:

The tilt angle derived from visual inspections represents the initial geometric alignment. During operation, quasi-static changes in vessel attitude (e.g. due

to varying mass distribution) may occur and introduce a vertical velocity bias of approximately $w_{\text{bias}} \approx \bar{U} \sin \theta$. In principle, the effective static tilt angle can be estimated for each measurement period using the Eq. 10. In this study, however, a constant value of 2.7° is used for simplicity and to preserve high data availability.

Comment 10

Finally, Fig. 7 shows data for one day. For a campaign overview paper, I would expect to see a more detailed statistical evaluation of the tilt correction with quantitative information about e.g. mean bias before and after the different corrections were applied.

Response:

We agree with the reviewer. To have a better picture, we have calculated the probability density function (PDF) of the Δw between the WindCube100S and the WindCubeV2 for the entire campaign and replaced figure 7 with fig. 4. We have also added the following to the main text:

Figure 4 presents the probability density function (PDF) of the vertical velocity difference between the WindCube100S and the WindCubeV2, using three different methods for tilt correction: no correction, a correction assuming $\bar{w} = 0$ at 400 m, and tilt correction with $\theta = 2.7$ derived from Eq. 10. The corrected WindCubeV2 is used as the reference instrument due to its dedicated wind-profiling configuration, continuous DBS operation, lower mechanical complexity, and consequently higher robustness and lower measurement uncertainty compared to the scanning WindCube100S. The PDF is computed from all available high-quality data collected between 2023-01-01 and 2023-07-12 (> 6000 samples). Without correction, the vertical velocity from the scanning lidar exhibits a positive bias, with a median of 0.28 m s^{-1} and values reaching up to 1 m s^{-1} . Applying a constant correction based on the assumption of zero mean vertical velocity at 400 m reduces the bias but over-corrects, resulting in a negative median of -0.20 m s^{-1} . The tilt-correction method based on Eq. 10 yields the lowest absolute bias (-0.05 m s^{-1}), the lowest root-mean-square error relative to the WindCubeV2, thus producing a noticeably narrower PDF, indicating improved accuracy and precision compared to the other approaches. It should be noted that in turbulence analysis, velocity components are decomposed into mean and fluctuating parts using Reynolds

decomposition

$$i = \bar{i} + i', \quad (3)$$

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.

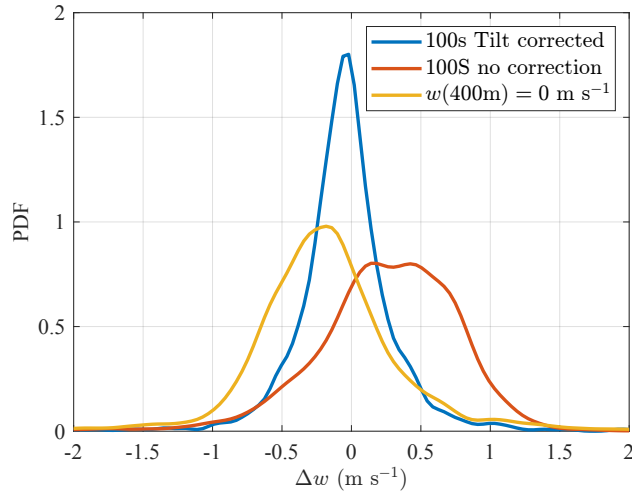


Figure 4: Probability density function (PDF) of the vertical velocity difference between the WindCube100S and the WindCubeV2 using data collected between 2023-01-01 and 2023-07-12 (> 6000 samples).

Comment 11

In line 269 it should say: “ Fig. 10 shows a comparison between the wind roses from WindcubeV2 and NORA 3 data.”

Response:

The line 269 was updated to

"[...] shows a comparison between the wind roses from NORA3 and WindcubeV2 data at 150 meters above sea level. "

Comment 12

The time series analysis in section 4.2 is confusing. I could not track down the source of Eq. 7 which is called an empirical fit to the Durst curve from 1960. Durst's work focused on relating maximum gust wind speeds to longer term mean wind speeds. Here the comparison deals with comparing hourly (but equivalent to 10min averages?) NORA 3 data to the Wind Cube 100s data (5-min averages recorded every 30 min) and Wind Cube V2 data (10-min averages). While I value the attention to the effect of different averaging times when comparing the data I do not see the value of blindly applying Eq. (7). The lidar scanning itself leads to averaging. Were the high-resolution time series of the Wind Cube V2 data and Wind Cube 100s not available for reprocessing using different averaging times? It would be best to compare 5-min vs. 10-min averages from the V2 and estimate if corrections should be applied to the 100s. The vertical resolution of the different lidars and the model data should also be discussed as it also affects the data. Also, Fig. 11 shows the temporal trends, but it is not ideal for visualizing the agreement or disagreement between the data sets. Scatter plots would be much better.

Response:

We agree that the effect of different averaging periods should be evaluated directly using the lidar measurements. Following this suggestion, we reprocessed the high-resolution lidar data and compared 5-min and 10-min averages derived from the WindCubeV2 measurements. The resulting differences in bias are small (within about 1%), indicating that the averaging-time difference has only a minor impact; therefore no correction was applied and Eq. (7) and the discussion related to the Durst curve were removed from the manuscript. To improve the comparison between datasets, we added scatter plots comparing the lidar measurements with the NORA3 data, in addition to the vertical profiles of bias and RMSE for the mean wind speed $\bar{u}$ (with and without motion correction), which better illustrate the agreement between datasets. The WindCubeV2 and WindCube100S have comparable probe volume lengths (≈ 25 m); therefore, differences in vertical resolution are negligible.

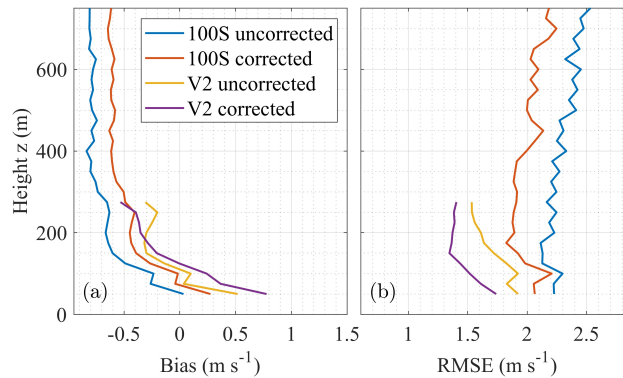


Figure 5: Comparison between the NORA3 and wind lidar measurements at various heights during the LOLLEX campaign. Left: Mean bias between NORA3 and the WindCube100S scanning lidar uncorrected (blue), corrected (red), WindCubeV2 lidar wind profiler uncorrected (orange), and corrected (purple), based on 10-minute averaged wind speed. Right: Root mean square error (RMSE) between NORA3 and the two lidars before and after motion correction.

Comment 13

In line 300, the reader should be pointed to Fig. 12

Response:

That is correct. The reader now is pointed to the appropriate figure.

Comment 14

In line 303 the authors write: “The RMSE is slightly higher for the WindCubeV2, which may be caused by uncertainties introduced during motion correction” Does this mean the motion correction makes things worse?

Response:

After correcting the motion-correction algorithm (see response to Comment 5), we recomputed the bias and RMSE profiles shown in Fig. 5. We have revised the manuscript accordingly.

Comment 15

In line 306 it is stated "Overall, both lidars show consistent performance compared to NORA3, especially above the rotor layer." How is consistent performance defined?

Response:

We acknowledge that the term "consistent performance" was used ambiguously and have therefore removed this statement from the manuscript.

Comment 16

Lines 324-238: "Before the event, a bright and persistent CNR maximum around 600 m indicates aerosol accumulation beneath the capping inversion, a common precursor to shear-driven instabilities." What causes the aerosol accumulation and what exactly serves as precursor to shear-driven instabilities?

Response:

We thank the reviewer for pointing out the ambiguity in our original formulation. We have removed the "*precursor to shear-driven instabilities*" phrasing, which may have caused confusion and we have revised the manuscript for clarification as

A bright and persistent CNR maximum is observed near 600 m (Fig. 12). This maximum corresponds to aerosol accumulation beneath the capping inversion at the top of the stable boundary layer. Strong temperature inversions can suppress vertical turbulent mixing and limit dispersion, leading to the accumulation of aerosols below the inversion (Zhang et al., 2024). Vertical wind shear due to the change from typical sub-geostrophic wind speeds in the ABL towards geostrophic winds in the free atmosphere above can create favourable conditions for Kelvin-Helmholtz instabilities.

Comment 17

The uncertainties discussed in line 338-343 are critical for DBS or VAD lidar scans, but the results shown are from the vertical stares.

Response:

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

Comment 18

Did the Wind Cube V2 measurements add any value to the analysis of the KHB?

Response:

We thank the reviewer for this question. In the KHB case study, the billows occurred above the maximum measurement range of the WindCubeV2, so this instrument was not used to directly observe the billow structures. However, the WindCubeV2 measurements were used to provide independent wind speed profiles in the lowest part of the marine atmospheric boundary layer (below about 300 m), which helped increase confidence in the mean wind speed profiles derived from the scanning lidar and used in the analysis of the case study.

Comment 19

Fig. 14 only shows results from the vertical stares with the 100s, but the discussion in section 5.2 also mentions wind speed and negative wind speed gradients. Where are those shown? Why are the data from the two lidar systems and different scans with the 100s not analyzed together?

Response:

We appreciate for pointing out this oversight. Now we have added profiles of the mean wind speed recorded by the WindCube V2 and the WindCube100S and NORA3 in manuscript (see Figure 6).

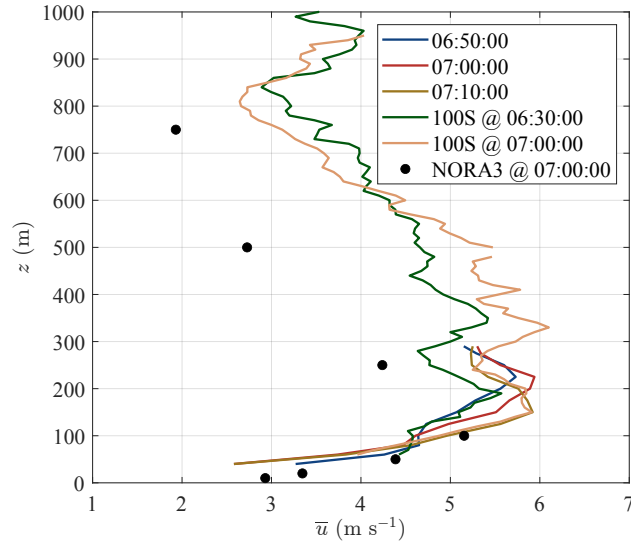


Figure 6: Profiles of the mean wind speed recorded by the WindCube V2 and the WindCube100S on 22 February 2023 between 06:30 and 07:10 in the harbour.

Comment 20

Near line 380 in this text: "Between 06:47 and 07:00, $I_w \approx 0.05$ at 500 m and between 07:05 and 07:12, the turbulence intensity has nearly doubled with $I_w \approx 0.09$ at 500 m. It should be noted that the lidar beam spatial averaging leads to an underestimation of the turbulence intensity I_w by 20-30%." How is I_w computed given the different sampling times of w and u ? How was the underestimation of 20-30% determined?

Response:

We thank the reviewer for raising this point. In the revised manuscript, we no longer use the turbulence intensity I_w and instead analyse the variance of w directly. This avoids the need to combine w measurements from the vertical staring mode with wind speed profiles obtained from the DBS scans and therefore removes the need for temporal interpolation between the two datasets. To better quantify turbulence and its dependency on the time, we added power spectral density (PSD) estimates of the vertical velocity before and after the observed increase in turbulence. The low-pass filtering, i.e., the along-beam spatial averaging, is always present but becomes more apparent when turbulence fluctuations increase. This is because the signal-to-noise ratio also increases with wind speed, thus the

high-frequency measurement noise that dominates at low wind speeds becomes less visible.

Comment 21

In line 396 a skewness profile is mentioned, where is this shown?

Response:

We have added the following text and figure to the manuscript:

The moving standard deviation of σ_w (Fig. 13 c) and the profile of σ_w (Fig. 7 a) show both a local increase of the vertical turbulent fluctuations at heights between 200 and 800 m above the surface. Figure 7 also presents the vertical profiles of kurtosis (b) and skewness (c) for three observational windows (06:35-06:47, 06:47-07:00, and 07:05-07:12), together with the corresponding power spectral densities of w at 430 m. Prior to 06:47, σ_w is relatively uniform between 100 and 800 m, with weak turbulence ($\sigma_w < 0.2 \text{ m s}^{-1}$) between 100 and 700 m above ground level. During this period, the power spectral density S_w exhibits pronounced white noise at frequencies above 0.1 Hz, indicating that turbulence fluctuations are too weak to be reliably resolved by the instrument. From 06:47 onwards, σ_w increases locally near 600 m and subsequently reaches values of approximately 0.5 m s^{-1} between 400 and 600 m. Between 06:47 and 07:00, the S_w spectra indicate improved detectability of turbulent fluctuations, although noise levels remain sufficiently high to partially obscure the effects of along-beam spatial averaging. After 07:05, vertical velocity fluctuations peak near 430 m, and the corresponding S_w spectrum shows reduced noise levels, with along-beam spatial averaging associated with the lidar probe-volume length becoming apparent at frequencies above 0.1 Hz, where the spectral slope steepens relative to the $-5/3$ inertial subrange. The pronounced spatial and temporal variability of σ_w reflects a non-stationary and vertically evolving boundary layer, suggesting the onset of downward transport from the boundary-layer top.

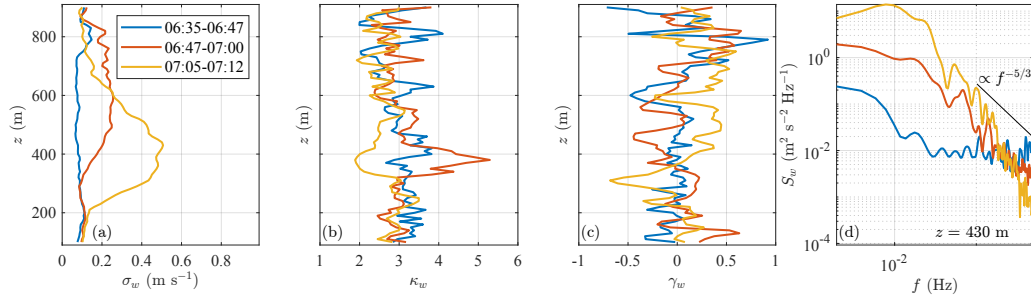


Figure 7: Profiles of the standard deviation σ_w (a), kurtosis κ_w (b), skewness γ_w of the vertical velocity component w (c), and the power spectral density of S_w at 430 m (d). All panels are based on data recorded by the WindCube100S in the harbour on 22 February 2023 between 06:35 and 07:16.

Comment 22

In case study 3 (section 5.3) the measurement location is not described and the data showing the low-level jet are not presented. This is a good example where the paper could be improved by presenting both the u and w observations together.

Response:

We have clarified this as

Case study 3 addresses observations of internal waves conducted in the harbour after the CTV returned from the offshore wind farm [...]

In addition, we have added the vertical profile of mean wind speed (fig. 8 a). This figure shows a low-level jet with its maximum at approximately 400 m. The profile is consistent between both lidars.

Comment 23

How do the spectra in Fig.16 compare to spectra for time periods prior/after the waves?

Response:

We thank the reviewer for this suggestion. To clarify this point, we added spectra for a period prior to the occurrence of the waves (22:05–22:15) and for a later

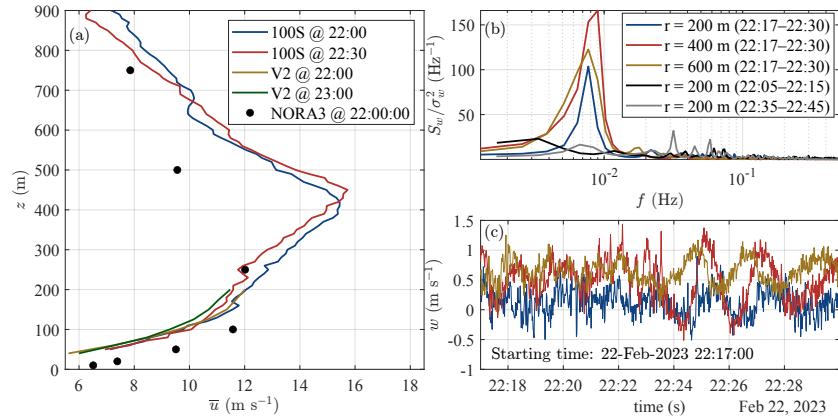


Figure 8: Panel (a): profiles of the mean wind speed measured by the WindCubeV2 and the WindCube100S on 22 February 2023 between 22:00 and 22:35 in the harbour. Panel (b): power spectral densities of the vertical velocity fluctuations from 200 to 600 m above the surface during the wave event (22:17–22:30). The black (grey) solid line shows the power spectral density at 200 m prior (after) to the observation of the waves for comparison. Panel (c): Time series of vertical velocity corresponding to the PSD shown in panel (b) between 22:17 and 22:30.

period (22:35–22:45) at 200 m in fig. 8 b. Before the waves appear, the power spectral density of the vertical velocity shows no distinct spectral peak, indicating the absence of wave activity at both 200 m and 400 m. After the waves develop, the spectrum at 200 m also does not exhibit a pronounced peak, consistent with the time series and suggesting that the wave signal does not extend down to this level. However, clear spectral peaks remain visible between approximately 350 and 600 m during the wave event, indicating that the wave activity is confined to higher altitudes near the nose of the low-level jet. These spectra have been added to Fig. 16 and described in the revised manuscript to clarify the vertical extent and temporal evolution of the wave signal.

Comment 24

Line 441-447: Could NORA 3 data not aid in the stability analysis for this case?

Response:

NORA3 could in principle be used to estimate atmospheric stability, for example through the Bulk Richardson number, as done in [Malekmohammadi et al. \(2025\)](#). However, its temporal and spatial resolution is limited relative to the scales of the processes investigated here. A more robust assessment would require co-located measurements with higher temporal and vertical resolution, such as temperature and humidity profiles from additional instrumentation, which were not available during the campaign. We have therefore not applied this approach and instead added the following sentence to the manuscript:

Moreover, although reanalysis products such as NORA3 could be used to estimate atmospheric stability (e.g., via the Bulk Richardson number), their temporal and spatial resolution is limited compared with the scales of the observed processes. A more robust assessment would require co-located, high-resolution profile measurements of temperature and humidity, e.g. by a passive microwave radiometer ([Westwater et al., 2004](#); [Rose et al., 2005](#)), a Raman lidar ([Lange et al., 2019](#); [Wulfmeyer & Behrendt, 2021](#)), or an infrared temperature profiler ([Knuteson et al., 2004](#); [Michaud-Belleau et al., 2025](#)). To obtain such measurements, a HATPRO RG4 profiler was installed at Rødsand harbour at the beginning of the campaign. Unfortunately, the instrument was malfunctioning, and no usable data could be obtained.

Comment 25

In section 5.4 details are missing about PY Wake (Ref?) simulations and the simulations mentioned in lines 465-467. The model performance should be expanded to provide quantitative information about model performance.

Response:

We have re-written section 5.4 entirely with a more focused study case combining the lidar V2 data and the data from the scanning lidar on the transformer platform and appropriate reference to PyWake ([Pedersen et al., 2023](#)).

Comment 26

Line 505: how was it determined that the V2 showed robust performance?

Response:

We thank the reviewer for this comment.

We have revised line 505 as:

The WindCubeV2 maintained considerably higher data availability during the campaign compared to the WindCube100S (by a factor of about 3.5). The WindCube100S showed a higher rate of scan interruptions, which we attribute not only to its mechanical configuration, specifically the relatively heavy two-axis external scanner head used to adjust both azimuth and elevation angles, but also to its sensitivity to platform motion. In particular, translational motions of the vessel appear to contribute to data loss and incomplete scans. In contrast, the WindCubeV2 uses optical switching without moving parts for beam steering (?), making it more robust under dynamic conditions. For the vertical staring mode conducted between 2023-01-25 and 2023-08-28 (approximately 9900 scans), 65% of the WindCube100S scans were flagged as incomplete (scan duration < 10 min) and excluded, while only 30% reached the full 25-min duration.

Comment 27

Line 508: how many data sets of the 100s are of good quality?

Response:

We have added the following to the line 508:

For the vertical staring mode conducted between 2023-01-25 and 2023-08-28 (approximately 9900 scans), 65 % of the WindCube 100S scans were flagged as incomplete (scan duration < 10 min) and excluded, while 30 % of the scans had a full 25-min duration.

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