

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

1 General comment from Reviewer 1

General Comments. The revised paper addresses my comments and is significantly improved. I am however not convinced that the newly included computation of entrainment rates provides much added scientific value to the paper. While the used approach for computing entrainment rates is a possibly solution of obtaining entrainment from the collected observations, the applied equations are based on a number of assumptions that have not been tested for the complex flows investigated in the paper. I would suggest showing σ_w in Fig. 12 in panel c and omit panel d in Fig. 13. While σ_w (or the variance) does not directly show entrainment, it is a good indicator of turbulent mixing, and the added information provided by the kurtosis, skewness, and spectra provide nice insights about important processes in the ABL and wind farm environment. It would further be good to add information about SR and SS times to the case study discussions, in particular for the 2. case study. In the discussion of this case, the authors often refer to the ABL top. It should be clarified how the ABL top was defined.

Response:

We appreciate the reviewer's positive assessment of the revised manuscript and the constructive suggestions. Regarding the entrainment-rate estimate, we agree that the formulation relies on assumptions that have not been independently validated for the case at hand. We have therefore revised the manuscript and removed this method, as recommended by both reviewers. We have revised figures 12, 13, and 17 as suggested and added the sunrise and sunset times to the second case-study discussion. In addition, we have clarified how the ABL top is defined in section 3.1, as follow

The ABL top is inferred from vertical-stare lidar measurements using both CNR and σ_w profiles when these diagnostics are well defined. Aerosol- and backscatter-based methods are commonly used to estimate ABL height from lidar and ceilometer observations (Caicedo et al., 2017; Vivone et al., 2021), but the resulting height

should be interpreted as a proxy rather than as an exact thermodynamic inversion height. This is particularly important in cloud-topped conditions, where cloud droplets may dominate the CNR signal and the CNR maximum may correspond to a cloud-related backscatter feature that is not necessarily collocated with the actual ABL top. When a distinct local minimum is visible in the vertical profile of σ_w , it is used as an additional turbulence-based indicator of ABL depth, following previous Doppler-lidar studies based on vertical velocity variance profiles: [Tucker et al. \(2009\)](#) used the altitude where σ_w^2 drops below an empirical turbulence threshold, whereas [Puccioni et al. \(2024\)](#) used the local minimum of σ_w^2 under stable conditions.

Authors' Response to Reviewer

Reviewer 2

2 General comment from Reviewer 2

General Comments. Thanks to the authors for revising the paper. The paper has improved, but it still lacks focus and rigor in some respects. The most problematic aspect remains the momentum flux from the lidar. There have been numerous studies that quantified the accuracy of Reynolds stresses measured by the lidar. Some obtained some success with $\overline{u'w'}$ using a line-of-sight variance de-projection (e.g., <https://doi.org/10.5194/wes-10-361-2025>, <https://doi.org/10.1175/2010JTECHA1389.1>, <https://doi.org/10.1063/5.0209729>). All those authors also show extensive validation vs. reference sensors and highlight the inherent limitations due to the lidar acquisition principle, noise, and sampling error. Here the authors propose an approach which seems totally novel for lidar applications but without any metrics for its accuracy compared to, for instance, a sonic anemometer. Moreover, the approach seems to stitch together different classic results from literature sources that are not consistent. E.g., Blackadar's paper is about neutral flow, MacDonald's is stable flow, Mellor Yamada in Eq. 26 uses a more complex formulation for the eddy diffusivity. The assumption of isotropic turbulence throughout the whole boundary layer is also very crude. In substance, if the authors want to propose a novel method to estimate $\overline{u'w'}$ from lidar, they should consider a dedicated campaign and a manuscript. The suggestion is to remove any mention of entrainment or momentum flux and just rewrite this as a data description paper with preliminary results on gravity wave observations and wake validation. The motion correction results are now more compelling thanks to the bug fix and the new figures, but the tilt correction still has some issues (see below). The comments about the lack of wind speed/direction and the use of RHI for wake scanning were partially addressed, but the limitations have been discussed more carefully so can be acceptable for preliminary work like this one.

Response:

We thank Reviewer 2 for this detailed assessment. Following the suggestion of both reviewers, we have removed the proposed new methodology to quantify vertical momentum entrainment. Thus, we no longer present the turbulence closure scheme. We now focus mainly on the moving standard deviation of the vertical velocity component to quantify turbulent mixing, which, together with wind shear, contributes to vertical momentum entrainment.

We have also added a discussion clarifying why the direct estimation of $\overline{u'w'}$ is outside the scope of the present study. The revised manuscript now explains that our approach complements direct Reynolds-stress retrievals from five- and six-beam DBS scans with DWL profilers, in which line-of-sight velocity variances are combined to estimate components of the Reynolds-stress tensor (Mann et al., 2010; Sathe et al., 2015; Letizia et al., 2024; Krishnamurthy et al., 2025). Existing multi-beam retrieval methods have primarily been evaluated for fixed-platform profiler lidars. Their application on moving offshore platforms remains an active research topic because platform motion affects the effective beam geometry, sampling, and uncertainty of the retrieved turbulence statistics. Recent work by Watson & Gottschall (2026) on Reynolds-stress-tensor recovery from a floating five-beam pulsed lidar illustrates the complexity and ongoing development of such methods. As mentioned by Reviewer 2, a rigorous validation against collocated in-situ turbulence measurements would require a dedicated measurement campaign and is therefore beyond the scope of the present study.

However, we respectfully disagree that the references cited in the previous version were mutually inconsistent. We recognize that the previous wording might not have made sufficiently clear how each reference was used. The Blackadar mixing length is widely used for stable, unstable, and neutral flow (Kepert, 2012). The choice by MacDonald & Teixeira (2020) to use $C_\mu = 0.1$ for stable conditions originates from Deardorff (1980), who focused on unstable conditions. The Mellor–Yamada formulation was not used in our original derivation. It was previously cited only as general background on eddy-diffusivity closures, but we recognize that this could have been misleading. Finally, the assumption of isotropic turbulence was already discussed in the previous version and typically leads to a multiplication factor that remains below 50%, without changing the conclusion.

3 Specific comments

Comment 1

Section 3.3: The tilt correction in Section 3.2 is only done for the vertical component, but the horizontal component also suffers from tilt effects and requires a correction. Please revise.

Response:

We agree that tilt can affect all velocity components when a full three-component wind vector is retrieved, as in DBS configurations. However, section 3.3 concerns the WindCube100S operated in vertical stare mode, for which only one line-of-sight velocity is measured. In this configuration, the horizontal velocity components cannot be retrieved from the scanning lidar alone, and therefore no direct horizontal-component correction can be applied to the vertical-stare data.

We have revised the text to clarify that the static tilt correction in section 3.3 applies only to the nominally vertical LOS velocity. Conversely, the effect of tilt on the mean horizontal wind component is expected to be negligible in the present case, since the correction term scales with $\bar{w} \sin \theta$, while $\bar{w} \ll \bar{u}$ and the tilt angle is small $\theta < 3^\circ$. We have updated equations 10-11 and added a few lines on that matter in the revised manuscript.

Comment 2

L372, “This correction does not affect the turbulence statistics,”: This sounds incorrect, because a static tilt angle if uncorrected or not corrected adequately will change the estimated Reynolds stresses. For instance:

$$w'_{tilt} = u' \sin \theta + w' \cos \theta \quad (1)$$

$$\overline{w'^2_{tilt}} = \overline{u'^2} \sin^2 \theta + \overline{w'^2} \cos^2 \theta + 2\overline{u'w'} \sin \theta \cos \theta. \quad (2)$$

Said differently, Reynolds stresses change by changing the axis on which they are evaluated.

Response:

We agree that the original statement was too broad. A static tilt of the measurement axis can affect Reynolds stresses and velocity variances because the fluctuat-

ing velocity measured along the tilted axis contains a projection of the horizontal turbulent component. Therefore, even after removal of the mean, second-order moments may still be affected by the coordinate rotation.

Our intended statement was narrower: the correction applied in this part of the analysis removes the mean bias in the vertical velocity associated with the static tilt. It does not imply that turbulence statistics are invariant under a change of measurement axis. We have revised the text accordingly and now explicitly state that unresolved tilt can contaminate $\overline{w'w'}$ through projection of horizontal turbulent fluctuations and covariance terms. This residual effect is now identified as a source of uncertainty in the turbulence statistics.

Comment 3

Section 5.2: Please add a sentence to justify why the observations are unlikely to be a KHB case based on the comment provided in the response.

Response:

Thank you for your comment.

We believe that the point raised by the reviewer concerns the interpretation presented in section 5.3 (Case Study 2), as the section numbering has changed following the addition of a new section in the revised manuscript. While the revised manuscript previously contained a brief discussion of the rationale for the KHB rollout (Case Study 2, Lines 507–516), we have now revised that paragraph based on our response provided during the first round of revisions, as follows:

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 characterised by a clear narrow-banded spectral peak, a well-organised 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 organised 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. In addition, local increases in kurtosis have previously been used as indicators of coherent KHB structures (Blumen *et al.*, 2001; Malekmohammadi *et al.*, 2025). In the present case, 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, even though σ_w continues to increase. This behaviour contrasts with the persistent kurtosis peak observed during the KHB event reported in Malekmohammadi *et al.* (2025). Taken together, these features suggest that KHB cannot be excluded, but that the event does not exhibit the clear spectral and temporal signatures of a classical, well-organised KHB case; it is therefore interpreted more cautiously as intermittent mixing and entrainment near the ABL top.

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