

# Authors Response to Review (reviewer 1)

Ivanell et al.

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The authors thank the reviewer for the time taken reviewing the paper and the helpful comments. This document specifies the modifications made according to the comments received by the reviewer. All changes are highlighted in a separate pdf for simplicity in parallel to the new (clean) version of the manuscript. In this document, we also comment on and discuss each review point made by the reviewer.

The input of the reviewer has improved the quality of the paper, we thank the reviewer for all contributions.

Sincerely

Authors

## Reviewer 1

This study investigates the impact of atmospheric boundary layer height (BLH) on wind farm efficiency and the influence of large-scale turbulence structures on turbine loads. The authors combine mesoscale modeling (WRF) with lidar measurements to assess climatology and uncertainty across sites in the North Sea and Baltic Sea. They further utilize CFD (code-saturne) for efficiency estimation and aeroelastic simulations (HAWC2/DYNAMIKS) for load analysis. The findings suggest that lower BLH reduces efficiency and that large-scale turbulence increases fatigue loads due to wake meandering.

### Major comments

1. The manuscript in its current form lacks a strong narrative thread. It currently presents as a series of plots/results—climatology, uncertainty, efficiency, and loads—without a clear discussion connecting the physics behind these results. For example, while the results show efficiency differences linked to BLH, there is limited discussion on the physical mechanisms driving these results. The paper would be significantly strengthened by moving beyond a presentation of plots to a cohesive storyline that explicitly links the atmospheric physics (stratification, capping inversion effects) to the observed engineering outcomes (wake recovery, loads).

**#Response#:** We thank the reviewer for this feedback. We have made a major revision of the paper to strengthen the narrative thread and where suitable increased the scientific discussion. To do so we removed a number of detailed parts to the appendix to make the narrative thread more clear.

**2. The authors use BLH as a proxy for atmospheric mixing, yet the CFD simulations for efficiency are run under "conventionally neutral conditions" with varying vertical caps. A low BLH in the Baltic Sea (Site C) is typically driven by stable stratification. Stable boundary layers suppress wake recovery much more than neutral layers with a geometric lid. By simulating these conditions as neutral, the study likely underestimates wake losses and overestimates efficiency for the low-BLH cases.**

**#Response#:** We agree with the reviewer. We have added a more detailed description of this in section 3.4 and 4.4 where we discuss this further and also in the conclusion regarding future work. However, here we are not able to run all possible stratification cases and are limited to CNBL regarding the high fidelity simulations. But the WRF and measurements data include all cases and a discussion now addresses how the farm efficiencies, based on CNBL, should be considered in the overall assessment.

**3. It is unclear if the CFD simulations used for efficiency calculations incorporate the large-scale 2D turbulence structures discussed in the second half of the paper. If they do not, there is a disconnection between the two main analyses. The authors should discuss how the efficiency results might change if the wake meandering effects were included in the efficiency loop.**

**#Response#:** These scales are not included in the RANS simulations so we might expect an increased mixing and therefore higher efficiencies. Nevertheless, it should be emphasized that the important feature to consider in this study is the comparison between the different sites (*i.e.* the relative efficiencies between the sites), not absolute values of the efficiencies. Indeed, the efficiencies are applicable to a given wind farm with a specific layout and specific turbines. They should not be viewed as universal values that can be generalized to all the wind farms at a given location. As a consequence, 2D features not taken into account in the CFD should not be important for the present analysis, except if a strong dependence exists between these features and boundary layer height and/or wind speed which we show that is not the case, see section 5.3 where this has been added to the discussion.

**4. It is unclear why 3DTKE was chosen for the WRF simulations (also given that the introduction only talks about the MYNN scheme instead). The authors should briefly justify the choice of 3DTKE for this specific study and perhaps comment on how it handles the capping inversion compared to MYNN.**

**#Response#:** The WRF configuration is based on an extensive sensitivity study focusing on accurately reproducing LLJ characteristics. Adding this to the WRF model configuration part: A comprehensive sensitivity study was conducted [1] to determine this model configuration with an emphasis on accurately reproducing LLJ climatological characteristics.

**5. The authors assess WRF BLH uncertainty using Lidar data at Site B (a coastal site) and apply this uncertainty to Sites A and C (offshore/marine sites). The assumption that a coastal site represents a "worst-case" or valid proxy for offshore sites like Dogger Bank (Site A) is strong and potentially flawed. Offshore marine boundary layers are driven by different fluxes than coastal transition zones. If no other data exists (what's the spatial extent of the WRF**

simulations?), the authors must acknowledge this as a major limitation rather than treating it as a standard conservative assumption.

**#Response#:** We agree that that statement warrants further discussion and description of potential limitations. We have added this in the discussion section (5.2): A further caveat concerns the transferability of the Site B BLH uncertainty to the offshore sites. Site B is coastal, whereas Sites A and C are marine. These boundary layers are governed by different processes: the coastal BLH and its pronounced diurnal cycle reflect land–sea contrast, fetch-dependent internal boundary layer growth, and heterogeneous surface fluxes, whereas offshore the high thermal inertia of the sea suppresses the diurnal cycle and the BLH is set primarily by air–sea temperature difference and advection, frequently producing shallow persistent stable layers with no coastal analogue. The Site B uncertainty therefore cannot be assumed to transfer to, or to bound, the offshore error: coastal heterogeneity may make Site B a more demanding test of the model, but offshore shallow stable layers are themselves a regime where mesoscale models struggle, and the sign and magnitude of the WRF–lidar discrepancy may differ. All three sites do fall within the same WRF domain, so physics and forcing are identical, suggesting a shared error component despite the differing regimes. Still, absent offshore BLH observations, we treat the application of the Site B uncertainty to Sites A and C as a significant limitation rather than a conservative assumption one motivating dedicated offshore BLH measurements to validate the mesoscale field directly.

## Specific Comments

**The introduction lacks a critical discussion of previous studies on the topic. It lists research questions but does not sufficiently contextualize the current work against existing literature on BLH-wind farm interactions.**

**#Response#:** The introduction has been extended and relevant literature has been added.

**Figure 5: How was the specific period shown in Figure 5 selected? Was it chosen for its particularly good agreement between lidar and WRF? o Is the time axis in UTC? o Please define "HWS".**

**#Response#:** The specific period was chosen randomly to show typical diurnal cycle wind speed regimes with sufficient non-filtered lidar data. "HWS" is defined in the caption. The time axis is indeed UTC, this has been added to the axis label.

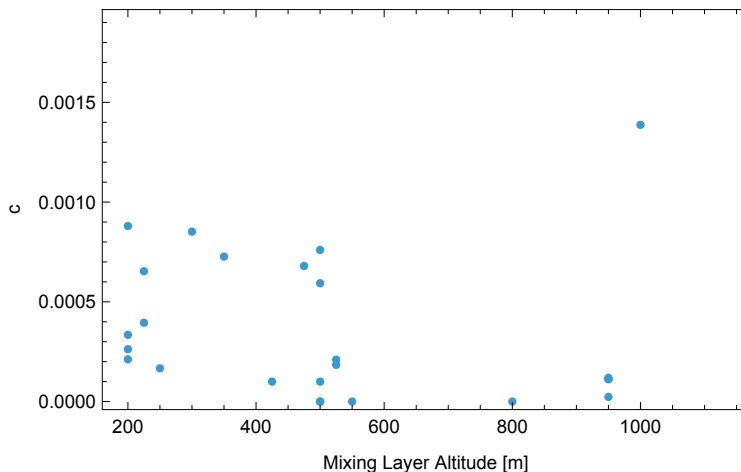
**Section 3.4: When comparing WRF and Lidar BLH, how do the range gate resolution of the lidar and the vertical resolution of the WRF model impact the observed bias? Could the error be an artifact of resolution differences?**

**#Response#:** The lidar vertical resolution is consistently 25 m, whereas the spacing between the time-varying height of WRF's levels grows with height from about 18 m near the surface to 70 m at around 1500 m (44 levels exist below this level). This difference in vertical resolution should not contribute as significantly to the BLH impact compared to the BLH detection method, and their criterion.

**Section 3.5:** The load analysis regarding wake meandering appears to be analyzed at a single mean wind speed. Given the paper’s emphasis on climatology in the first half, limiting the load analysis to a single wind speed feels restrictive. The authors should expand the discussion on how sensitive these load increases are to wind speed variations (e.g., would the meandering effect diminish at rated power?).

**#Response#:** The load analysis is on purpose chosen at a wind speed where the impact is expected to be the largest, i.e. below rated. A thorough investigation of the full impact of low-frequency turbulence is beyond the scope of the paper, as it would require numerous additional calculations, including all wind speeds up to the rated value and all wind directions. However, we do now have load analyses for tower-base fore-aft, blade root flapwise moment and mooring line tension at three distinct velocities: below-rated, rated, and above-rated, in what is now figures 9, 10 and 11 (and were 15, 16, and 17 in the original submission). We have conducted a detailed investigation into the dynamic response and fatigue loads (including tower fore-aft and blade root flapwise moments) over the full range of wind-speed variations (from cut-in to cut-out) in a companion study ([3], accepted to be published). We have added a reference to this companion paper in Section 4.6.

It would be great to link the paper better together by linking the low-frequency turbulence (the parameters  $c$  and  $\psi$ ) with the boundary-layer height. We have tried that, but the results are inconclusive, or indicate no relation between BLH and  $c$ . We took the parameters for the coherence runs analyzed in [2] and plotted them versus concurrent BLH estimates from a ceilometer:



This shows no clear relation between  $c$  and the BLH (= Mixed Layer Altitude). We have now mentioned in the manuscript in appendix A1 that preliminary investigations show no clear relation between low-frequency parameters and the BLH, but we need analysis of more data before we can draw any definite conclusions.

## Technical Corrections

Throughout the manuscript, please add a space between the number and its unit (e.g., "150 m" not "150m") and between unit components (e.g., "m s" not "ms").

**#Response#:** Thanks for noticing this, this has been corrected throughout the paper

**Use exponential notation for units consistently, in accordance with WES standards.**

**#Response#:** Thanks for noticing this, this has been corrected throughout the paper

**Tables 2–5: Remove the dot in the unit.**

**#Response#:** This has been corrected.

**Table 6: The number of decimals provided is excessive. Suggest reducing them to make comparisons easier.**

**#Response#:** This has been updated

**Table 7: Please add units for "Average of Bias" and "RMSE".**

**#Response#:** This has been updated

**Figures 11, 12, 14: Please add where appropriate.**

**#Response#:** This has been updated.

**L. 74: Remove the parentheses around the reference.**

**#Response#:** This has been updated

**Please add DOIs to all references where appropriate.**

**#Response#:** We have updated the reference list to include DOIs where appropriate

**The reference list contains at least one "Discussion" paper that has been published. Please update these to the final published versions.**

**#Response#:** Thanks for noticing, we have updated the status for relevant papers.

## References

- [1] B. T. E. Olsen, A. N. Hahmann, N. G. Alonso-de Linaje, M. Žagar, and M. Dörenkämper. Low-level jets in the north and baltic seas: mesoscale model sensitivity and climatology using wrf v4.2.1. *Geoscientific Model Development*, 18(14):4499–4533, 2025.

- [2] Ansh Patel, Jakob Mann, Mikael Sjöholm, Gunhild Rolighed Thorsen, Abdul Haseeb Syed, Elliot Irving Simon, Lin-Ya Hung, and Julia Gottschall. Lidar observations of turbulence for tall offshore wind turbines. *Journal of Fluid Mechanics*, 1028:A9, 2026.
- [3] A. H. Syed, Á. Hannesdóttir, and J. Mann. Dynamic response and loads analysis of a large offshore wind turbine under low-frequency wind fluctuations. *Wind Energy Science Discussions*, 2026:1–22, 2026.