

Authors Response to Review (reviewer 3)

Ivanell et al.

June 30, 2026

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 3

0.1 General comments

“Impact of Boundary Layer Height and Large-Scale Turbulence on the Efficiency and Loads of Offshore Wind Farms” aims to address two questions: **How does boundary layer height affect wind farm efficiency? And how does turbulence of different scales affect turbine loads? While both questions are important, the manuscript should undergo major revisions to improve the clarity of the methods and results. Furthermore, the assumptions and methods require some additional justification to strengthen the findings. See specific comments and technical corrections below.**

0.2 Specific comments

Page 2: The uncertainty of model BLH is discussed here, but the uncertainty of calculating BLH from experimental data is not addressed. The definition of BLH from experimental data is still an open question, especially under stable conditions (see, e.g., Jozef et al. 2022).

#Response#: We agree the uncertainty of BLH from experimental data is a widely discussed topic in the meteorological community. It proves difficult to find a true reference for both stable and unstable conditions. For this application, the BLH here is not seen as a true reference but rather a comparative value to the WRF BLH. The lidar BLH is defined as the mixing layer height based on turbulence gradient, which is the closest link to the BLH derived from WRF. The lidar provides on-site, local insight to the coastal BLH variation compared to the 1km resolution output from WRF. This has been included in the discussion.

Page 4, Figure 1: Are sites b and d co-located? It is difficult to distinguish the two symbols on the map. An inset of that region could help clarify their locations.

#Response#: The figure has been updated with an inset.

Page 4, Section 2.3: The reader is referred to Ivanell et al. (2025) for details of the numerical setup, but some information should be provided here to provide a framework for the reader. In particular, how is boundary layer height defined in the model?

#Response#: This has been added in Appendix B on *code_saturne* setup

Page 5, lines 113-114: The authors describe sites b and d as having “offshore conditions undisturbed by topography without the need to be far away from shore. ” Has this claim been demonstrated?

#Response#: We here consider Westerly flow directions and since Denmark is rather flat we consider this assumption to be reasonable but have added a discussion about this assumption in the manuscript, see section 5.2.

Page 5, lines 121-123: As mentioned above, the definition of BLH from lidar data is non-trivial. Please elaborate on the method.

#Response#: The method to derive the BLH from the lidar comes from an integrated Vaisala tool applied to WindCube Scan data. Further elaboration from what is already described in [1] unfortunately cannot be disclosed.

Page 6, lines 131-133: These results are discussed before the results from FINO1. Please either rearrange the discussion, or remove the references to FINO1.

#Response#: This has been rearranged.

Page 6, line 134: This line references “frequencies below 10-3 Hz, but frequency information is not included in the Figure.

#Response#: Since we do not actually show the spectra, we only describe them in words. It would require too many figures to show representative fits. We now state in the text that the actual spectral fits are not shown.

Page 6, Figure 2: This figure is not explained sufficiently. What information is the left panel adding?

#Response#: This figure is now in appendix A. we have added text describing in more detail what is observed in the left plot of the figure showing the parameter c as a function of stability and wind speed.

Section 2.4: It would be much easier to compare turbulence characteristics for the sites if the same information were shown in the plots in each subsection. If it is important to show c , $\alpha\epsilon^{2/3}$, Ψ and Γ , then these plots should be included for all four sites discussed, along with a description of the physical meaning of each parameter. If only a subset of these parameters are needed, then please provide the corresponding plots for all four sites

#Response#: Some of the sites are analysed in other paper, and others are analysed for the first time in this paper. It would be repetition from other papers and require too many plots to show parameters from all sites. Instead, we have added a sub-section after the discussion of all the individual sites that summarizes the finding from all sites with emphasis on the two parameters c and ψ that describe the low-frequency part of the spectrum.

Section 2.5: Not enough information is included in this section. In particular, how do the authors justify the use of a single measurement site to characterize the uncertainty over such a large region?

#Response#: We have added a section on this in 3.6.

Page 10, Figure 5: Please include some discussion comparing measured and modeled BLH, as shown in this figure. In particular, the lidar doesn't appear to capture the shallowest boundary layers. How will this affect the results?

#Response#: As previously mentioned, the BLH is a difficult variable to obtain a reference point. The lidar BLH is defined by the vertical turbulence gradient, which may not capture all the processes as the PBL scheme from WRF. For wind farm applications, however, the BLH defined by turbulence represents the most impactful zone in the boundary layer for wind farm applications. This has been included in the corresponding section within the Discussion.

Page 11, lines 212-213: These two sentences are unclear. Are the authors arguing that lower wind speed show a stronger dependence on BLH? Does "most extreme BLH" mean "shallowest BLH"?

#Response#: We believe that the reviewer here refers to line 223-224. With extreme we here mean most shallow BLH, the manuscript has been updated accordingly.

Page 13, lines 227-228: The meaning of this sentence is unclear. Please re-write.

#Response#: The text has been updated.

Page 13, lines 232-233: Based on the plots shown in Figure 8, the results are extrapolated extensively. Why not run cases with a wider range of parameters?

#Response#: We acknowledge that this adds a large contribution to the uncertainty. Across the 3 sites (A, B, C), the efficiency envelope covers about 40% of events, meaning these can be interpolated inside the existing efficiency results. But this also means that 60% of events are extrapolated. We found this range to be most of interest and due to limited simulation capacity we did not run additional cases. We have added a discussion on this in the manuscript.

Page 14, lines 237-238: The “Cp and Thrust methods” are not explained or described.

#Response#: The results are based on power calculated by $T \times U_d$ and the corresponding explanation has been updated and the preliminary results based on Cp has been removed.

Page 14, lines 243-244: Why is the trend clearest in spring and summer periods? Is it related to seasonal changes in the diurnal cycle?

#Response#: This is because larger extent of stable conditions at some of the sites during this period.

Page 15, line 256: Why is the dataset “limited to fulfill the winter and summer season”?

#Response#: This line has been clarified.

Page 16, line 263: How is Figure 11 similar to Figure 6?

#Response#: This was a mistype, as has been corrected.

Page 16, line 267: How are the first and third quantiles of the BLH bias defined?

#Response#: The first and third quantiles of the BLH bias are the bias at 25% and 75% of the distribution. This has been included in the text, now in the appendix.

Page 16, line 268: What does it mean that the quantile values were “applied” to the bin center?

#Response#: The quantile values were subtracted/added to the bin center value depending on the quantile value. This has been clarified in the text, now in the appendix. 3

Page 18, Figure 13: Why are both the upper and lower BLH estimates subtracted from the bin center? Shouldn't the upper estimate be added?

#Response#: This is correct, the figure has been corrected.

Page 19, Table 8: Please add more description to the table caption. “Uncertainty performance metrics” is not very specific.

#Response#: This has been added.

Page 19, Section 3.5.1: Why is there no discussion comparing the three different types of turbulence shown in Figure 16?

#Response#: Section 3.6.1 (previously Section 3.5.1) and the description of Figures 8 and 9 (previously Figures 15 and 16) are now updated. It now contains a brief description of the three different types of input turbulence fields used in the aero-elastic simulations. A reference is also added in this subsection, which refers to a more detailed published study on the load simulations.

Page 19, Section 3.5.2: The beginning of this section should be moved to the methods section.

#Response#: This has been rearranged

Page 20, line 310: How is “wind turbine outside the wake flow” defined? Is this the same as front row turbines?

#Response#: Yes. The two front rows of turbines are referred to as the wind turbines outside the wake flow. This has now been added to the description. See Appendix D2.

Page 24, lines 330-332: Please use more specific wording in these sentences. The meaning of “the variation in impact” and “the influence on the impact variation” is not clear.

#Response#: These lines has been removed in the updated manuscript.

Page 24, lines 334-338: Again, please be more specific. What uncertainty is described here? What are the units? The results of what at site B have a small overestimation?

#Response#: This part has been rewritten

0.3 Technical corrections

Page 2, line 51: It looks like there’s a typo or word missing in this phrase: “offshore BLH radiosonde from a ship campaign” .

#Response#: This has been updated.

Page 9, lines 206-207: It would be clearer to say: “The distributions show lower BLH occurring more frequently at site B than site A... ”

#Response#: The text has been modified.

Page 14, line 242-243: It would be clearer to say: “...the farm blockage varies between the sites, where sites with a larger share of stable conditions have lower efficiency. ”

#Response#: This part has been reformulated in the updated version

References

- [1] C. Mulet-Benzo, A. Black, and J. Gottschall. Comparing atmospheric boundary layer heights from vertical profiling scanning lidars to ERA5 and WRF. *Wind Energy Science Discussions*, 2025:1–30, 2025.