Authors Response to Review (reviewer 2)

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 2

0.1 General comments

The authors examine 5 o shore sites in Northern Europe to establish a connection between boundary layer height (BLH) and wind farm efficiency. The study, conducted using both WRF mesoscale simulations and Lidar observations (for one of the sites), reveals higher wind farm efficiency for strong wind and high BLH, as opposed to lower BLH and/or wind speed. Additionally, turbulent large scales typically present in o shore flows are shown to have an impact on fatigue loads beyond the temporal duration typically considered by fatigue design standards. The topic is of primary interest both for WES and for the wind energy community in general, given the ongoing growth of o shore wind turbines, both in size and number.

Before recommending this article for publications, a number of major points should be addressed. On a high level, I had the following impressions reading the manuscript:
Some details regarding the technical approach are overseen and left to the reader's intuition (see, e.g., lines 235-239).

#Response#: We thank the reviewer for this feedback and agree that parts need a more developed description. We have made a major review of the paper and included a more detailed description where suitable.

The authors are over relying on previous publications when it comes to provide insights on the technical approach. The details about what is done in this article should be clear just by reading this article. More interested readers can find more information by looking at previous publications, but numerical simulations and experimental setups should be described here in

more detail.

#Response#: We have added more details about the numeral and experimental setup. See e.g. appendix B and C for numerical setup and uncertainty study.

The technical details are provided in a quite scattered fashion. Some details regarding Lidar scanning strategies are left to the Results section (lines 250-254, for example), while some results are described in the Methodology section. I suggest devoting one sub-section to numerical simulation details and another one to experiments.

#Response#: The details on the lidar scanning strategy have been moved to the methodology section.

0.2 Specific comments

Lines 62 – 64: The approach proposed by the present study is a bit confusing at this stage of the manuscript. Please clarify further how you map different atmospheric conditions and how you connect them to climatology.

#Response#: We have restructured the manuscript to better explain the methodolgy

Lines 102 – 104: The presence of large-scale turbulent motions is already a well- documented feature of o shore environments (see, e.g., ref.), so reiterating this point in the present study is not necessary unless it is better specified for the present scope.

#Response#: We believe it is important to introduce the current status and reference to this before describing what is added in this study at this point in the manuscript.

Lines 114 – 117: These lines could be nicely integrated with a figure reporting the position and scan direction of each Lidar.

#Response#: We thank the reviewer for this good suggestion, we have integrated this in ti figure 1.

Line 135: Which parameter and value are the authors utilizing to define neutral atmospheric conditions?

#Response#: We have utilized Obukhov Length (L) to define the atmospheric stability classes for measurements from Høvsøre and FINO1. An explanation is added in Appendix A2.

Line 149: The necessity of utilizing both 2D and 3D spectral models is still unclear. Also, it appears that ψ and Γ exhibit significantly different behaviors, i.e. ψ decreases with velocity and shows little sensitivity to thermal stability, while Γ increases with velocity and shows distinct values according to different stability classes. Please clarify.

#Response#: The results of the load calculations in offshore wind farms show that it matters which turbulence models to use. We believe the 2D+3D model is more realistic and we therefore propose that it is worthwhile to investigate further in order to get more realistic load estimations on offshore wind farms. We have included more discussion of this point in the sub-section ”Load assessment methodology”.

The reviewer is right in noting the parameters depend in different ways as functions of wind speed and stability. The exact dependencies, or even if the parameters are the best way to describe turbulence, is not yet entirely clear. One clear weakness is that all turbulence parameters are obtained from a single sea, The North Sea. In order to shed more light on the question raised by the reviewer, we need more data sets from different offshore regions. We have included these considerations in the Discussion.

Lines 159 – 160: Similar to the discussion about Mann's spectral model, please report the explicit formulation of 2D and 3D spectral models, in addition to a brief description of their parameters. Also, at this stage the rationale of using two different spectral models for the same dataset is unclear.

#Response#: We have described the parameters in more detail in the sub-section ”Load assessment methodology” and addition to a motivation for the use of the 2D+3D models.

Line 164: Since the authors mention climatology assessment, I expect the measurements to cover a multi-year period. For FINO1, this period corresponds to 2 years (if I understood correctly), which is reasonable. Please report the analogous measurement periods related to the remaining sites.

#Response#: This has been added to the measurement campaign descriptions

Section 3.1: In my impression, this section is a repetition of what is already displayed in Fig. 5 and 6 without any real insights of physical phenomena explaining the observed differences at sites A, B and C. Please either provide more insights or remove this section.

#Response#: We here introduce and explain what are seen in figure 5 and 6 an believed it is important to refer to what the figures show.

Line 204: Is the time local or UTC zone? Also, which method(s) did the authors use to evaluate BLH from Lidar data?

#Response#: The time is in UTC zone, this has been added to the axis label. The BLH derived from the lidar data is based on a gradient method applied to the vertical velocity turbulence. A reference to the method by mulet-benzo$_{c}omparing_{2}025hasbeenaddedtothemaintextandappendix$.

Line 205 – 206: The peak of occurrence in Fig. 6a-6c seems to occur during daytime convective conditions, since it's characterized by low wind speed and high BLH. There should also be a secondary peak related to stable night-time conditions featuring low BLH and high wind speed. Please provide more context for this.

#Response#: We thank the reviewer for this observation that is a good additional decription to the figure and has been added. The secondary peak is however limited in this case with all seasons but more visible considering the seasonal variations, see Appendix E.

Line 219: From Fig. 7, it appears that the overall wind farm efficiency is very close to 1 (and even greater than 1 from Tab. 2, 3, 4 and 5), thus meaning nearly zero under-production from the second to the last row (cf. Eq. 1) and zero blockage. This seems a contradiction with existing literature. Please provide further clarification.

#Response#: Efficiency close to one occurs for very high speeds meaning that all turbines (even the waked one) operate above rated. This is not in contradiction with previous literature. For the cases where the turbines are operating below or close to rated wind speed the efficiency is lower, and perhaps more comparable with the cases you refer to.

Figure 7 and Tables 2 – 5: Please revise the unit on the x-axis (m.s-1).

#Response#: Units for wind speed have been updated in figure and tables in the revised manuscript.

Line 233: I am a bit skeptical about the extrapolation of the efficiency outside of the simulated bounds, especially since it yields values larger than one for high BLH and wind speed. Perhaps you could utilize a subset of the simulated points and extrapolate on the remaining ones to assess the accuracy of this method.

#Response#: We acknowledge that this is 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 have added a discussion on this in the manuscript.

Lines 237 – 238: There are a few caveats with this statement. First, please provide more details on the calculations of power from Cp and thrust (did you use Cp and Ct curves?); second, the length of the climatology period (5 years) has not been stated previously, and it should be reported where the simulation setup is described. Third, possible discrepancies between thrust and Cp -based estimates of power should be discussed.

#Response#: We do use the Thrust and we have have removed the preliminary C_p based results from the figure. The description on the use of the climatology has also been extended.

Lines 239 – 240: Please revise this sentence as it does not sound grammatically correct.

#Response#: The sentence has been updated.

Line 240: A1 - A4 are not appendices, they are figures listed in Appendix A without neither

context nor explanatory discussion. This falls below the standard of acceptable scientific publications. Please either add text to Appendix A or remove it.

#Response#: This has been updated and the figures in the appendix are now properly discussed, thanks for point this out.

Lines 242 – 243: This sentence is hard to understand. How do the authors quantify stable conditions? Also, the farm blockage typically refers to the slowdown exerted by the entire wind farm on the flow upstream, whereas here it seems like the authors are using the term "blockage" to refer to wake interactions or single-turbine blockage.

#Response#: We have modified the description to make it easier to follow. We here use the term blockage for the non-local efficiency with is impact of the entire farm, i.e., not the wake interaction or single turbine induction.

Line 244: According to previous statements, the total efficiency is the product of local and non-local effects, not their sum. Please revise.

#Response#: Thanks for noticing this, that is now updated

Table 6: This table is not mentioned in the text; hence I suggest either remove it or add it to the discussion. Also, it makes little physical sense to report efficiencies with 6 decimal digits, unless the reported values are equal up to the 5-th decimal digit.

#Response#: We have modified the table with reduced number of decimals. We have also moved it to the appendix since we believe is a valuable to have the explicit numbers for comparative studies.

Lines 250-254: Again, this information pertains to the description of the experimental campaign, not the Results section. Additionally, identifying the mixed layer height with the BLH is valid only for daytime well-mixed atmospheric conditions. How did the authors treat night-time stable conditions?

#Response#: The description of the measurement campaign has been moved to the methodology. We agree that the mixed layer height is valid for daytime, convective conditions where the boundary layer can be defined by dominant turbulent processes. The algorithm does not produce BLHs when there is no vertical turbulence gradient, so very stable conditions may not have been properly represented. We have acknowledged this in the discussion.

Line 256: Table 7 reports data from February through June, so it does not fully cover winter and summer.

#Response#: This has been clarified in the text.

Table 7: Is the Average of Bias a percentage or is it reported in meters? Please clarify.

#Response#: It is reported in meters, this has been clarified in the text.

$$\eta \approx f(BLH, U) * BLH \tag{1}$$

However, I also expect a bias between the WRF and Lidar-derived values of wind speed. Assuming Gaussian distribution of BLH and wind speed bias, the previous equation becomes:

$$\Delta\eta \approx \frac{\partial f}{\partial BLH} * \Delta BLH + \frac{\partial f}{\partial U}\Delta U$$
(2)

,where quantities indicated by Δ indicates the interquartile difference. If the wind speed bias is negligible, please report literature references or, otherwise, please quantify it.**Line 269: This is a crucial passage of the uncertainty quantification that deserves more in- depth analysis. The authors utilize the values plotted in Figure 11 as a transfer function ($f(BLH, U)$, where U is the horizontal wind speed) to map the BLH bias (WRF vs. lidar) into wind farm e iciency uncertainty, thus relying on the equation:**

$$\eta \approx f(BLH, U) * BLH \tag{3}$$

However, I also expect a bias between the WRF and Lidar-derived values of wind speed. Assuming Gaussian distribution of BLH and wind speed bias, the previous equation becomes:

$$\Delta\eta \approx \frac{\partial f}{\partial BLH} * \Delta BLH + \frac{\partial f}{\partial U}\Delta U$$
(4)

,where quantities indicated by Δ indicates the interquartile difference. If the wind speed bias is negligible, please report literature references or, otherwise, please quantify it.

#Response#: We don't fully understand this comment. The purpose of Figure 11 is to separate the uncertainty of the wind farm efficiencies by binned distribution of PBLH and wind speed as it relates only to WRF, not the lidar. Both the PBLH and wind speed axis bins are based on the values derived from WRF to apply this uncertainty directly to only modeled values.

Line 275: The largest uncertainty of the wind farm efficiency is found for relatively low BLH values, which correspond to night-time conditions. I would be cautious in considering Lidar-

derived BLH as ground truth for stable, night-time conditions. A significant portion of the observed BLH bias could be introduced by the instrument and, thus, not related to WRF modeling uncertainties. I wonder how the Lidar-derived BLH uncertainty during nighttime compare with the total uncertainty.

#Response#: We agree that it is difficult to trust lidar derived BLH for extremely stable conditions, especially given that the minimum range of the lidar is 100m. This limitation has been included in the discussion. The spread of the data as seen in figure 1 however, shows the majority of boundary layer heights above 200m, showing that the lidar is capable of detecting low boundary layer heights.

Whether they are representative of true BLH in stable conditions requires further research. A separate analysis using radiosondes to better characterize stable conditions and BLH would be interesting to compare with lidar retrievals.

References

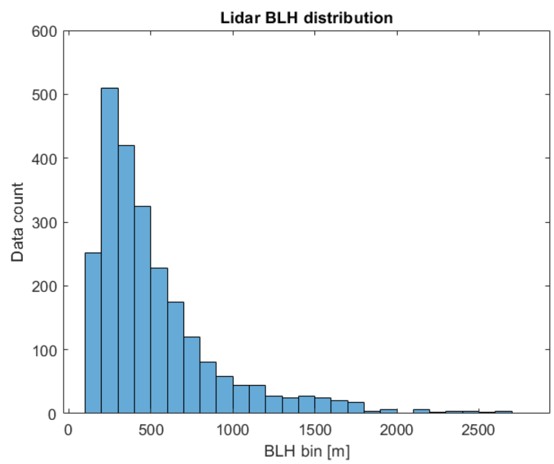

Figure 1: Histogram of lidar BLH retrievals for Trans campaign.