Authors response on the reviewers comments on

Low-level jets' influence on the power conversion efficiency of offshore wind turbines

Johannes Paulsen, Jörge Schneemann, Gerald Steinfeld, Frauke Theuer & Martin Kühn

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Dear reviewers,
we would like to thank you for the positive and constructive review which resulted in a considerable improvement of our manuscript. We have taken your comments into careful consideration and implemented according changes into the manuscript. Below we have listed detailed answers to your comments. The reviewer comments are depicted in black, while our answers are written in blue and phrases reformulated for the revised manuscript in green.

1 R1 Review Comment #1

All changes recommended by the reviewer have been implemented into the revised manuscript. As the remarks are purely technical and grammar related, we do not mention them one by one here, to keep this document concise.

2 R2 Review Comment #2

General comments:

My major concern in this version of the manuscript is the lack of explanation and validation of what the authors refer to as "volumetric approach" of data retrieval. The methods section (around lines 195-200) mentions spatial averaging of wind speed measurement across the entire range of the scans (9 km) and the entire azimuth sector, but the validity of this approach is not discussed enough. While negligibility of vertical wind velocity component is a more or less established convention, homogeneity of the wind field over such a large distance is not self-evident. The analysis would benefit from an assessment of this assumption. Has such approach been used in any other studies reported in the literature? I see that in the discussion section the authors report a comparison of their data with a wind turbine's operational data and with Visich and Conan (2025)'s "metmast method". However, these comparisons are only mentioned briefly with no supporting evidence in the form of scatter plots or other reasonable visualization. A rigorous validation study similar to what the authors presented in Appendix A for SST and atmospheric stability is much needed for the wind speed & direction analysis, especially given that this is the actual core of the work. It would also be beneficial for the overall clarity to emphasize (e.g. in the conclusion) that the study only accounts for the LLJs in the incoming winds for a particular wind farm and does not reflect the total occurrence of

LLJs in a location.

To obtain averaged vertical wind profiles for LLJ detection we use the entire lidar scanning volume spanned by the multi-elevation PPI scans. The applied VAD algorithm assumes a horizontally homogeneous wind direction for each range gate of the PPI scans. Local variations in the wind direction can not be resolved by the fit and we obtain a "fit-averaged" wind direction. Since we are working on a time scale of ten minutes (600 s), we are not interested in the small scale dynamics of an inhomogeneous wind field but in the average vertical wind profile representative for the Nordergründe location. The approach to use data from a large measurement volume will inherently produce a mean wind profile in the regarded area, to capture effects persisting on a wind farm scale. This reasoning is now also mentioned at in L.214 of the revised manuscript:

"As we are interested in LLJ phenomena, which are present on spatial scales encompassing the entire wind farm and temporal scales of 10 minutes, this spatial averaging reduces noise in the data, i.e. short term and small scale turbulent fluctuations in the wind field. This allows for a more robust detection of persistent LLJs."

To provide a more robust explanation on why we view the volumetric approach for wind profile generation as suitable for our study, we slightly adjusted our methods section, extended the discussion and also added a third appendix to the manuscript, comparing the volumetric approach to profiles generated at three "virtual met masts" (VMasts) generated from the same lidar data set and to wind speed and direction measurements from the nacelle of NG17.

Additionally to the described changes to the manuscript, we performed further analysis, dividing the data based on the prevailing stability regime and also considering different points within our measurement sector. Both approaches yield very similar results to the analysis shown in the manuscript with small deviations between volumetric and localized approach, as well as high correlation coefficients.

To consider your comment about the classification of our results and emphasizing that they are only valid for a certain wind direction sector at one location, we have adjusted the following passage in the conclusion:

"We observe LLJs based on our volumetric wind profile between 2.4% and 22.6% of the investigated time for undisturbed inflow towards the wind farm from wind directions between 100° and 320° depending on the used definition."

Other remarks:

1. Line 144: please specify how many scans were performed per measurement
   Within each set of multi-elevation lidar measurements, one scan is carried

out for all 16 different elevations. So, 16 scans in total per measurement. This information has been added again to the text, to clarify further.

2. Table 1 in Elevation angles: please specify in the text/caption that -0.2 stands for the single measurement outside of the range (it's not very evident)
   To improve clarity, the information has been added to the manuscript. The added sentence reads as:

   "The first scan is measured with a negative elevation angle of -0.2°, followed by 15 scans with elevations ranging from 0° to 2.1° in steps of 0.15°. "

3. Line 184: please elaborate on why these measurements are uncertain
   The reason behind the increased uncertainties with these measurements lies in the fact, that when measuring almost orthogonally to the wind direction the line-of-sight component of the wind gets extremely small and hence, the relative error in line-of-sight velocity and the impact of the wind component perpendicular to the main wind direction increase. The following passage has been added to the manuscript to improve clarity:

   "Due to the very small wind component in the direction of the line of sight of the lidar the uncertainty in the reconstructed main wind component is high at these angles, as the relative error in the measurement itself, as well the impact of the wind component perpendicular to the main wind direction are increased."

4. Figure 2: consider marking range gates in subfigure (a)
   When designing the revised manuscript, we tested the inclusion of range gates in the visualisation. However, due to the small distance between the gates compared to the overall range of the lidar, they are no longer distinguishable in the Figure. Thus, we excluded this in the revised manuscript.

5. Eq. 6: please explain the abbreviation. I guess PO stands for power output and TI for turbulent intensity, implying that the fluctuations are due to turbulence, but it's not very clear from the text.
   Exactly, the variable is computed similarly to the turbulence intensity (TI) in wind speed. A more elaborated explanation is included in the manuscript:

   "In addition to the mean apparent power μ_P we also quantify the normalised power fluctuations, i.e. power turbulence intensity ($\mathrm{PO_{TI}}$) similar to the turbulence intensity (TI) in the wind speed as [...]"

6. Lines 308-309: please summarize and/or visualize the proportion of profiles discarded at the different stages of data preprocessing
When filtering the profiles for our analysis, we discard 27.11 % due to the wind direction filter, and an additional 3.31 %, due to bad data quality in the scans/wind profiles. The corresponding numbers have been added in Section 3.1 of the revised manuscript:

"After excluding measurements with wind farm wakes inside the lidar scanned area (approx. 27 % of the data) or insufficient data quality (approx. 3 % of the data), we apply the LLJ detection algorithm."

7. Line 328-329: consider replacing "our measurements" with "the lidar measurements" to make a clearer distinction between the lidar data (within a restricted wind direction sector) and the nacelle anemometer data (presumably unrestricted). Also, were the nacelle data somehow filtered? Could it happen that the lower wind speeds from north and east are due to the turbine wakes?
The term has been rephrased according to your suggestion. Regarding the second part of the comment, no, the nacelle data was not filtered. Thus, in the wind rose also wake measurements are included. For the analysis however, the wake influenced measurements are filtered out.

8. Figure 6: does 0.2 correspond to the smaller circle in the map and 0.3 to the bigger one? The labels are slightly off.
Yes, the labels belong to the two circles. The figure has been recompiled for better accuracy.

9. Figure 7: wouldn't it make more sense to use local time (CET) for this graph and not UTC?
As the majority of our datasets are recorded with UTC timestamps in a coherent way, we decided to use this reference time. This also avoids dealing with time skips and/or double time steps due to daylight savings. Only the turbine operational data is recorded in CET and was transferred to UTC time for the analysis.

10. Figure 9: N refers to LLJ counts? Also, consider adding some white space between the subfigures for better readability.
Yes, N stands for the number of LLJ core heights counted at the specific height. The caption has been adjusted for a more accurate description. Also, we added a little bit of whitespace between the Figures for increased readability.

11. Lines 401 to 408: the descriptions seem to be mixed up. The second paragraph should probably refer to Figure 13b (not 13a) and describe veer, not shear (line 407).
Yes, there was a typo in the manuscript, which has now been rectified.

12. Line 409: please add what TI stands for
    The abbreviation TI has already been introduced in Section 2.5. However, due to the large gap between the mentions, we introduce the abbreviation here again.

13. Line 469: the sentence adding with "... conclusions about this region" – meaning "this region only", as opposed to other regions?
    Yes, the measurements carried out allow conclusions for that specific region only. For the analysis presented in our study, we bin all cases based on the near-surface stability. To improve clarity, that we use this estimate in our analysis, the sentence has been adjusted to:

    "Further, as we measure temperature differences between the TP of the turbine and the sea surface, we bin our data based on the near-surface stability estimate, retrieved from the measurements."

Small issues:
The small comments raised by the reviewer have all been considered and implemented in the manuscript. For brevity, they are not listed here.

References

Visich, A. and Conan, B.: Measurement and Analysis of High Altitude Wind Profiles over the Sea in a Coastal Zone Using a Scanning Doppler LiDAR: Application to Offshore Wind Energy, Ocean Engineering, 325, 120 749, https://doi.org/10.1016/j.oceaneng.2025.120749, 2025.