Authors response on the reviewers comments on

"Low-level jets influence power efficiency of offshore wind turbines"

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Oldenburg, September 15th, 2025

Dear Dr. Cheynet, dear Referees,

Thank you very much for your thorough reviews and constructive feedback and recommendations regarding our manuscript. We've taken all of your comments into consideration and, if applicable, implemented according changes into the manuscript. Below you can find detailed answers (in blue and italics) to your comments (in black).

EC Editor comments

EC1 The literature review appears to be incomplete or at least selective. The work of Gutierrez et al., Murphy et al., and others on low-level jets (LLJs) is noteworthy and may be directly relevant to the present study. The manuscript could better establish its novelty by more clearly identifying the knowledge gap relative to prior research. The current self-citation rate is approximately 20%, which further suggests that the literature review would benefit from broader coverage.

Thank you for the suggestions, where applicable, we incorporated a more thorough literature review including the recommended papers on turbines' nacelle and tower motions (Gutierrez et al. 2017), as well as turbine response to different shear and veer situations (Murphy et al. 2020). Furthermore, we took this comment as an opportunity to update the literature review. Next to the suggested references, we also added information on Frontal LLJs and subsequent downward ramps (Browning and Harrold, 1970; Baki et al., 2025), as well as LLJ detections at the US east coast from lidar observations (Pichugina et al., 2017) and mesoscale simulations (Aird et al., 2022) and comparison between reanalysis data and lidar data from Bui et al., (2025).

Moreover, we aimed at identifying the knowledge gap addressed in our manuscript more clearly, highlighting our work on a commercially operating offshore wind farm with turbines of the 6MW generation including the analysis of SCADA data and the comparison of turbine efficiency during situations with equal REWS in aeroelastic simulations, compared to other studies (e.g. Murphy et al., 2019) .

EC2 The use of lidar PPI scanning for wind profiling is indeed an interesting component of the study. However, this technique has been applied in previous research in both wind engineering and meteorology. As such, it may not be considered fundamentally novel, contrary to what is suggested in the cover letter. See, for instance, Goit et al. (2017) or Visich and Conan (2025) for both recent and earlier applications of this scanning mode. Notably, Visich and Conan (2025) also used PPI scanning to detect LLJs.

The suggested papers are now included in the methodological description of our study, to avoid the impression of this being the first time applying this scanning technique. While both techniques seem similar, there are some key differences between the application of the scanning techniques. Goit et al. (2017) only perform PPI scans at two different elevations, to generate wind speed time series at the rotor height of nearby wind turbines. Visich and Conan (2025) on the other hand use six subsequent scans with increasing elevation from 0° up to 13.89°. This allows them to measure wind speeds up to a height of 600m, despite their comparatively lower scanning range of only 3000m. The Generation of wind profiles between Visich and Conan (2025) and our study follows a different principle as well. While we in our study use the entire measurement volume to generate the profiles, Visich and Conan place a virtual metmast in their scanning area, to generate a more localized vertical wind profile. Tests within our data showed that the two different approaches show slight deviations in the lower 100m of the scan, while not greatly affecting our LLJ detection rate. Further details are available in the answer to comment ***R1.3***.

EC3 The study reports large wind veer across the rotor, with $\Delta\theta$ ranging from 0 to 40 degrees. It may be worth clarifying whether values of $|\Delta\theta|>20^\circ$ are realistic under typical atmospheric conditions. For example, wind veer is rarely above 0.1°/m, even in stable conditions. For a turbine with a rotor diameter of 126 m, this would suggest a typical $\Delta\theta<15^\circ$. If previous studies have reported larger directional shear in the atmospheric boundary layer, it would be helpful to cite them in support of the current findings.

We included further information in the manuscript in Section 4, lines 258ff., directing to our own findings, where we observed large wind veer across the rotor area, even exceeding the before-mentioned 40°. Also, we included references to two studies also observing very large directional veer onshore during all situations (Murphy et al., 2020) as well as offshore during LLJ situations (Olsen et al., 2024) to embed our choice of artificial profiles into a larger context.

EC4 The reported frequency of LLJ occurrence under convective conditions appears higher than that found in previous studies, such as Wagner et al. (2019). An interpretation of these results in light of existing literature could help contextualize the findings.

To contextualize our findings, we conducted further analysis of LLJ occurrences during convective boundary layer situations to investigate the discrepancies to the work of Wagner et al. (2019) . Here, we observe again, a clear tendency for LLJs to emerge from directions where the fetch lengths are rather small, indicating an advection of the LLJs from land masses, i.e. not generated over the open water under the prevailing atmospheric conditions at the wind farm. However, as we are estimating the atmospheric stability based on the temperature difference between sea surface and transition piece, we can only make claims about the conditions in that specific height band. Wagner et al. (2019) also mention a dependency of LLJ occurrence on the synoptic weather patterns.

As their measurements stem from a different location as well as a different time frame and apply a different measurement method, the results are not comparable one on one as well. Future studies based on our dataset could reveal the occurrence of such patterns, e.g. baroclinicity, frictional decoupling, or the passing of warm/cold fronts, favoring the emergence of LLJs.

Another factor leading to the differences in detected LLJ events is the definition of such events. Comparing the two different definitions from Wagner and Hallgren, we observe clear differences of LLJ occurrence during different stratifications. Figure 1 shows, that while the number of detected LLJs decreases from Hallgren to Wagner across all different regimes, during unstable stratification, this difference is largest.

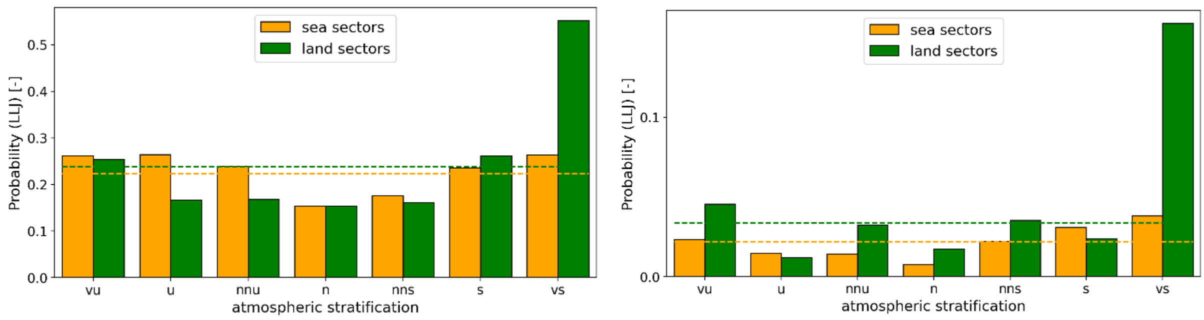

Figure 1: Probability of LLJ detection across all stability regimes for the Shear definition introduced by Hallgren et al. (2024) on the left and the fall-off definition used by Wagner et al. (2019) on the right.

Additionally, we also observe, that the average core height across the stability regimes, shows a downward trend towards more stable situations (Fig. 2). Together with the insight, that the Fall-off based detected LLJs tend to have lower core heights (Fig.3), this also adds to the explanation of the observed discrepancy.

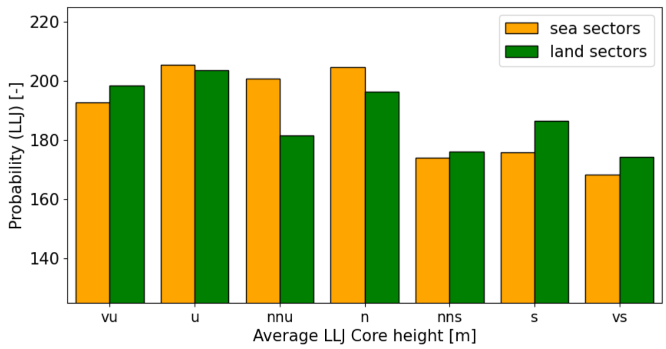

Figure 2: Average core heights of all detected LLJ events across the different stability regimes.

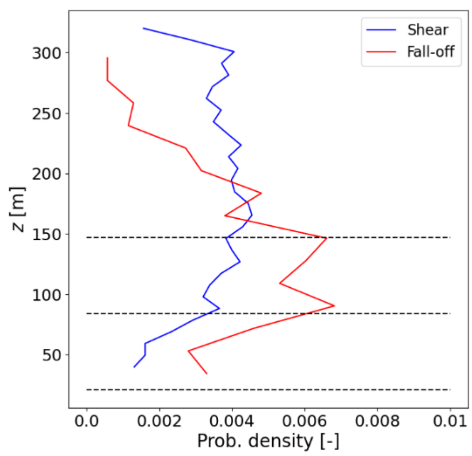

Figure 3: Distribution of the Core heights for all detected LLJ events using either the Shear definition (blue) or the Fall-off definition (blue).

R1 Review Comment #1

Major Comments and Concerns

R1.a My primary concern is that the wording used throughout the manuscript (including the title, abstract, and discussion) creates a strong impression that LLJs lead to a general decrease in turbine power (e.g., Title; Line 416; Line 441), not just a decrease in conversion efficiency. This framing undermines the well-known physical mechanisms of LLJs (e.g., inertial oscillation), which accelerate wind to super-geostrophic speeds, thereby increasing the total available wind energy and potentially increasing absolute power output compared to non-LLJ conditions. The depiction in Figure 2, while illustrative of profile shape, could also enhance this potentially false impression.

To rectify this, the authors should reframe the manuscript to focus explicitly on the impact of LLJs on the turbine's energy conversion efficiency. This is the true finding of the study. Consequently, the title should be revised to reflect this focus (e.g., "...on the power conversion efficiency of offshore wind turbines"). To test the hypothesis of overall power impact, the authors could perform a direct comparison of absolute power from SCADA data during LLJ episodes versus non-LLJ episodes on the same days (thus excluding non-LLJ extreme events, e.g. cyclones). Without this analysis, claims about overall power reduction are unsubstantiated.

Thank you for the comment. During the preparation phase of this manuscript, we have discussed the framing of the paper at length and completely agree with your before-mentioned comments. However, some of the formulations in the presented manuscript remained unclear and might be misleading in that regard. We have thus reformulated statements and claims made in the paper, where required, to more accurately reflect the results of our study. Following your suggestion, we changed the manuscript title to

"Low-level jets influence power efficiency of offshore wind turbines",

further clarifying the scope and main outcome of our study.

Regarding the second part of this comment: As we do not aim at describing the emergence mechanisms and origins of the detected LLJs, we decided to exclude this type of analysis from our study. To compare e.g. situations with similar geostrophic wind speeds we would need measurements reaching higher up into the boundary layer, not available for this location during the time of the measurements. In this study, we want to highlight more the energy conversion process during situations with the same REWS.

R1.b The study's conclusions are heavily dependent on the choice of the shear-based LLJ definition from Hallgren et al. (2023), which yields an occurrence frequency (22.6%) that is nearly an order of magnitude larger than other methods (e.g., Wagner et al. (2019), Kalverla et al. (2019)), which are in closer agreement with one another (Table 5). The authors must provide a more robust justification for using this "odd one out" definition.

The author should clarify how the shear is computed. Is it the maximum shear above and below the jet core? Given the sensitivity of shear computation to the variations and noise, which are known to be present in measurements, how are these factors treated?

In the manuscript, we provided two main reasons, why we use the Shear definition. First, Hallgren et al. (2023) show, that this definition is less sensitive to the chosen height band, i.e. the difference in detected LLJs doesn't increase as much with available measurement height for the shear definition compared to fall-off based definitions. This is crucial within our study, as we are only recording profiles up to maximum heights of 350m. Secondly, previous studies have shown, that the shear in the vertical wind profile has an effect on turbine performance (Murphy et al., 2020). Following this, the more crucial metric for examining an LLJs effect on turbine performance would also rather be the shear than the fall-off, which can be realized across several tens or even hundreds of meters.

Indeed, for this definition, the maximum shear below the core and minimum shear above the core,

$$min\left(\frac{du}{dz}\right) < -0.01\frac{1}{s}\text{ , above core height and}$$

$$max\left(\frac{du}{dz}\right) > 0.01\frac{1}{s}\text{ , below core height}$$

respectively, have been considered for detection. To reduce influence of noise, we apply a slight smoothing across a 30m window, before computing the shear.

In the revised manuscript, we aimed at highlighting our reasoning more clearly. Section 3, lines 307ff now read as:

> *"For all further analysis carried out in this study, we use this definition. We choose the shear based LLJ definition for two reasons. First, its decreased sensitivity to the available range of the vertical wind profile (Hallgren et al., 2023). Secondly, the shear definition is able to capture the change of wind speed across smaller height differences, thus taking factors impacting the wind turbine performance directly into account, whereas the fall-off can also be realised across several tens or hundreds of meters.."*

This aspect has also been explored in the Discussion, in lines 423ff.

> *"In the literature many different LLJ definitions are used, all coming with individual benefits and drawbacks. [...] For our analysis, we mainly use the definition of an LLJ proposed by Hallgren et al. (2023). The main characteristic of this definition is that instead of using the absolute and/or relative fall-off of the wind speed it makes use of the shear of the wind speed. Hallgren et al. (2023) show that this makes the provided definition less sensitive to limited measurement heights. This is especially important in our case, as we generated wind profiles from multi-elevation PPI scans, reaching maximum heights of around 350m. Other studies using e.g. reanalysis data make use of*

increased measurement heights, thus also showing occurrences of LLJs at higher altitudes (e.g. Kalverla et al., 2019). Also, the shear-based definition is more applicable to wind energy-related purposes, as it concentrates directly on the shear, a property which is shown to have a non-negligible influence on the conversion efficiency of a wind turbine (Dörenkämper et al., 2014; Murphy et al., 2020). Using this local property instead of a fall-off - which can in theory be realised over a large height difference - also allows a precise description of the inflow conditions across the rotor area.“

Minor Comments and Suggestions

R1.1 Line 172 (and Introduction): The concept of REWS is fundamental to this paper's methodology and novelty, yet it is not formally introduced until Section 2.4. REWS and its motivation should be introduced much earlier, in the Introduction (Section 1), to properly frame the study for the reader.

The concept of REWS is now introduced in the introduction as part of the description of existing literature on experimental campaigns. Further, along with our objective statement, we introduce the concept of multi-elevation lidar scans to give a quick idea on how LLJs are detected and the REWS is obtained.

R1.2 Line 170: To highlight its significance, consider changing the section title to better reflect the use of REWS, for example: "Performance Analysis Using an Equal Rotor Equivalent Wind Speed Framework."

The suggested title is descriptive and well-fitting to the contents of the section. We inserted a similar title in the new manuscript:

“Wind turbine performance analysis via an equal REWS framework”

R1.3 Line 133: How is the wind direction estimated from the VAD algorithm? This assumption of a spatially homogeneous wind direction is critical, as the lidar scans cover a range of nearly 10 km. A brief discussion of the validity and potential uncertainty of this assumption is needed.

Using the VAD algorithm, one wind speed is estimated across each single range gate of the lidar measurement. Using the least-squares fitting method, we fit a cosine-function to the measured line-of sight velocities v_{LOS} of the observed range gate. The phase offset of the cosine then allows us to make an assumption about the wind direction of this subset of the data. A description of the considered uncertainties is given in Appendix B. A description of the fitting procedure was added to Section 2.2 in the revised manuscript.

“Second, applying the velocity azimuth display (VAD) algorithm the horizontal wind speed is computed from the measured line-of-sight velocities v_{LOS}. Here, we assume a spatially homogeneous wind direction across each range gate and a negligible influence of the vertical wind speed component due to the small elevation angles. To obtain the wind direction at each range gate, we perform a least-squares fit using a cosine-function

to fit the measured v_{LOS}. The phase offset of the obtained fit determines the wind direction for the particular subset of the data."

Regarding the validity of a profile generated from wind speed data ranging up to 10km from the lidar, we performed a comparison of our used profiles to a second set of profiles generated via a method similar to Visich and Conan (2025), i.e. choosing one localized point in space based on the scan settings, and only considering data points in the vicinity of that point at all available altitudes. First, as the measurement campaign, from which the data stems was initially designed to generate profiles for the wind power forecast, elevation angles of the PPIs were kept rather small. Thus, to gather data at altitudes relevant for LLJ detection, we have to include measurements form far away ranges. However, comparing the wind speed of our lidars measured at wind turbine hub height as well as the computed REWS to the nacelle wind speed of turbine NG17 in the NG wind farm, we reach good agreement (cf. Comment ***R2.23****). Second, placing a "virtual metmast" at a large distance from the lidar leads to significant data losses, generating incomplete noisy profiles.*

Figure 4 shows the profiles of one entire day once only considering data points at one location in the xy-plane across all different altitudes and once considering every data point in our measurement volume. This day of samples exemplary shows the worse availability of the first method due to the long ranges necessary to depict the entire rotor area and make valid claims about LLJ detections.

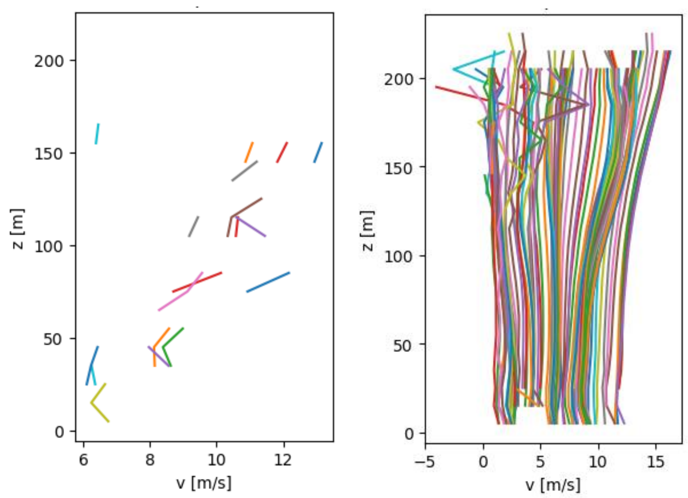

Figure 4: Vertical wind profiles generated via a virtual metmast approach (left) and the entire volume of the scan (right) for one entire day.

Averaged across the entire month of March 2022, we observe, that both methods generate a very similar vertical wind profile and the average absolute difference between the two lies below 1 ms^{-1} across almost the entire height range (Fig 5b, c), even including many situations with southerly winds, i.e. with coastal effects present (Fig. 5a). Only towards very high altitudes above 300 m with very little availability for the metmast approach, the two methods deviate visibly from each other. Further, we also examined the standard deviation of wind speeds with height from the volumetric wind profile approach (Fig. 5d). Here, we see that while it starts out slightly above 1.2 ms^{-1} at the lower end of the profile it reduces towards 0.9 ms^{-1} at higher altitudes.

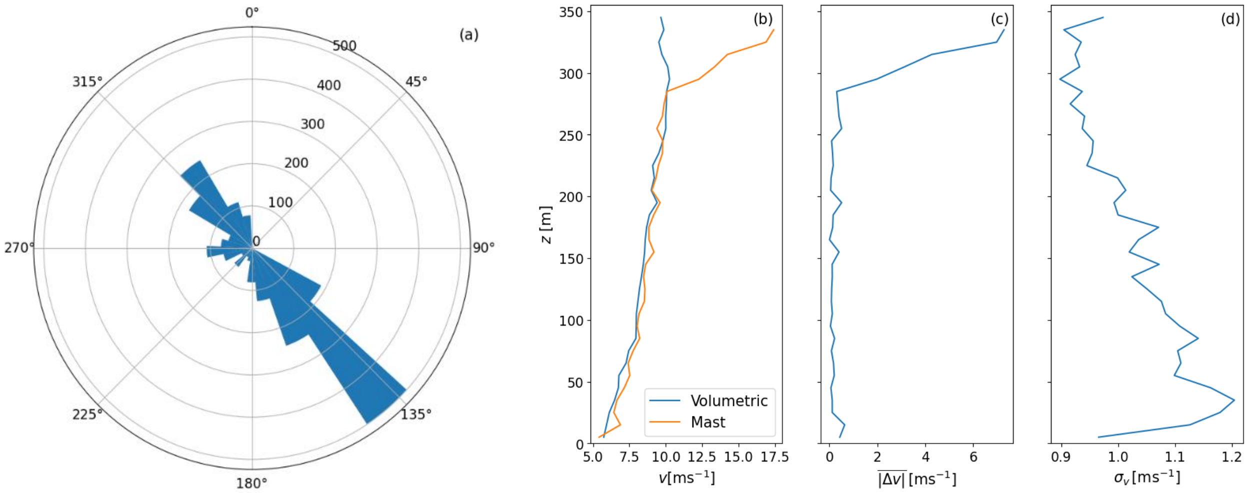

Figure 5: Wind speed distribution at hub height during March 2022 (a). Monthly averaged vertical wind profiles of both approaches are depicted in (b), as well as the average absolute difference between them (b) and the average standard deviation of wind speeds over height for volumetric scans (c).

Next, we evaluated the LLJ detection rate of the virtual metmast approach. In March 2022 – the month with highest LLJ detection rate - the temporal availability of profiles dropped from 99.52 % with the volumetric approach to 79.88 % using the metmast method. Next to the decreased availability, the detected occurrence frequency only slightly changed from 33.12 % of all profiles with our approach to 36.62 % of all profiles using the metmast approach. While these particular numbers are taken for the Shear definition of the LLJ, results are similar throughout all the different definitions. To address this matter in the revised manuscript, we added the following paragraph in the Discussion section:

"Further, we also compared vertical wind profiles retrieved using our volumetric approach to more localized wind profiles obtained via a method similar to Visich and Conan (2025) for one month of data. Here, we observe average absolute differences between the two methods below 1 ms^{-1} across almost all heights. The LLJ detection rate for the two different methods also only differs by 3.52 %, despite a considerably lower data availability of the metmast-style profiles, dropping by 19.64 %."

R1.4 Lines 149-152: The vertical profile is derived from multiple low-angle scans. It is unclear where the resulting vertical profile is horizontally located relative to the turbine.

An illustration depicting the scan geometry and the effective location of the final wind profile would be extremely helpful for clarity.

An additional figure displaying the dimensions of the measured wind field across heights is added together with information about the range of the rotor swept area of the turbines in the NG wind farm. Together with a revised version of Figure 2 it has been added to the manuscript. Figure 6 shows the revised version of Figure 2 from the manuscript.

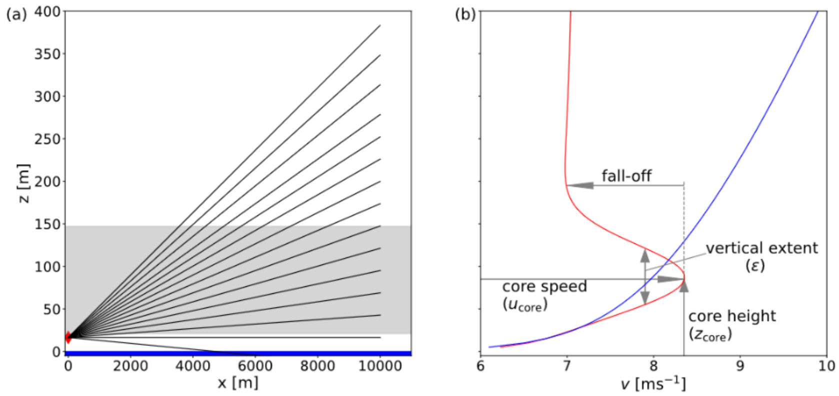

Figure 2. (a) Exemplary scan pattern for this campaign, with the lidar displayed at the height of the transition piece (red ♦), the lidar beams as black lines and the rotor swept area marked as a gray background. (b) Exemplary LLJ profile in red, with the important terminology defined. For reference, a logarithmic wind profile with the same REWS is shown in blue.

Figure 6: Revised version of Figure 2 in the manuscript.

R1.5 Figure 2: As mentioned in the major comments, this figure risks creating a false impression. This is a perfect opportunity to visually demonstrate the paper's core concept: including a non-LLJ (e.g., logarithmic) profile that has the exact same REWS. This would clarify that the study is about the shape of the profile, not the absolute magnitude of the wind.

The corresponding figure has been revised, to better fit the overall theme of our paper. Before, the aim was to show how different LLJs and logarithmic wind profiles with the same hub height wind speed are concerning the energy flux through the rotor area. Now in the revised version, we depict two wind profiles with same REWS. Figure 7 shows the old version (left) and the new version (right). The new version is now also part of a double *figure, containing a sketch of the scanning pattern including information on the rotor swept area of the turbines (cf. Comment* ***R1.4****).*

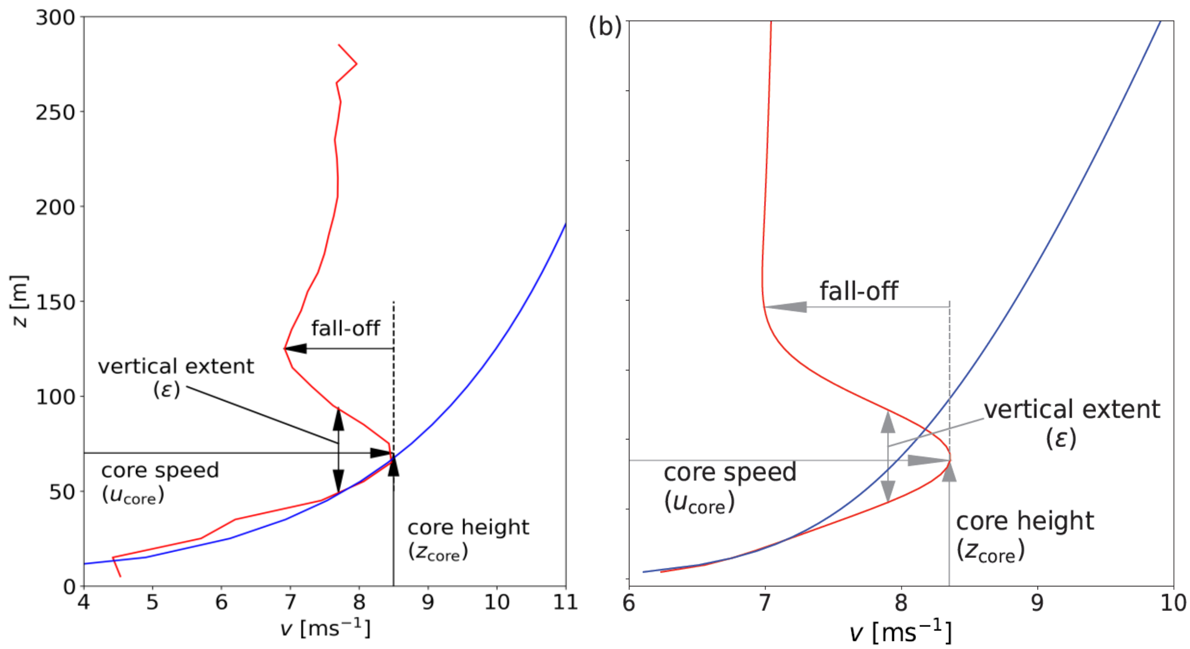

Figure 7: Old version on the left with the same hub height wind speed for logarithmic and LLJ profile respectively and new version of Figure 2b in the revised manuscript on the right with same REWS for both profiles.

R1.6 Figure 13: This figure is insightful but could be improved. Is it possible to add a colour code or different markers to the data points that relates back to the specific profile characteristics shown in Figure 5? This would help the reader understand the source of the scatter in the LLJ results.

Including the different connotations for logarithmic, uniform and LLJ profiles, we would like to redirect the attention to Figures 14 and 15. Here, all the different core heights and widths, TI and fall-off parameters of the different LLJ profiles are depicted and their impact on the power conversion efficiency is described.

R1.7 Section 2.5: The simulations confirm the general trend of reduced performance. However, as seen in Figure 13, there appear to be a few intriguing cases where the turbine performance for LLJ profiles is better than for the logarithmic or even uniform profiles with the same REWS. A deeper analysis and discussion of these specific cases could reveal valuable insights into turbine energy conversion under complex inflow. It also raises the question: could this be a limitation or artifact of the Blade Element Momentum (BEM) theory used in OpenFAST under such extreme shear?

From Figure 13a and 13b we conclude that these cases occur during rather low shear and veer situations, paired with low core heights and a comparably small fall-off. Looking further into the data, we found that these cases also feature the highest TI – which has *also been shown to improve power production (e.g. Dörenkämper et al., 2014) - and comparatively low fall-off values as well as rather wide LLJ cores, also shown to be beneficial for power generation (Figure 15a&b).*

2 Review Comment #2

General comments:

R2.a The article is a good and valuable piece of work, I do not have major issues to point out. A number of medium and minor ones are listed below. I would recommend that the authors perform a consistency check on the article as a whole, as it is in some places noticeable that different parts of it were written by different people (which is completely fine, but having consistent style, especially when it comes to figures, would benefit the overall impression; but that's just a suggestion). The methods section could be expanded a bit more, keeping in mind that not every reader may be familiar with the specific technology used in the study. The authors might also find the following article (and its reference list) of some interest, as it presents a somewhat similar study but was not referenced in the manuscript: https://doi.org/10.1016/j.oceaneng.2025.120749.

*Thank you very much for the positive evaluation of our manuscript and the very constructive feedback given to improve the manuscripts quality. We aimed at providing more in-depth breakdown of our methodology, while at the same time not expanding the paper too much. Also, we made an effort to make the style of the written text as well as the Figures more unified. The suggested study, also referred to by the editor in his comment (**EC2**) has been included in the methodological section of our manuscript.*

All minor comments regarding spelling and punctuation have been incorporated into the revised manuscript. As there is no direct answer required from our side, these comments have been excluded from this document.

Questions and remarks:

R2.1 Lines 29-30: what is the difference between thermal-driven winds and thermal wind? Also, a brief explanation of reverse shear flow could be nice.

When the atmosphere is not barotropic (height independent horizontal pressure gradient and geostrophic wind), but instead baroclinic, i.e. horizontal temperature gradients causing the horizontal pressure gradient to change with height, the geostrophic wind also becomes height dependent. The corresponding difference between the geostrophic wind at different heights is then called thermal wind (Emeis, 2018). While thermal winds are thus only changes in the geostrophic wind with height, thermally-driven winds are actual winds that are generated via temperature and heat flux differences at the Earth's surface, e.g. at land-sea barriers (National Research Council. 1992). In this context "reverse shear flow" refers to thermal winds, appearing in baroclinic situations, opposing the geostrophic wind/thermal wind vector and thus resulting in LLJs. The condensed depiction in the presented manuscript may have caused confusion. Therefore, the corresponding section has been adjusted in the manuscript for improved clarity and now reads as:

"Further, LLJs can also emerge during baroclinic situations, as in these cases the thermal wind vector may oppose the direction of the geostrophic wind and thus cause a reverse shear flow (Guest et al, 2018)."

R2.2 Line 107: it might be worth it to mention if the turbines are fixed or floating, as it might affect the measurements.

The information has been added to the manuscript.

R2.3 Line 108: it can be deduced from the context that Vaisala Windcube 400S is a lidar but mentioning it explicitly can improve clarity.

The information has been added to the manuscript.

R2.4 A sketch showing the turbine, its transitional piece, the location of the lidar, the size of the rotor, the sea level and the direction of the lidar scans would be very illustrative.

A Figure containing a vertical slice of the scanning pattern together with the lidar position above mean sea level and the rotor swept area of the turbine has been added to Figure 2a in the revised manuscript. The revised Figure is displayed in Figure 6 as an answer to comment ***R1.4****.*

R2.5 Figure 1: the map would benefit from a frame in subfigure (a) that corresponds to the zoom-in in subfigure (b).

The suggestion has been implemented. Figure 8 displays the updated version of Figure 1 in the manuscript.

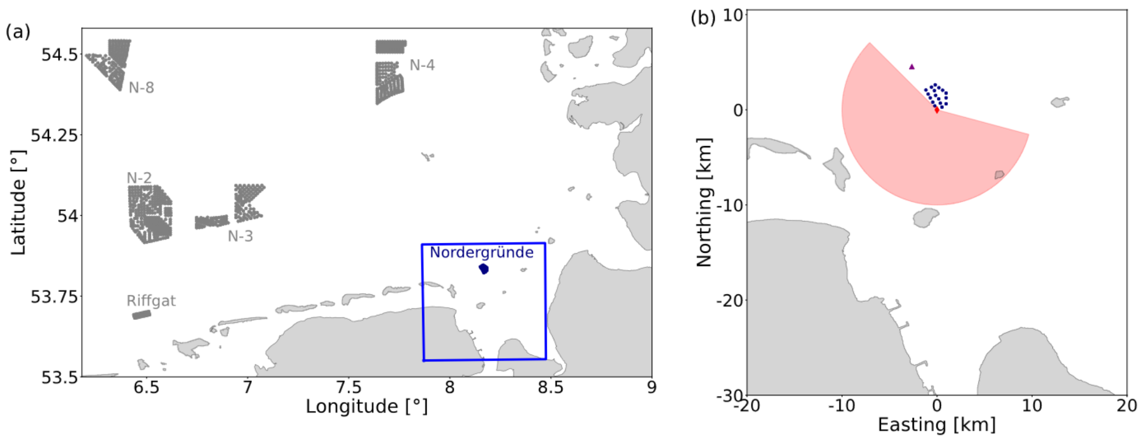

Figure 8: Revised version of Figure 1 in the presented manuscript.

R2.6 Table 1: please specify what is meant by "PPI opening angle" and "range gates". Better still, elaborate a bit more on how the scans are performed, as it might be not obvious for the readers that do not have much experience with lidars.

The information has been added to the manuscript. The new paragraph reads as follows:

"The lidar at NG17 measures a set of azimuthal scans (plan-position indicator, PPI) with increasing elevations, similar to Goit et al. (2020) and Visich and Conan (2020). The measurement sector is aligned with the prevailing wind direction at lidar height according to Theuer et al. (2024) and each PPI covers an azimuthal range, i.e. has an opening angle, of 80°. For each azimuthal angle, the lidar is able to process wind speed information at 159 ranges along the beam, also called range gates. At each range gate, the line-of-sight velocity (v_{LOS}) and carrier-to-noise ratio (CNR) as a quality measure are stored."

R2.7 Table 1: in "elevation angles", what does "-0.2" stand for?

*The first scan in every set is measuring at a negative elevation, thus the -0.2°. In the revised manuscript a sketch of the scanning pattern (Fig. 2a) has been added as well, aiming to clarify this question (cf. Comment **R1.4**).*

R2.8 Line 124: please explain briefly what SCADA data contains.

A brief explanation on the information contained by the SCADA dataset has been added. The following paragraph has been added to the manuscript:

"The SCADA data contains various parameters that describe the turbine condition, such as the generated power, the yaw direction, blade pitch and operational status as well as meteorological parameters derived at hub height, i.e. wind speed and direction."

R2.9 Lines 142-144: the first and the last sentence in these lines seem to be contradicting each other, unless I misunderstood something. Please consider revising the paragraph for clarity.

The corresponding sentences have been restructured for a better understanding. The method using inclinometers uses the same underlying principles, but with less assumptions. The original method presented in Rott et al. (2022) can use the same algorithm without inclinometer measurements by deriving the tilt of the turbine from the operational status. In the revised manuscript, we now explain these methods as follows:

"First, there are uncertainties regarding the pitch and roll angles of the lidar which dynamically change due to platform movement of the TP, which is mainly caused by the thrust of the wind turbine rotor (Rott et al., 2022). To account for this, we use tilt measurements of inclinometers placed in the lidar and correct the height of the lidars probe volume. During periods without inclinometer measurements, we use a different method introduced by Rott et al. (2022). This method estimates the platform tilt without any motion sensor measurements and instead relies on the yaw position and power production of the turbine."

R2.10 Line 155: is there any justification for using a 30 m window?

The aim here is to slightly smoothen the profile in vertical direction to generate a less noisy profile and enable the peak-finding mechanism used to detect LLJs to work more reliably. The 30m window is chosen to only include the measurements directly neighbouring the point of interest, thus altering the measured profile as little as possible.

R2.11 Lines 186-187: isn't "we applied a filter neglecting..." sentence repeating what was already described in lines 179-181?

Yes, the two sentences have been summarized into one, as they both display the same information.

R2.12 Line 245: by "four dimensional wind fields", do you mean a 3D wind field that varies with time?

This was a typo, it should have been three dimensional. Using TurbSim, a temporally varying wind field along the height and width of a predetermined grid is created, thus only covering two spatial and one temporal dimension.

R2.13 Line 250-251: are the subroutines named in accordance with the jobs they do? I.e., ElastoDyn is used for structural dynamics, SubDyn for sub-structural dynamics and so on. A clear indication could be valuable for a reader who wants to perform similar simulations.

The information has now been rearranged in a way that it becomes more clear, which subroutine conducts which calculations.

"Further, structural dynamics (ElastoDyn), sub-structural dynamics (SubDyn), control and electrical-drive dynamics (ServoDyn) as well as aerodynamic loads (AeroDyn) are computed using different subroutines within the openFast framework."

R2.14 Line 263: can you elaborate why exactly there are fewer measurement points at higher altitudes? An illustration would be helpful.

*An illustration depicting the measurement geometry, showing that only few scans are measuring at the highest altitudes, while lower altitudes are covered by a wide range of scans has been added (cf. Comment **R1.4**, Fig.6) .*

R2.15 Line 266 and in general: is there any correction for LLJ duration in time? For example, if three 10-minute profiles in a row feature an LLJ, then two don't, and then 30 more do, will the two outliers be counted or not? As I understand it, LLJs as a meteorological phenomenon have to be prolonged in time and space and are not just a feature of individual averaged profiles. Profile-based detection, as you mentioned earlier, can be a good tool but is prone to errors.

We agree, that profile-based detection – especially when using experimental data – is prone to uncertainties. However, the main objective of our study is the influence of anomalies in the vertical wind profile - more specifically local wind speed maxima and *strong positive and negative shear around them - on the power conversion efficiency of the offshore turbines. Hence, we didn't include any correction for the LLJ duration in time. Any profile that contains a local maximum accompanied by strong positive and negative wind shear, independent of LLJ occurrences in subsequent scans, is treated as an LLJ event.*

R2.16 Figure 6 (a): the coastlines are helpful but consider shading or hatching land vs sea for a fuller picture. Also, maybe a bit of zoom-out would aid comprehension, because based on the current picture the reasoning for the authors' choice of land and sea sectors is not clear. Especially on the top-left it would seem that the land-sea sector border should be placed further clockwise. It is understandable, of course, that such a complex coastline makes the land-sea direction definition rather complicated.

*The suggested change with marking the land area to provide better differentiation has been implemented in the revised manuscript. For consistency, this change is also implemented in Fig. 1a and 1b in the revised manuscript (cf. Figure 8, Comment **R2.2**). The definition of the land and sea sectors, respectively, has undergone intense discussions prior to the submission of the first manuscript and there are also arguments to be made for changing them slightly. In the end we decided to exclude the North Frisian Islands and concentrate on distinct landmarks of the coastline such as the Jade Bay and the mouth of the river Weser to define these sectors.*

R2.17 Figure 6 (b): consider marking the same land & sea sectors as in (a) on the wind rose plot.

The suggested change has been tested, but due to the colours already present in the plot we feel this would decrease the readability of the plot.

R2.18 Line 293: please provide the formula used for obtaining the Obukhov's length from the temperatures, unless it's a very complicated one (then the reference is sufficient).

The formula to compute the Obukhov length from our measurements is now included in Section 2.1:

"The meteorological measurements are used to estimate the prevailing stability regime at the measurement location, via the Obukhov length L following Schneemann et al. (2021). First, the bulk Richardson number

$$Ri_b = \frac{g}{T_\mathrm{v}} \frac{0.5 z_\mathrm{TP} \left(\Theta_\mathrm{TP} - \Theta_0\right)}{u_\mathrm{li}^2}$$

is calculated using the gravitational acceleration g, the virtual temperature at the sea surface T_V, the transition piece height z_{TP}, the virtual potential temperatures at transition piece height and sea surface Θ_{TP} and Θ_0, respectively, and the wind speed at transition piece height as measured at the closest range gate of the lidar u_{li}.

Second, the bulk Richardson number is used to compute the dimensionless stability parameter

$$\zeta = \begin{cases} \frac{10Ri_b}{1-5Ri_b} & Ri_b > 0 \\ 10Ri_b & Ri_b \leq 0. \end{cases}$$

Finally, we compute the Obukhov length

$$L = \frac{0.5z_{\mathrm{TP}}}{\zeta}.$$

Due to our measurements of meteorological parameters at sea surface level and transition piece height, the estimation of L is strictly valid only between these two heights."

R2.19 Figure 10: adding an edge line of the same shade but darker to each of the two histograms might improve the graphs' readability (just a suggestion, feel free to ignore)

For improved clarity, the edge lines have been added to the plot in Figure 10 in the revised manuscript as well as the other histograms in the manuscript for consistency (Fig. 8, Fig. 9a & Fig. 11 in the revised manuscript). Figure 9, below, displays the implemented changes.

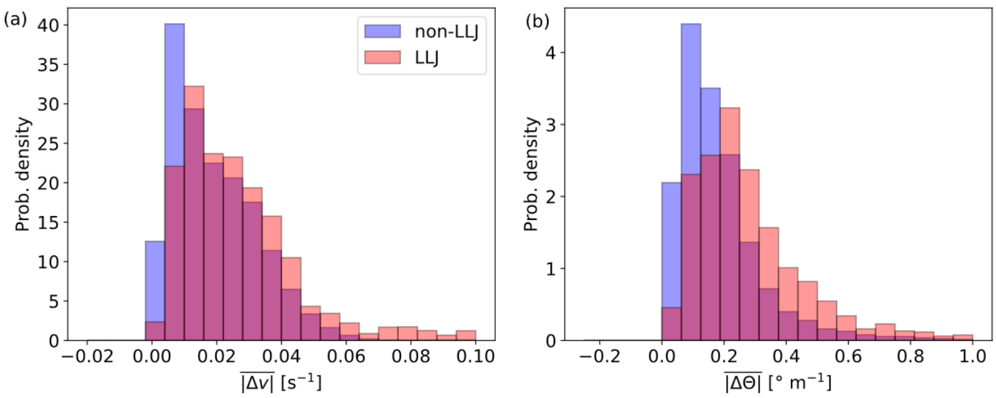

Figure 9: Revised Figure including black edge colour for improved clarity of the presented histograms.

R2.20 Figure 11: consider adding a cross-reference to the definition of v_{eq} in the figure's caption.

The suggestion has been implemented in the manuscript.

R2.21 Line 334: a box plot doesn't fully reflect the distribution of a quantity, only some metrics of it.

Thank you for the hint, a correction has been implemented. Now the corresponding part reads as follows:

"Figure 12 shows box plots and the median of the PO_{TI} per 10-minute interval over the prevailing stability regime."

R2.22 Figure 13: I am not sure I understood: does every marker correspond to one simulation?

Yes, every marker corresponds to one of the simulated inflow profiles. A clarification has been added to the revised manuscript. The revised caption of Fig. 13 now reads as:

"Simulated normalised power production over the shear (a) and veer (b) of the wind profiles across the rotor swept area for each of the simulated inflow profiles. Uniform wind profiles are depicted as orange crosses, logarithmic profiles as black circles and the LLJ profiles as red diamonds."

R2.23 Line 375: the statement of a good agreement would be stronger with some quantification of it

We agree, the corresponding sentence has now been adjusted as follows:

"However, generating the wind profiles from the volumetric 3D wind field, our results at hub height showed a good agreement with the wind speed obtained from operational data of the turbine NG17. The correlation coefficient between the two variables lies at R^2=0.97."

Figure 10 shows a scatter plot, comparing the REWS and lidar measured hub height wind speed to the hub height wind speed measured by NG 17. The orthogonal distance regression performed here, also shows very small bias and a slope close to one for both of the variables. Due to brevity this depiction is not included in the manuscript.

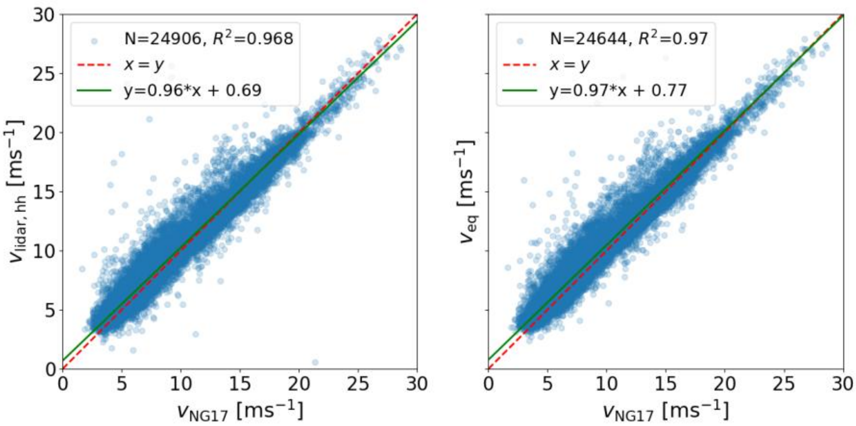

Figure 10: Scatter plot between the hub height wind measured at the nacelle of NG17 vs. the lidar measured hub wind speed in (a) and the computed REWS in (b). Both plots also feature a regression line obtained from ODR in green as well as the x=y curve in red.

R2.24 Lines 409-410: the sentence starting with "Our results..." is a bit confusing. Isn't LLJ a feature of the whole profile? The authors probably meant that the cores of LLJs tend to be located at higher altitudes.

Yes, clarification has been added to the manuscript. The revised part now reads:

"Our results also show that the amount of detected LLJ cores increases with height, up to a local maximum at 165m."

R2.25 Line 428: it is not quite clear if "the authors" refers to Zhang et al. or the authors of the present paper.

The statement is referring to Zhang et al. (2019). A clearer reference has been added.

"The trend we observe is further backed up by Zhang et al. (2019), who also show a decreased power production for LLJ-situations with the same hub height wind speed as for a logarithmic wind profile within their simulations, despite using rather weakly pronounced LLJs in their study. Here, Zhang et al. (2020) also show that this deficit is highly dependent on the relative position of the wind speed maximum inside the rotor area, which is confirmed by our simulations."

R2.26 Figure A4: making the axes square and starting at 0 would be more visually appealing. Also, especially for (a), consider making the markers more transparent, like in Figure A5: this would help the reader to judge the density of the cloud.

This is a good suggestion, aligning the plot further to the overall style of the manuscript. Figure 11 displays the revised version of Figure A4 in the manuscript.

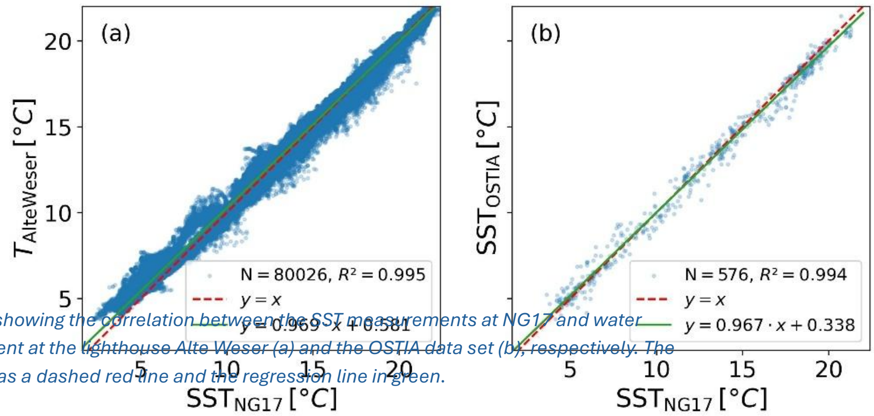

Figure 11: Scatter plots showing the correlation between the SST measurements at NG17 and water temperature measurement at the lighthouse Alte Weser (a) and the OSTIA data set (b), respectively. The $y=x$ curve is depicted as a dashed red line and the regression line in green.

R2.27 Eqs. B5: in the second equation, shouldn't the right-hand term include (vi/rho0) as a multiplier?

That is correct and the equation has been modified accordingly. We also checked the calculation in our code again. Here, the derivative has been correct all along, so no changes regarding the presented results occur. The correct formulation of Eq. B5 now reads:

$$\frac{\partial v_{\mathrm{i,corr}}}{\partial v_i} = \left(\frac{\rho(z)}{\rho_0}\right)^{1/3} \quad \text{and} \quad \frac{\partial v_{i,\mathrm{corr}}}{\partial \rho} = \frac{1}{3} v_i \rho(z)^{-2/3} {\rho_0}^{-1/3}.$$

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