Review of paper titled ”Offshore wind profile characteristics and their impact on floating wind turbine power production” by Nikolas Angelou and Camille Dubreuil-Boisclair (wes-2026-20)

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1 Overall paper summary

The presented manuscript ”Offshore wind profile characteristics and their impact on floating wind turbine power production” by Nikolas Angelou and Camille Dubreuil-Boisclair uses measurements from a WindIris nacelle-mounted lidar as well as SCADA data from a floating offshore wind turbine from the scottish Hywind wind farm to evaluate the impact of different characteristics of wind shear on the power production of the turbine. The authors show that during one third of all measured situations a non-standard wind profile is present. Comparing these findings to the turbines' SCADA, the authors claim that negative shear profiles lead to a reduction in power production of up to 20 % compared to the reference power curve of a fixed-bottom wind turbine of the same type. Hence, the authors conclude that nacelle mounted lidars can have a positive impact in the assessment of (floating) offshore wind turbines, as these devices are able to measure wind speeds across the entire rotor swept are instead of only providing measurements at one point in space like anemometers do.

While the authors present a very interesting data set and the results provided in the manuscript certainly raise important questions on how we treat inverse shear for power prediction not only for fixed but also for FOWT, the paper itself needs a significant rework in several sections to be considered for publication. We therefore recommend major revisions to be carried out before acceptance.

General comments

- Section 2.1: this has only one paragraph denoting the field study, most of this subsection is about the magnitude of the angles of motion of the FOWT. What is missing is the description of the field campaign, layouts of the turbines in the wind farm and the location of the lidar. While the entire description need not be repeated from [1], this section is missing essential information, one example being the north orientation of the turbine/wind farm, since several wind directions are mentioned in the rest of the manuscript without any reference to the reader.
- Section 3: This chapter needs to be more precise in reflecting the workflow to compute the radial wind speed components. model rotor induction and estimate the vertical wind profile. Here, I am confused about the different assumptions that are being made, i.e. wind speed in the xy-plane being completely homogeneous (L. 134) vs. being only homogeneous at each measurement distance (L.139). Also, in Eq. 1, the induction seems to be already modeled, courtesy of f_{ind}. However, this is not properly explained in the text. In Eq. 2 and following it is explained that an induction model is (again) applied to all measurements. Isn't that double? Further, I understand that the rotor induction widens with upstream distance to the rotor, but does it make sense to

treat all the points equally independent if they lie within or outside the rotor area? Please clarify this along with a more detailed breakdown of the equation terms.

- Section 4.2: Why did the authors not consider computing a vertical wind speed $U = \sqrt{u^2 + v^2}$ and wind direction $\tan^{-1}(v/u)$ to compare shear and veer. Especially for the veer this estimate might be more meaningful, as with increasing vertical wind speed, the v-component (and hence the gradient of the v-component) might increase despite significant direction changes. This should be a point of discussion.

- A major finding is the almost 20% reduction in expected power production for the situations of inverse shear. As there is access to SCADA, why wasn't the power curve of the floating turbine used as a reference? The reductions have to be put into context of this specific turbine, so the comparison has to be made with a generated power curve from SCADA for the FOWT, else the differences in positive and negative shear could be much less than shown at below-rated wind speeds.

- The discussion section of the paper needs a major overhaul. While there are two limitations discussed briefly, there is no discussion of the results in the context of the state-of-the art results. While sample size is mentioned as a limitation, it is not discussed if it actually affects the interpretation of the results. Please add statistical significance tests or similar analyses to justify your results or better contextualize it in the state-of the art. Explain it here and compare the 18% magnitude with the exiting studies on LLJs and potentially comment on how this would affect the AEP given that you also state in the abstract that these conditions occur commonly. What is missing is a critical discussion and evaluation of the results in the context of the established and latest literature. There are also summary paragraphs in this section that can be completely removed.

- Section 5/6: I am missing a conclusion and/or discussion of the comparison between the inflow model and the radial wind speed measurements. During which situations were good agreements observed, where were deviations found and do the authors have an idea where possible deviations are originating from?

Specific comments

- L.21: Please add newer studies such as [2, 3] investigating the effect of LLJs on wind turbine performance and loads, and please mention findings of LLJ impact on turbine performance in the introduction. Further, discuss how your results compare to theirs in the discussion.

- L. 60ff: Please revise this paragraph completely to streamline the introduction section. Please include clearly in one or two sentences the main objective of the paper and what research question/gap the objective is addressing.

- Section 2.1: To make it clear to the reader, please introduce a table with the degrees of freedom, the mean values and standard deviations for below rated, rated and above rated regimes for clarity.

- L. 93: you could even have a schematic showing the degrees of motion and the floater type for the current study. Overall, this part on the field measurements is lacking, as mentioned in the general comments previously.

- L. 123: During the yawing motion, is care taken to avoid considering these scans? please mention that here. Why is this number specifically chosen ($< 10°$), do the coordinates not get distorted when turbine yaw standard deviation is 9°for a selected 10-min period? Please elaborate on this in the discussion section.

- Section 2.1: Information about how many scans were performed during one 10-minute interval is missing. This is especially important later when the RMSE is discussed.
- L. 119: Here, it would be good if a comparison to the lidar data availability would be given. Was lidar data available the entire time frame or were there certain data gaps?
- Figure 1: This figure is quite confusing, as the line indicating the rotor is not tilted by 5°, but the dots indicating the range gates of the lidar beams are shown based on the average turbine tilt of 5°. Please consider aligning this for sake of consistency.
- L. 127: It would like to ask the authors to include a brief description and the general idea of the data filtering and processing, so that the reader does not have to switch through papers to understand the processing procedure.
- L.132: Are u, v and w following the tilt of the turbine or are these variables defined in a global coordinate system with a moving turbine?
- L. 134: Is this that at each position in the x,y- plane the wind speed components u and v are the same at each height z? In my opinion, this is a this assumption is quite big, given that you have several measurements at different distances from the lidar to compute a volume of different wind speeds. Is this then an average across all measurement ranges and horizontal spread?
- L.140: Please clarify if the notation of veer $\partial v/\partial z$ is standard, as in general veer represents a wind direction change reflected in both components of the wind vector. This partial derivative represents a magnitude of the lateral component wind speed change with height, not necessarily cause by veer. If this is standard practice or has been already proven in the scientific literature, please provide references for the same.
- L. 166: I would like to ask the authors to clarify how the vertical wind profile is reconstructed from two measurement points in the upper part of the rotor area. Or are measurements across the entire range of the lidar beams which lie at different heights used to reconstruct the profile?
- L. 170: The main objectives of the paper should be defined in the introduction, and this is not referred to before. This is purely methodological and does not address the main objective of wind field characterization and verification of the power curve. Reformulate this sentence and clarify it or add proper descriptions to the introduction part and state this as an objective of the manuscript.
- Figure 2: Please add a reference to Figure 1 for the color scheme and describe what the dots and lines represent in the caption.
- L. 178-179: Could you please elaborate further on what deviations become larger with the higher beams due the wind veer?
- L. 204ff: even though a month-wise distribution is shown in the appendix, please include a table of the RMSE for the bins of your empirical conditions to discard a case. Seeing this in tabular form or through a figure will further help the reader associate it to a potential LLJ.
- Figure 4, Sub-figure (c): please show how these quantities are calculated, is the sonic anemometer wind speed corrected using a transfer function for the effect of the rotor? If not, please add a correction.
- Figure 5: Why are the number of periods different between the sub-figures? Also, expand on the caption for the individual sub-figures, also describe the dotted and dashed lines in (b).
- L. 254: Were these decreases in power production also related to the wind veer? Previous studies have shown that this also has a non negligible effect on the power production (e.g. [4]).

- L. 255: Did the author make sure that the calibration of the nacelle mounted anemometer was proper and that measurements were in the calibrated wind speed range/operating regime?

- L. 292: Why is there no difference between the positive and negative shear (P_{std}/P_{rated}) up until 0.6 of the $\bar{u}_{\infty}/u_{rated}$? This should be investigated in the discussion.

- Figure 7: why is the order of these figures switched, please maintain consistency as in Fig.6, where $2.6D$ is first, all range gates is second and the last is REWS. Also maintain consistent title for the sub-figures between Fig.5 and Fig.6. Consider adding confidence intervals to the curves for better statistical interpretation.

- L. 311ff: Please remove this paragraph, as this does not discuss any results and has some conclusions.

- L. 325ff: these sentences are confusing, you say the standard recommends pre tilt, and you also do the same, but the sentence in between states what happens when there are no beams at hub height. Please organise this paragraph to properly discuss your results.

- L. 328ff: This point comes completely out of nowhere, but raises an interesting question. Did the authors consider estimating the TI and relate them to the power production, as this is shown to have an impact as well.

- Appendix B: Consider adding a figure showing the difference in the height of the two beams as a function of the pitch/ roll angle. This adds more to the interpretation of the FOWT motion impacts on the measuring locations.

Technical comments

There are several issues on typography such as incorrect reference formatting throughout the manuscript that should be fixed. Some of them are highlighted below:

- L. 19, L.21, L.30 and others: Please use the correct way of citing, i.e. with or without parentheses

- L. 23: "th" should be corrected to "the"

- L. 116ff: The grammar and build up of this sentence is wrong, please rephrase this part.

- L.125: It should read "post-processed" not "post-processing"

- Eq.3: Denominator is incomplete, hanging parentheses.

- L.175, L. 180: Here, a "," is used after "Figure". I suppose a backslash is missing to indicate a half-space character.

- L. 178 (and other parts): Pleas make sure to use either "line of sight" or "line-of-sight" consistently

- L. 227: It should be $\sqrt{\overline{u}_{\infty}^2 + \overline{v}_{\infty}^2}$ instead of $\sqrt{\overline{u}_{\infty} + \overline{v}_{\infty}}$

- Line 295: Should it not say $2.6D$?

- L. 344: "A1" is mentioned twice here.

References

[1] Nikolas Angelou, Jakob Mann, and Camille Dubreuil-Boisclair. Revealing inflow and wake conditions of a 6 mw floating turbine. *Wind Energy Science*, 8(10):1511–1531, 2023.

[2] Johannes Paulsen, Jörge Schneemann, Gerald Steinfeld, Frauke Theuer, and Martin Kühn. Low-level jets' influence on the power conversion efficiency of offshore wind turbines. *Wind Energy Science*, 11(2):321–346, 2026.

[3] Gerard Schepers, Pim Van Dorp, Remco Verzijlbergh, Peter Baas, and Harmen Jonker. Aeroelastic loads on a 10 MW turbine exposed to extreme events selected from a year-long large-eddy simulation over the North Sea. 6:983–996.

[4] Patrick Murphy, Julie K Lundquist, and Paul Fleming. How wind speed shear and directional veer affect the power production of a megawatt-scale operational wind turbine. *Wind Energy Science*, 5(3):1169–1190, 2020.