Answer to Reviewer 2

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We would like to thank both reviewers for their thorough review of our manuscript. They have raised a number of points that require clarifications or corrections in the manuscript. Please find below the comments of the Reviewer 2 (using blue fonts), along with our corresponding answer to each comment (using black fonts).

General comments

1. 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 (Angelou et al., 2023), 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.

We agree with the reviewer that more information should have been added here. This was partly done to not repeat a lot of information already mentioned in the article Angelou et al. (2023). The first paragraph of Section 2.1 is now rewritten and a new figure has been added that also shows the two rotations that have been considered in this study. The updated paragraph is the following:

The wind turbine examined in this study is one of five floating offshore wind turbines (FOWTs) that form the Hywind Scotland wind farm. The wind turbine positions (labelled HS1, HS2, HS3, HS4, and HS5) are arranged in a W-shaped configuration, rotated 30° clockwise relative to true north, as illustrated in Fig. 1. The configuration is described within a right-handed coordinate system, where the y-axis is oriented toward the north, and the origin is defined at the location of the HS4 turbine. The FOWTs (SWT-6.0-154, Siemens Gamesa Renewable Energy) have a hub height of 98.6 m and a rotor diameter of D=154 m. They are mounted on floating platforms based on a ballasted spar buoy concept and have been operating since 2017 Jacobsen and Godvik (2021). Below the rated wind speed, blade pitch is regulated in the same manner as in a conventional bottom-fixed wind turbine. Above the rated wind speed, however, the blade pitch control operates in coordination with the floater motion control system.

Furthermore, we have added the information about in which wind turbine was the nacelle-mounted wind lidar installed.

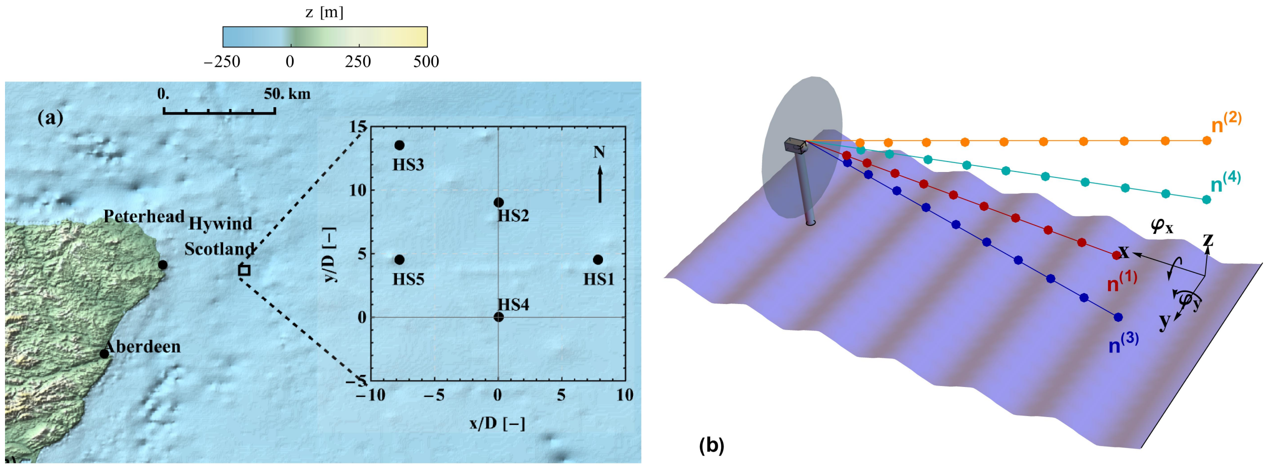

Figure 1. (a) Map of Scotland's eastern coastline indicating the offshore Hywind Scotland wind farm location (black rectangle). A schematic of the wind farm layout, comprising five turbines (labelled HS1, HS2, HS3, HS4, and HS5), represented in a right-handed coordinate system with the y-axis oriented toward the north and the origin located at the HS4 turbine. (b) Schematic of the wind turbine showing the four nacelle-mounted Doppler lidar line-of-sight directions ($\mathbf{n}$) and the coordinate system used to describe rotations about the x- and y-axes, corresponding to nacelle pitch and roll rotations.

2. 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 modelled, courtesy of find. 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.

We have tried to clarify the considerations and assumptions stated here in the revised manuscript. In specific:

(a) We state that the x-axis is always horizontal and does not follow the tilt of the wind turbine rotor.

(b) Regarding the flow homogeneity we first consider that the free flow at a height z is spatially homogeneous. Subsequently we take into consideration that the flow is distorted only along the x-axis as it approaches the wind turbine.

(c) We explain more the parameters of Eq. 1.

(d) We explain why we use the same equation also to transverse locations outside the rotor. We agree with the reviewer that the rotor induction widens with upstream distance, however we don't know the characteristics of the induction zone expansion. We decided to apply the same model in all the range gates to avoid selecting a specific radial distance where the inflow is undisturbed by the induction zone. This choice is not expected to have a significant impact on the calculations of neither the induction factor nor the two horizontal components of the free flow at the hub height. Besides the revision of Section 3, we have added the following sentences in the last paragraph of the *Discussion* section:

In this study the induction zone was modelled using a simplified parameterization based on the induction factor and the upwind distance. Radial variations of the induction factor along the rotor were not considered. Furthermore, the application of the induction model was applied to all range gates, even if those were located outside the rotor area. Therefore since we don't know how do the dimensions of the induction zone expand with upstream distance we cannot be sure that the wind conditions at the further measured distances (i.e. 2.08D – 2.60D) were distorted by the operation of the wind turbine. Nevertheless, the impact of this assumption in the estimation of the induction factor and of the two horizontal wind components at the hub height is considered to be negligible.

3. 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.

Our intention with Fig. 3 was to present the value that are derived by the model of Eq. 1. Following the suggestion of the reviewer we have added here two sub-figures that present the shear and veer of the wind profile at the top part of the rotor and we have expanded the discussion of the results in Section 4.2

4. 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.

First, regarding the power curve comment, we agree with the reviewer that ideally the power curves presented in this study should be compared to the theoretical power curve of the specific FOWT, however we don't have access to it. We have added one sentence in the manuscript to clarify this. Second, our objective was not to present that the FOWT's power production was reduced in relation to the fixed-bottom, but rather to highlight that wind profile inversions that can frequently occur offshore can lead to significant differences in a power curve verification when the hub-height wind speed is estimated. The use of the power curve of the fixed-bottom wind turbine as a reference was used to show that the power curves of FOWTs should be expected to be different from the fixed-bottom ones. Lastly, since the wind lidar observations in this field study have been acquired only on the top part of the rotor it is not possible to estimate a rotor equivalent wind speed and thus study in detail the relationship between power production and wind speed. We have have highlighted this in the discussion and also changed the discussion of the results of our article to clarify the aforementioned points.

5. 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.

To address the valid points raised by the reviewer we have re-written the Discussion. One of the things added is a discussion about the comparison of the frequency of the LLJ appearance in our study with results presented in already published studies. Regarding the impact of the LLJs on the AEP, we think that we cannot perform such an analysis, since first our data set is representative on specific months, and thus the statistical frequency of these events is not necessarily the same throughout a whole year. Second, as mentioned in our answer to the previous comment, since the wind lidar observations have been acquired only on the top part of the rotor it is not possible to estimate a rotor equivalent wind speed and thus study in detail the relationship between power production and wind speed. Lastly, in connection to also the *Specific Comment* 24 we have removed the 2nd last paragraph of the *Discussion* section in the original manuscript.

6. 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?

We have added the following sentence in the *Conclusions* section:

The wind profile characteristics at the North Sea over two seasons (summer and autumn) are studied using observations acquired by a nacelle-mounted wind lidar installed on the Hywind Scotland wind farm. We found that, in 88.5% of the cases, modelling the radial speeds using a linear wind shear and veer across the rotor, along with an induction factor representing turbine operation, provided satisfactory results within the examined height range (100 m – 200 m). The cases in which the model failed were associated with wind speed inversions occurring within the lowest 200 m of the atmosphere.

Specific comments

1. 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.

   Following the suggestion of the reviewer, we:

   (a) Added the two suggested references in the Introduction and furthermore we added the references that present the study of LLJs using wind lidars and/or the impact of LLJs on the power production.

   (b) A discussion of the results of already published studies in relation to the results of this study has been added in the Discussion session.

2. 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.

   This is paragraph is re-written in the revised manuscript.

3. 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.

   Following this suggestion of the reviewer as well as the *General Comment 1* we have revised the Section 2.1 and we added a figure showing the two degrees of freedom examined in relation to the coordinate system used in this study. We understand the concern of the reviewer, but we think that the variation of the pitch and roll angles could not be simply be split to below rated, rated and above rated regimes, since they are both a function of wind speed. We have referred to the article of Angelou et al. (2023) in which a figure depicts the mean pitch and roll angle as a function of speed. •

4. 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.

   Please see our answer to your first general comment.

5. 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 $9/circ$ for a selected 10-min period? Please elaborate on this in the discussion section.

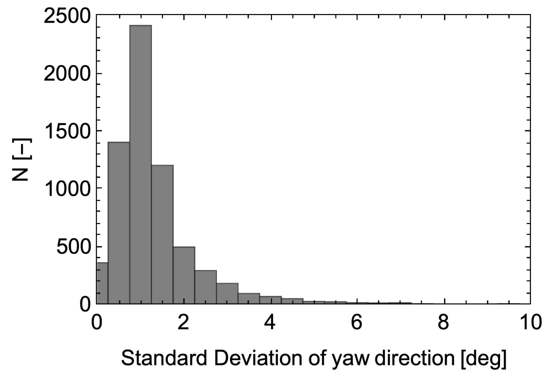

Figure 2. Histogram of the standard deviation of the yaw direction.

Overall, the standard deviation of the yaw was very low (i.e. average value of less than 3 degrees). However, there were few cases where the standard deviation was large. A histogram of the standard deviation is presented below:

We have added the following sentence in second paragraph of the Discussion:

Another source of uncertainty in estimating the hub-height wind speed is the dynamic variation of the yaw direction. To minimize potential biases, only periods with a yaw direction standard deviation below 10° were considered. This threshold was chosen empirically. The resulting dataset is characterized by an average yaw direction standard deviation of 2.6°, indicating that the impact of yaw motion variability is expected to be minimal.

6. 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.

One scan was performed every 4 seconds, so in total 150 measurements were acquired per range gate during a 10-minute period. This information is now added in Section 2.2. The RMSE was calculated using the 10 minute mean values per range gate of each of the four line of sights. Therefore 40 measurements were used for the estimation of the RMSE. We have clarified in the end of the Section 3 and we have added in the Section 2.2 information regarding the data availability.

7. 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?

Following a comment of the Reviewer 1 we have re-written a part of this paragraph to clarify the data availability as following:

In total data 13,247 ten-minute periods of SCADA data and 17,057 equivalent periods of wind lidar were available. However, only in 6,729 periods concurrent data were available..

Furthermore, we have edited the last sentence of the section 2.3 as following:

The wind lidar data were filtered based on the data availability at each range gate. Only periods with at least 50% were selected which reduced the initial wind lidar data set by 10%.

8. 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.

We have updated the figure by tilting the rotor in both subplots.

9. 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.

Please find our answer to the *Specific Comment* 7.

10. 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?

The wind vector is defined in a fixed coordinate system, where the x-axis is always horizontal. We have added a clarification of this.

11. 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?

We assume that the free inflow at a specific height z is independent of the y coordinate. As the free inflow is approaching the distance, then due to the induction zone of the wind turbine the wind speed is decreased. The estimation of the wind profile characteristics is performed every 10 minutes by taking into account that each range gate is acquired in different upwind distances in front of the rotor. We have revised the Section 3 to clarify this part.

12. L.140: Please clarify if the notation of veer ∂v/∂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.

We have re-written this part, as well as the rest of the manuscript, to address $\partial v/\partial z$ as the gradient of the transverse component not as the veer.

13. 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?

The wind profile is reconstructed using all measurements, since different ranges gates are acquired at different heights. We have tried to clarify this part.

14. 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.

We agree with the reviewer, that in the original manuscript the objective and the aim of the study was not clearly stated. Following the *Specific Comment* 2 we have reformulated the Introduction and we have reformulated the first sentence of the Section 4.1.

15. Figure 2: Please add a reference to Figure 2 for the color scheme and describe what the dots and lines represent in the caption.

Following the suggestion of the reviewer the figure and the caption have been updated.

16. L. 178-179: Could you please elaborate further on what deviations become larger with the higher beams due the wind veer?

The comment is those lines is a a general one. In the presence of veer the radial wind speed of the two upper beams will have larger deviation. The two sentences between the lines 177 and 180 of the original manuscript are now re-written to clarify this point as following:

In general, when the turbine yaw is aligned with the wind direction, the two lower beams measure the same line of sight velocity at any distance. Vertical shear results in higher radial speeds for the upper beams compared to the lower beams, *while vertical veer causes larger differences between the upper beams' radial speeds because wind direction changes with height.*

17. 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.

We are not sure if we have understood correctly the comment of the reviewer. We have drawn a bar-chart of the rmse values per month, where it is shown that the higher values are found during the months from June to August (see figure below). If the reviewer thinks that this figure is relative we can add it as an Appendix in our manuscript.

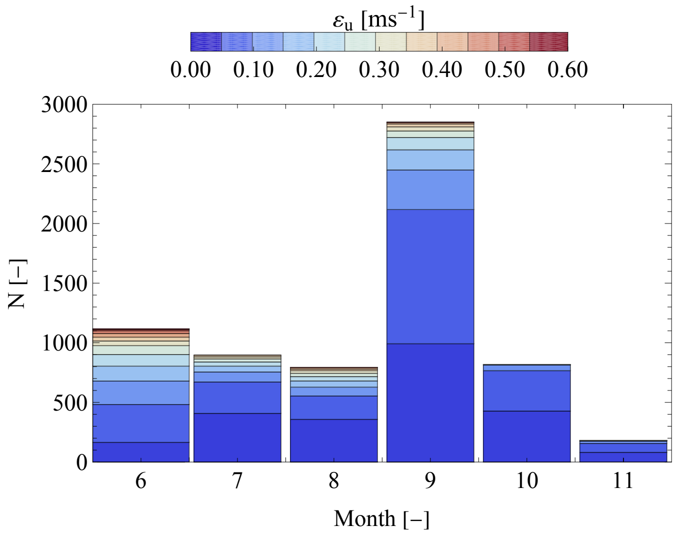

Figure 3. Bar-chart of the RMSE values of the model used to predict the radial wind speed measurements of the nacelle-mounted wind lidar per different months.

18. 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.

The sonic anemometer wind speed is corrected using a transfer function to compensate for the effect of the rotor. This correction is performed by the wind turbine manufacturer. We have added one sentence to state this. In addition we have removed from this plot the REWS because it is not straightforward to calculate that when a wind speed inversion characterizes the wind profile. We elaborate a bit more on this in Section 4.4 point 3 where the REWS estimation is discussed.

19. 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).

In the original manuscript the Fig. 5(a) was presenting the duration of the periods in which wind profiles with inversions or wind profiles with negative shear were detected. These periods were selected based on the criterion that these wind profile characteristics were observed during at least 30 minutes within one hour. While the Figs. 5(b) and (c) presented 10-minute data. We have clarified this in the text and in the caption and also changed the order of the sub-figures. Furthermore, the dotted and dashed lines are now described.

20. blueL. 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]).

The decreases in the power production were related to both wind shear and veer. The Reviewer 1 had also a similar question. We have expanded the discussion of our results to comment on that and we have also added the reference suggested by the reviewer.

21. 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?

Unfortunately we don't have any information regarding the calibration of the nacelle-mounted anemometer, so the results of the calculation of the vertical offset of the lidar are dependent on the assumption that the nacelle-mounted anemometer is calibrated properly. However, we would like to note here that that the estimated height offset is in a good agreement with the measured location of the nacelle-mounted wind lidar.

In the following figure we present the agreement of the estimated hub-height wind speed and of the REWS with the wind speed measured by the nacelle-mounted anemometer for different wind profile characteristics. We observe that in the cases where a negative shear is found the deviation between the hub-height wind speed based on the nacelle-mounted Doppler lidar and the corresponding ones of the sonic anemometer increase.

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

We dont know the explanation of this trend. Our hypothesis is that this increase in the standard deviation could be partially attributed to the fact that the hub-height wind speed estimation that we have is not necessary representative of the RWES in the case of wind profiles with a negative shear. We have added a comment to state this both in the *Results* and the *Discussion* section.

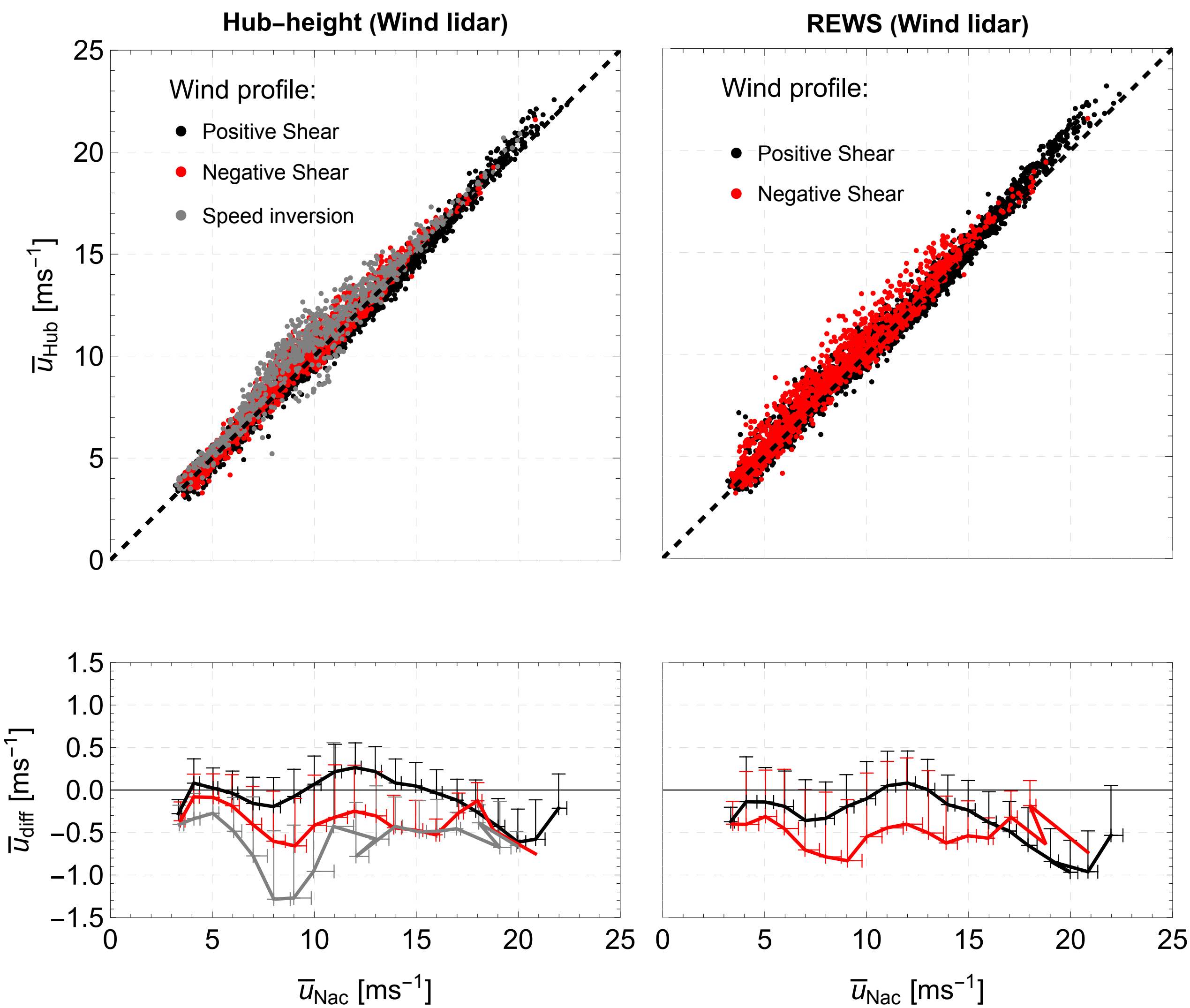

Figure 4. Comparison the estimated hub-height wind speed and of the REWS with the wind speed measured by the nacelle-mounted anemometer

23. 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.

The order of the figures is correct, but the legends were wrong. This has been corrected in the revised version. Following the suggestion of the reviewer (General comment 4) we have added the 95% confidence interval in Fig. 7.

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

Following the suggestion of the reviewer this paragraph is deleted. Part of this text is moved to the *Conclusions* section.

25. 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.

We agree with the reviewer that this paragraph was not written well. It has been revised and a clarification regarding the tilt of the optical axis of the wind lidar has been added.

26. 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.

We have re-written this part of the discussion. In our study we cannot get a direction estimation of TI. However, we could get an estimation of the turbulence conditions of the inflow by checking the standard deviation of the radial velocity of the furthest range gate of the two lower beams (which were approximately horizontal). We have added a corresponding plot in the Appendix D. We find that in the case of the wind profiles with a positive shear turbulence is on average marginal higher than in those cases with a negative shear. Given the observed small difference, the fact that we have measured the standard deviation of the radial speed and not of the TI, as well as since we dont have information about the wind conditions in the lower part of the rotor, we did not consider estimating TI and relating it to the power production.

27. 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.

Following the suggestion of the reviewer such a plot is added in the Appendix C.

Technical Comments

1. The reference formatting was checked and corrected throughout the manuscript.

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

Corrected

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

   The sentences between the lines 116 and 119 of the original manuscript are now re-written as following:

   The sentence is re-written in the revised version of the manuscript.

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

   Corrected

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

   Corrected.

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

   Corrected.

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

   The correction that we have applied throughout the manuscript is that the hyphen is used when the line of sight is used before a noun (line-of-sight speed) and without a hyphen when it is used as a noun.

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

   Corrected.

9. Line 295: Should it not say 2.6D?

   Yes, the reviewer is right.

10. L. 344: ”A1” is mentioned twice here.

    Corrected.

References

Angelou, N., Mann, J., and Dubreuil-Boisclair, C.: Revealing inflow and wake conditions of a 6 MW floating turbine, Wind Energy Science, 8, 1511–1531, https://doi.org/10.5194/wes-8-1511-2023, 2023.

Jacobsen, A. and Godvik, M.: Influence of wakes and atmospheric stability on the floater responses of the Hywind Scotland wind turbines, Wind Energy, 24, 149–161, https://doi.org/10.1002/we.2563, 2021.