# Answer to Reviewer 1

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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 1 (using blue fonts), along with our corresponding answer to each comment (using black fonts).

**Major comments:**

– Line 55: It may also be worth mentioning that nacelle-mounted lidar systems are generally simpler to install and more cost-effective than floater-based lidar systems. In addition, the lidar beams can be aligned with the longitudinal (mean wind) direction, which minimizes mismatch with longitudinal wind fluctuations. This is particularly important for accurately estimating freestream turbulence intensity at hub height (Pls. see https://doi.org/10.1016/j.marstruc.2026.104019).

The reviewer highlights that nacelle-mounted Doppler lidars have the advantage of a simpler installation and more cost-effective operations than the floater-based Doppler lidars which is one of the alternatives. We acknowledge the comment of the reviewer but we think that we should not add such a statement in the introduction. The reason is that first the installation and the alignment of a nacelle-mounted Doppler lidar on a floating offshore wind turbine can be a challenging task and the second is that despite that we agree with the reviewer that nacelle-mounted Doppler lidars are cheaper than floating-based Doppler lidars we cannot provide a reference to this statement.

Regarding the comment of the detection of the longitudinal free stream turbulence intensity we think that it is a good suggestion to add such a statement. The sentence in Line 55 is now re-written as following:

*First, the optical axis of nacelle-mounted wind lidars follows the yaw direction of the nacelle, thus maximising data availability (Wagner et al., 2014), and second since they provide measurements at different heights, necessary for the estimation of a REWS. Furthermore, nacelle-mounted wind lidars that include a levelled line of sight parallel to the yaw direction can provide estimations of the turbulence intensity of the wind (Peña et al., 2017; Guo et al., 2026). This makes nacelle-mounted wind lidars an alternative option to floating wind lidars for offshore measurements, in the context of a PCV.*

- Line 124: There appears to be a significant reduction in the available lidar data. Could the authors clarify the data selection criteria? For example, is there a minimum data availability threshold within each 10-minute interval (e.g., for all range gates), and how is this applied?

We would like to thank the reviewer for this comment, because our statement in the original manuscript was misleading. In the original manuscript we wrote: *From the 13,247 ten-minute periods identified, lidar data were available for 6,729 periods.* The number of 6,729 periods is not a result of a filtering process but it concerns period that both SCADA and wind lidar data were available. A reduction in the system availability could be the outcome of either that the wind lidar was not operating or it was operating but data were not stored or that the SCADA data were not available. We cannot be sure exactly for which reason this occurred. To clarify this point the sentence is re-written as following:

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

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%.*

- Figure 6: It is recommended to clarify how the power curve is calculated. For example, is it based on bin-averaged statistics? Additionally, the reference power curve is not clearly defined. Is it obtained from an aero-hydro-servo-elastic simulation assuming a rigid rotor and tower, or does it include all dynamic effects of the system?

We didn't have access to a reference power curve for a floating wind turbine. The power curve used was a model of a fixed-bottom wind turbine of the same type as the ones used in Hywind Scotland. The power curve was estimated using 0.5-ms$^{-1}$ bins. The following sentence has been added in the line 280 of the original manuscript:

*In this context, power and wind speed data were grouped in 0.5-ms$^{-1}$ bins and mean and standard deviation statistics of the produced power were estimated which are presented in Fig. 6. The mean power is compared to a reference power curve model of a fixed-bottom wind turbine of the same type as the one installed in Hywind Scotland.*

- Figure 6 conveys important information from the current study. It would be interesting to plot the power ratio as a function of the wind shear gradient to examine whether the power discrepancy is related to the magnitude of negative shear.

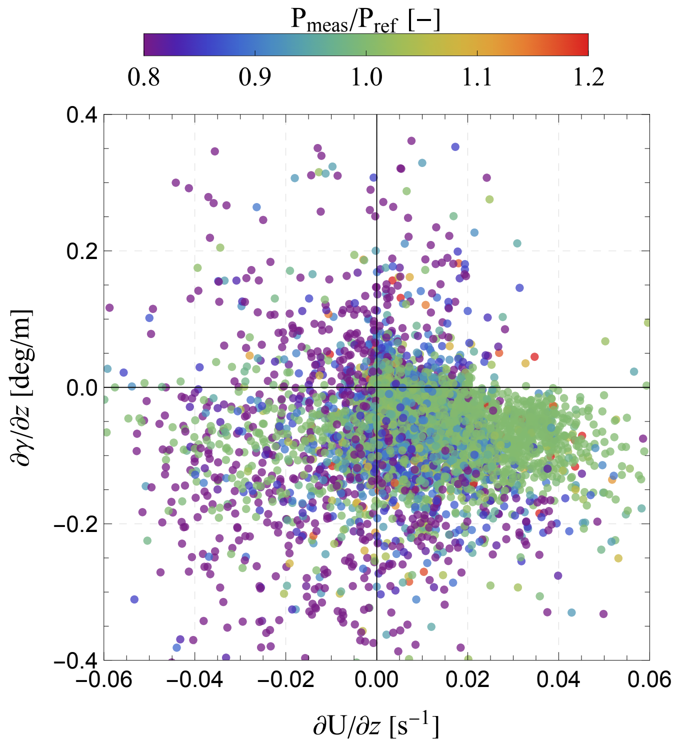


**Figure 1.** Power ratio, measured power divided by the theoretical produced power using a hub height wind speed, for different wind shear and veer values estimated over 10-minute periods.

The power curve results in Fig. 6 are going to be dependent on the accuracy of the hub-height wind speed. Since we don't have measurements at the lower part of the rotor it is not straightforward to assess what were the REWS in the case of the wind profiles with a negative shear. Therefore the main message that we want to convey with this plot is that the power curve verification offshore should not rely on solely on hub height wind speeds.

Following the suggestion of the reviewers we plot the power ratio (measured power divided by the theoretical produced power using a hub height wind speed) as a function of both the shear and veer of the wind. The results are presented in the following figure. We have decided to add this figure in the Appendix and added the following comment on the end of the line 295:

*The variation of the produced power are attributed to both the shear and veer values of the wind profile. In the examined dataset negative shear values are usually related to negative veer values which results in reduction to the power production (see Appendix C). This result is in agreement with the findings reported by Murphy et al. (2020) and Paulsen et al. (2026) for case of onshore and offshore fixed-bottom wind turbines, respectively.*

– In Figure 6, it appears that negative shear leads to higher power standard deviations. It would be interesting to plot the power spectra to identify the differences in fluctuations. Under negative shear conditions, wind speed decreases closer to the sea surface due to surface friction. A blade operating in a low-level jet (LLJ) can experience enhanced 2P excitations: First period: the mean wind speed varies from maximum at hub height to lower values upward and then returns to maximum at hub height. Second period: the mean wind speed varies from maximum at hub height to lower values downward and then returns to maximum at hub height. (The wind speed is lower downwards due to the effects of tower shadow and positive shear near the surface) . Therefore, it would be insightful to determine whether the additional fluctuations in power are caused by these 2P effects or are primarily due to high ambient turbulence intensity at lower frequencies.

The standard deviation values presented in Fig. 6 correspond to the standard deviation of the 10-minute mean power estimations per wind speed bin. Therefore the values don't correspond to the dynamic power fluctuations per 10-minute period. We have clarified this part in the revised manuscript.

A wind profile with a negative shear will have higher wind speeds as approaching the sea surface until there is a point where a wind speed inversion will occur. The aerodynamic response of the FOWT and its overall power production will be impacted by the relative height of the wind speed inversion in relation the FOWT's rotor. Since, in this study we don't have measurements of the wind profile at the lower part of the rotor and thus we cannot identify where exactly the wind speed inversion in the cases of the wind profiles with negative shear occurs. We agree with the reviewer that it would be very interesting to study the dynamic production of the wind turbine during the events with negative shear, but we think that we don't have all the required information to perform a thorough analysis on this.

– Based on the reviewer's understanding, even under positive or negative shear, the theoretical power curve of a floating wind turbine would generally be lower than that of a bottom-fixed turbine at below-rated wind speeds. As mentioned by the authors, the thrust force can induce turbine tilt of several degrees. Combined with rotor tilt, the total tilt of the rotor relative to the mean wind direction could approach 10 degrees, which affects the net longitudinal wind speed experienced by the rotor. In addition, turbine structural flexibility also influences the theoretical power curve. Therefore, it is important for the authors to clarify how the reference power curve is obtained. For example, are shaft tilt, pre-cone, and blade and tower flexibility considered in the calculation?

The tilt of the rotor should reduce the overall power production of a FOWT in relation to fixed-bottom wind turbine. This trend is observed in Fig. 7. The theoretical power curve included in Fig. 6 is intended to be used as a reference and its use is not aim to assess how well a FOWT operates. Unfortunately we don't have the information that the reviewer has requested regarding the parameters included in the calculation of the power curve. We have added the following comment in the Discussion of the revised manuscript.

*In our study we find a decrease in the power curve between a FOWT and the theoretical modelled of fixed-bottom wind turbine. This difference could be partially attributed to the added tilt angle of the rotor during the operation of a FOWT. However, the observed deviations between the reference power curve and the measured one during occasions with a* *positive wind shear point to the direction that the offshore wind characteristics can have a significant impact on the performance of a FOWT. For example, the aerodynamic response of the FOWT and its overall power production will be impacted by the relative height of the wind speed inversion in relation the FOWT's rotor and the shear and veer above and below the speed inversion (Paulsen et al., 2026). Since, we don't have measurements of the wind profile at the lower part of the rotor and thus we cannot identify where exactly the wind speed inversion in the cases of the wind profiles with negative shear occurs and thus we cannot provide an representative estimation of REWS which would enable a more thorough study of the power production of the FOWT.*

– Conclusion: The current conclusion states that negative wind shear and wind speed inversions influence turbine power production, but it is not clear how they affect it. Based on the authors' observations, negative shear or wind speed inversions can result in maximum or elevated wind speeds at hub height. However, the turbine does not rely solely on hub-height wind speed to produce power; it responds to the rotor-equivalent wind speed (REWS). If hub-height wind speed is used to derive the power curve, which may be higher than the actual REWS, a weaker-than-expected power curve will result. It is recommended that the authors emphasize in the conclusion that using hub-height wind speed for power performance verification offshore may have inherent limitations.

We agree with the reviewer that the power production of a wind turbine depends on the rotor-equivalent wind speed (REWS) and that our statement in the *Conclusion* section needs a clarification. The sentence:

*These phenomena occurred in 22% and 11% of the examined cases, respectively and significantly influenced turbine power production, in respect to the corresponding power curve of a fixed-bottom wind turbine, particularly under negative shear.*

is now re-written as following:

*These phenomena occurred in 22% and 11% of the examined cases, respectively and can have a significant impact on a power curve verification procedure that is based solely on hub-height wind speed without taking into consideration the variations in the vertical wind profile. We show that using only the hub-height wind speed as a reference wind speed to power curve verification leads to a reduced turbine power production, in respect to the corresponding power curve of a fixed-bottom wind turbine, particularly under negative shear.*

**Minor comments:**

1. Line 25: The statement "only at two locations in the North Sea" appears to be too absolute. There are other platforms, such as the Inch Cape Meteorological (Met) Mast in the UK, that should also be considered.

The corresponding sentence is now re-written as:

*For this reason, tall (≥ 100 m) offshore masts equipped with in-situ wind sensors are currently installed only at few locations mainly (e.g. the FINO – Research platforms in the North Sea and Baltic Sea (FINO, 2025) and the meteorological mast in the Inch Cape Offshore Wind Farm).*

2. Line 55, maybe also mention that the induction effect to nacelle lidar needs to be compensated as well.

The following sentence is now added in Line 57 of the original manuscript:

*A general complication in performing a PCV using a nacelle-mounted wind lidar is that the impact of the induction zone of the wind turbine should be taken into consideration in the case that measurements acquired at different distances from the rotor are used to parameterize the inflow conditions.*

3. Line 55: In addition to different measurement heights, the measurements in different radial positions are possible.

The following statement is added:

*...they provide measurements at different heights and radial distances,...*

4. Line 65: There is prior work using motion-compensated lidar measurements to assess the power performance of a 2 MW barge-type floating wind turbine (DOI: 10.1088/1742-6596/2265/4/042016). It would be beneficial to reference this study.

The recommended article from the reviewer is already referenced in our manuscript. However, to our understanding this study doesn't perform a motion compensation to the nacelle-mounted wind lidar measurements.

5. Lines 78, 80, 86, 143: The citation format is inconsistent (e.g., should follow "Jacobsen and Godvik, 2021"). The authors are encouraged to check and standardize all citations throughout the manuscript.

The citation format is corrected in the revised manuscript.

6. Line 79: It would be clearer to explicitly specify "platform pitch" to avoid confusion with blade pitch.

The word nacelle is added to the sentence.

7. Line 158: In the Trondborg and Meyer model, the induction effect also depends on the radial position; this could be clarified.

The following sentence is added in the Line 158 of the original manuscript:

*..., which considers the radial distance in the parametrization of the induction factor $a$.*

8. Equation (3): Does the formulation account for the mean platform pitch angle in the vertical wind profile subject to induction? Additionally, there appears to be a missing bracket in the expression.

Equation 3 takes into account the pitch and roll angle in the calculation of the unit vectors of each line-of-sight. The induction factor value used is based on the results of the fitting of Eq. 1 to the measured data. This have been clarified now in the text. The missing parenthesis is now added.

9. Line 182, Fig. 2(c): There is a typographical error in the symbol ";".

The error is corrected.

10. Figure 2: Readability would be improved if the legends clearly distinguish between solid lines and scatter points.

A legend has been added to the plot Fig. 2(a) to explain that the measurements of each line of sight are denoted using dots. In the label of the figure a description of the solid lines has been added.

11. Line 191: Equation (1) appears to provide an analytical expression for RMSE, but the calculation procedure is not clearly explained. It is recommended to clarify how RMSE is computed. Additionally, this seems to be the first occurrence of RMSE, while its definition appears later in the text.

The first mentioning of the root-mean-square error is in the line 160, where it is also explained how is it calculated. We have added the abbreviation in that line as well as we added the information the Eq. 1 is used to generate the predictions of the radial wind speeds.

12. Equation (1): The variables and should be clearly defined

In the explanation of Eq. 1 it is now added a description of the unit vector components.

13. Line 205: LLJ is defined multiple times; it would be better to define it once and use the abbreviation consistently thereafter.

The abbreviation LLJ is defined for the first time at the line 29 of the original manuscript. We have removed any repetition of the definition that was included in the manuscript.

14. Line 245: Is the sonic-based wind speed corrected using a nacelle transfer function? It is recommended to clarify whether the induction effect is also considered for the sonic measurements.

The sonic-based wind speed is corrected using a nacelle transfer function. We don't know exactly how is the correction performed. However from the results of the comparison between the nacelle-mounted wind lidar and the nacelle-mounted sonic anemometer that was presented in our previous study (Angelou et al., 2023) we can draw a conclusion that the induction effect is considered when the correction is applied.

We have added the following sentence in the line 243 of the original manuscript:

*We note here that a transfer function for correcting the effect of the rotor has been applied by the wind turbine manufacturer.*

15. Line 267: The nacelle anemometer is typically mounted above hub height. Is this measurement treated as the ground-truth hub-height wind speed? Please clarify.

The height offset was taken into account in the calculation of the hub-height wind speed. This is describe in the first paragraph of the Section 4.4

16. Figure 5: The black solid and dashed lines should be clearly explained in the legend or caption.

An explanation has been added to the legend.

17. Line 315: For continuous-wave (CW) lidar focusing at short distances, induction effects may also be significant and should be discussed.

We agree with the reviewer. This is why in the sentence at the line 316 we wrote: *However, the latter requires incorporating line-of-sight measurements within the induction zone of the turbine into the parametrisation.*

We have now re-written the Discussion and added the following comment:

*However, the latter requires incorporating line-of-sight measurements within the induction zone of the turbine into the parametrisation, and thus a model of the induction factor is necessary.*

18. Conclusion: It is recommended to specify the number of cases analyzed in the study.

Following the recommendation of the reviewer we have added the following sentence in the *Conclusion* section:

*These phenomena occurred in 22% and 11% of the examined data set which had an effective length of approximately 46 days (i.e. 6659 10-minute periods) spanned over summer and winter months.*

## References


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