Author response to reviewer 1

The authors response is shown in red
Changes implemented in the new version of the manuscript are in blue

We thank the reviewer for the comments, which we consider very important. Here our response to each of them.

More motivation for the presented work should be included. For example, I was wondering how accurate current IEC-recommended methods for estimating loads in waked conditions are, and if there is a clear need to improve these methods using lidar-measured wind conditions. Additionally, I was wondering why the authors did not compare the accuracy of the load predictions to the accuracy when using these IEC standard wake modeling methods. It was not until the end of the paper (last paragraph of Section 5) that these topics were discussed. I believe this material should be moved to the introduction to better motivate the research and to explain the scope of the current work (i.e., that comparison with DWM, etc. is not part of the current work).

Following the comment of the reviewer, we now provide further motivation on the need to improve load validation approaches in wakes, and clarify the scope of the work along the manuscript. Further, "Sect. 2.1 Requirements for load validation in wakes" is fully dedicated to define the scope of the current work and introduce the reader with the elements of the load analysis.

The following text is added on page 2 line 9:

The comparison of fatigue loads predicted using the DWM and the effective turbulence approach by the IEC showed a discrepancy of 20% (Thomsen et al., 2007). The uncertainty varied according to the inflow conditions and spacing between turbines. The work of Larsen et al. (2013) showed a very fine agreement between both power and load measurements and predictions based on a site-specific calibrated DWM model for the Dutch Egmond aan Zee wind farm. However, the study did not quantify uncertainty in a systematic approach. More recently, Reinwardt et al. (2018) estimated fatigue load biases in the range 11–15% for the tower bottom and 8–21% for the blade-root flapwise bending moments using the DWM. To date, these approaches are characterized by a significant level of uncertainty, due to the stochastic nature of environmental conditions and the various simplifying assumptions used in the wake model definitions (Schmidt et al., 2011). Further, these results motivate the need for improving wind turbine load validation approaches in wake conditions.

The following replacement is done in the abstract on page 1 line 4:

From: "We account for wake-induced effects by means of wind field parameters commonly used as inputs for load simulations, which are reconstructed using lidar measurements."

To: "The wake flow fields are described by lidar-estimated wind field characteristics, which are commonly used as inputs for load simulations, without employing wake deficit models."

The following text is added in the introduction on page 2 line 21:

Based on these findings, we extend the load validation procedure defined in Dimitrov et al. (2019) to include wake conditions. Therefore, wake-induced effects are accounted for by means of wind field parameters commonly used as inputs for load simulations, which are reconstructed using lidar measurements, yet without employing wake deficit models.

-Many of the paragraphs throughout the manuscript are very long, often an entire page. Organizing them into smaller paragraphs would greatly improve the readability. For example, pg. 2, pg. 11, pg. 14, pg. 17.

To improve readability, we restructured the manuscript as following:

- Section 4.1 Wake effects on reconstructed wind parameters
- Section 4.2 Wake effects on turbulence spectra properties

We renamed section 4.3 from “Load simulation results” to “Load validation procedure” and re-arranged the section 4.3 as:

- Section 4.3 Load validation procedure
- Section 4.3.1 Power predictions
- Section 4.3.2 Extreme load predictions
- Section 4.3.3 Fatigue load predictions

“Sect. 4.4 Sensitivity analysis” is now divided in:

- Sect. 4.4 Sensitivity of inflow parameters on load predictions
- Sect. 4.4.1 Uncertainty distribution as function of wind speed

Specific comments:

1) Pg. 1, ln. 10: ": : :lead to an increase of the relative error as low as 4%" Kind of a confusing sentence. Maybe something like "only increase the relative error by 4% in some cases"?

The following text is modified in the abstract on page 1 lines (10)

From: “Compared to the reference case, the simulations in wake conditions lead to an increase of the relative error as low as 4%”

To: “Compared to the free-wind case, the simulations in wake conditions lead to increased relative errors (4–11%).”

2) Pg. 2, ln. 2: "To account for these effects, aero-elastic load simulations are combined with wake models: : :" How are lifetime fatigue loads calculated? Are the fatigue loads with and without the added wake turbulence/wake models added together, weighted by the frequency of occurrence for waked and freestream conditions?

In our opinion the current best practice is to derive lifetime fatigue loads based on site-specific conditions, e.g., using mast measurements, if available, or assuming appropriate probability distribution of the wind conditions at the site. With knowledge of the ambient conditions and the wind direction distribution in relation to the wind farm layout, the lifetime fatigue loads can be derived by weighting the frequency of occurrence of waked and free-stream conditions. Here, we derive the 1-Hz damage equivalent load for each 10-min period, without computing lifetime fatigue loads. However, the proposed approach could also provide information on the frequency of occurrence of waked and free-stream conditions and it can be extended to derive lifetime fatigue loads.

3) Pg. 2: The lidar literature review is very concentrated on the activities of DTU Wind Energy. DTU is certainly one of the leaders in lidar for wind energy, but including more works from other organizations would make a more representative review. As an example, a couple other relevant references are:
-Turbulence: Newman, J. F. and Clifton, A.: An error reduction algorithm to improve lidar turbulence estimates for wind energy, Wind Energy Science, 2017.
-Wakes: Iungo, G.V.; Porté-Agel, F. Volumetric Lidar Scanning of Wind Turbine Wakes under Convective and Neutral Atmospheric Stability Regimes. J. Atmos. Ocean. Technol. 2014.

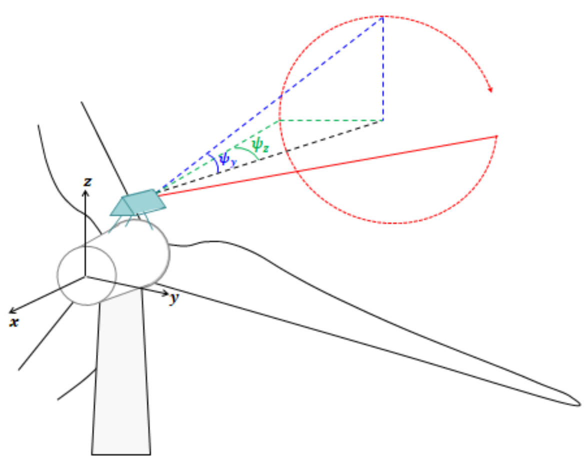
The recommended references are now added.

4) Table 1: Can you explain the difference between u_{hub} (mean wind speed at hub height) and U (in the TI definition)? Or are these the same and could be written with the same symbol?

We now replace the U with u in Table 1 and throughout the manuscript.

5) Section 3.1: A figure showing the coordinate system and variables would be helpful.

The coordinate system and orientation of the axis can be derived from Fig. 2 and the definition of the angle is self-explanatory in the text. As the manuscript is already quite long, we added a figure showing the coordinate system and the variables in the appendix.



6) Eq. 4+5: Please explain the meaning of a 3D LOS vector. The component along the lidar beam direction makes sense, but what are the other two components?

The T_{LOS} is a 3x3 rotational matrix obtained from the product of two rotations about the y- and z-axes, ψ_y and ψ_z (see figure above). The 1st row of T_{LOS} defines the transformation between the LOS (along the beam of the lidar) and the reference-coordinate system (x,y,z) of figure above.

Theoretically, the 2nd and 3rd rows of T_{LOS} would refer to the transformation between two vectors perpendicular to the LOS and the reference system. As the lidar measures only the LOS, then only the 1st row of T_{LOS} is used.

In order to clarify this, we correct the manuscript and specify that only the 1st row of T_{LOS} is used before introducing Eq. 5.

The following text is added on page 7 line 8 before Eq. 5:

As lidars measure only the LOS velocity, the first row alone of T_{LOS} is considered. The relation between the wind vector and the LOS velocity is expressed in terms of matrix transformations as:

$$\boldsymbol{u_{los}} = \boldsymbol{T_{LOS} T_1 u}. \tag{1}$$

7) Eq. 6: Should C_{ind} only be applied to the first term in the brackets, aligned with the rotor orientation (the direction of the rotor thrust)? This is how it is written in Dimitrov et al. 2019. Additionally, a little more information about C_{ind} would be useful for the reader. What are the input parameters to C_{ind}?

Eq. 6 is now corrected.

The following text is added on page 7 line 16:

The two-dimensional induction model assumes longitudinal and radial variation of the induced wind velocity. The resulting induction factor C_{ind} is computed as:

$$C_{ind} = \left[1 - a_0\left(1 - \frac{\xi_x}{\sqrt{1+\xi_x^2}}\right) \cdot \left(\frac{2}{\exp\left(+\beta_a\epsilon_a\right) + \exp\left(-\beta_a\epsilon_a\right)}\right)^2\right], \tag{2}$$

where a_0 is the induction factor at the rotor center area; $\xi_x = x/R_{rotor}$ is the distance from the rotor normalized by the rotor radius; $\rho_a = \sqrt{y^2+z^2}/R_{rotor}$ is the radial distance from the rotor center axis; $\epsilon_a = \rho_a/\sqrt{\lambda_a(\eta_a+\xi_x^2)}$, where $\gamma_a = 1.1$, $\beta_a = \sqrt{2}$, $\alpha_a = 8/9$, $\lambda_a = 0.587$, $\eta_a = 1.32$ (Dimitrov et al., 2019).

8) Pg. 7, ln. 17: "The parameters (u_{hub},: : :" Because of the induction zone model, I imagine the induction factor is also a parameter that is estimated. Is this true?

This is correct, the C_{ind} is now included in the sentence.

9) Pg. 7, ln. 28: "The velocity fluctuations, denoted by u = (u,v,w), are expected homogenous: : :" Shouldn't it be that the "statistics" of the velocity fluctuations are expected homogeneous?

We corrected and added "statistics" in the sentence.

10) Eq. 7: Should R be a function of the separation vector r?

R is defined as the covariance tensor. R(r) is a function of the separation vector when computing a two-point turbulent statistics, where the selected two points are separated by a vector

r. However, for single-point turbulent statistics r=0, the covariance tensor (R(r=0) = R) can be expressed in matrix form that contains variance and covariance terms of the wind field u=(u,v,w).

To clarify, the following text is replaced on page 7 line 28:

From: "The velocity fluctuations, denoted by u = (u,v,w), are expected homogeneous in space (Mann, 1994). The covariance tensor of single-point turbulent statistics can be written as:"

To: " The velocity fluctuations (u', v', w'), where $(')$ denotes fluctuations around the mean value, are expected homogeneous in space (Mann, 1994). It follows that the auto- or cross-covariance function between two points can be defined only in terms of the separation distance as $R_{ij}(\boldsymbol{r}) = \boldsymbol{u}'_i(\boldsymbol{x})\boldsymbol{u}'_j(\boldsymbol{x}+\boldsymbol{r})\rangle$, where i, j = (1,2,3) are the indices corresponding to the components of the wind field, $\langle\rangle$ denotes ensemble averaging, and $\boldsymbol{r} = (r_1, r_2, r_3)$ is the separation vector in the three-dimensional Cartesian coordinate system. The covariance tensor of single-point turbulent statistics $(\boldsymbol{R}(\boldsymbol{r} = 0) = \boldsymbol{R})$ can be written as:

$$\boldsymbol{R} = \begin{bmatrix} \langle u'u' \rangle & \langle u'v' \rangle & \langle u'w' \rangle \\ \langle v'u' \rangle & \langle v'v' \rangle & \langle v'w' \rangle \\ \langle w'u' \rangle & \langle w'v' \rangle & \langle w'w' \rangle \end{bmatrix} = \begin{bmatrix} \sigma_u^2 & \sigma_{uv} & \sigma_{uw} \\ \sigma_{vu} & \sigma_v^2 & \sigma_{vw} \\ \sigma_{wu} & \sigma_{wv} & \sigma_w^2 \end{bmatrix}, \tag{3}$$

11) Eq. 8: R_i, j should be a function of r here, not k.

This is now corrected.

12) Pg. 9, ln. 24: "The relation between the covariance matrix of the LOS is then: : :" It seems something is missing here. The relationship between the covariance matrix and what else?

The following text is replaced on page 9 line 24:

From: "The relation between the covariance matrix of the LOS is then expressed in terms of σ_u^2 as:"

To: "The relation between the covariance matrix of the LOS components and that of the undisturbed wind field is then expressed in terms of σ_u^2 as:"

13) Eq. 13: I believe this equation results in a matrix. How do you go from a matrix to a scalar value used to scale the LOS variance?

R_{LOS} is the full covariance matrix containing three vector components: the LOS and the other two vectors perpendicular to the LOS. However, as lidar measures only the LOS component, only the first component of R_{LOS} is measured. Therefore the ratio in Eq. 13 defines the relation between the LOS variance (as scalar) and the variance of the u-component.

The following text is replaced on page 9 line 26:

From: "Since only LOS velocities are measured by the nacelle-mounted lidar, the ratio in Eq. (13) identifies the relation between the LOS variance and the wind field variance in the longitudinal direction"

To: "Note that R_{LOS} is expressed as a full covariance matrix containing three vector components. However, as only LOS velocities are measured by the nacelle-mounted lidar, only the first component of R_{LOS} is measured. It follows that the ratio in Eq. (14) identifies the relation between the LOS variance and the wind field variance in the longitudinal direction"

14) Pg. 10, lns. 4-6: The full details can be left to Pena et al. 2017, but a little more detail about how the different beam directions are combined to find the u-component variance would be appreciated, since "computing the variance of Eq. (5)" is hard to interpret.

We added the equation below, which shows the relation between the LOS variance and the variance of the u-component. The equation is derived from the extended form of Eq. 5 by applying the variance operator. In its extended form, as shown in Peña et al. 2017, the LOS variance and the lidar orientation angles are known variables, while the variances $\langle u'u'\rangle$,$\langle v'v'\rangle$,$\langle w'w'\rangle$ and covariance $\langle u'w'\rangle$ are unknown. As the measurements of the CW lidar are grouped in 10 bins within each range, we require minimum four of these bins to solve the system and derive the variance of the u-component. This is valid under the assumption of homogenous turbulence.

The following text is now added on Page 10 lines 4-6:

By assuming homogeneous turbulence, we use the scanning pattern to account for cross-contamination of different velocity components and extract 10-min σ_u^2 statistics by computing the variance of Eq. (5) as:

$$\mathrm{Var}(u_{los}) = \mathrm{Var}\left((\cos\psi_y\cos\psi_z\cos\varphi - \cos\psi_y\sin\psi_z\sin\varphi)u - (\cos\psi_y\cos\psi_z\sin\varphi + \cos\psi_y\sin\psi_z\cos\varphi)v + (\sin\varphi)w\right) \tag{4}$$

By solving the variance operator and neglecting the resulting terms $\langle u'v'\rangle$ and $\langle v'w'\rangle$, as explained above, σ_u^2 is derived as shown in Eq. (10) in Peña et al. (2017).

15) Pg. 10, ln. 8: "The filtered turbulence derived from CW and PL lidars are plotted: : :" Which beams are used to derive the turbulence?

To derive the filtered turbulence estimates from both the CW and PL lidars, we used all the beams and ranges. To derive the unfiltered turbulence from the CW lidar, we use only measurements at 1.3 D.

We replaced the text in the manuscript on page 10 line 8:

From: "The filtered turbulence derived from the CW and PL lidars are plotted respectively in Fig. 4 (left and middle), whereas the unfiltered turbulence derived from the CW lidar is shown in Fig. 4 (right)."

To: "The filtered turbulence derived from CW and PL lidars, using all the ranges and beams, are plotted respectively in Fig. 4 (left and middle), whereas the unfiltered turbulence derived from the CW lidar measurements at 1.3 D are shown in Fig. 4 (right)."

16) Pg. 11, ln. 27: "The conservative thresholds ensure a strong wake influence in the inflow conditions: : :" This seems like a good approach for this study, but using the algorithm for a full load validation would probably miss some of the more benign wake conditions. Would this lead to overestimating the wake loads if only the strong wake conditions are simulated?

The conservative thresholds ensure the strong impact of wakes on the 10-min measured/simulated wind field and wind turbine statistics. As we run the uncertainty quantification separately for partial-, and full-wake conditions, this approach allows us to identify and differentiate the conditions within the 10-min periods. Further, as the uncertainty increases for wake situations compared to the free case scenario, we indeed expect these results to be on the conservative side.

17) Pg. 12, ln. 5: ": : :through the PL and CW lidar-estimated wind speed, turbulence and shear exponent in Fig. 6." Is Eq. 6 used to find the wind parameters, or another method? Since Eq. 6 combines multiple ranges together to estimate the wind field parameters, I'm guessing you are using a different approach here.

We corrected and rephrased the text. In the analysis shown in Fig. 6, we investigate how the lidar-based wind characteristics varies as function of the upstream ranges in free, partial- and full-wake situations. For this particular analysis, we disregard induction effects.

The following text is now added on page 12, line 5:

From: "We observe these effects through the PL and CW lidar-estimated wind speed, turbulence and shear exponent in Fig. 6. Here, the slope of a linear regression model between the free wind mast-measured and lidar-estimated wind parameters in free-, partial- and full-wake situations are shown as function of the upstream distance from the rotor."

To: "Here, the slope (m) of a linear regression model between the free wind mast-measured and lidar-estimated wind parameters in free-, partial- and full-wake situations is shown. In this particular analysis, the lidar-based wind parameters are derived from Eq. (6) evaluated a different upstream distances from the rotor without including induction effects."

18) Pg. 12, ln. 9: "while the mast is wake-free": How are you determining if the mast is wake free? For example, from Fig. 1, at 265 degrees, the mast looks like it will be waked.

Some details were described on page 11 line 19. However, we now add an Appendix to describe the application of the wake detection algorithm to the mast measurements.

Appendix A: Wake detection from mast measurements

The wake detection algorithm (see Sect.3.4) is extended to the mast measurements to classify 10-min periods where the mast is in free or wake situations. For this purpose, turbulence observations from the cup anemometer at 80 m and vertical wind shear computed using the measurements from the cup anemometers at 57.5 and 80 m are used as wake detection parameters. Their $99th$ percentiles are used as conservative thresholds to characterize the limits of the normal range of the site-specific free wind conditions. The resulting thresholds are $TI_{mast,99} = 0.20$ and $\alpha_{mast,99} = -0.02$. If one of the two limits is exceeded within a 10-min period, the mast is considered in wake conditions and shown with green markers in Fig. 1.

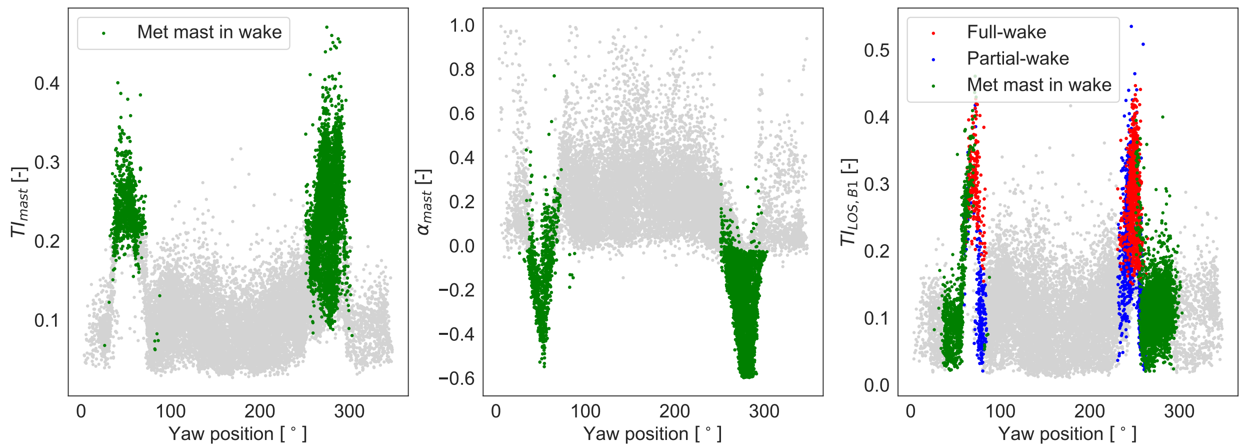


Figure 1: Left and middle: 10-min observations of the turbulence intensity and vertical wind shear at the mast as function of turbine yaw position. Free wind conditions relative to the mast are identified with grey markers, and waked situations with green markers. Right: PL-estimated 10-min wake detection parameter $TI_{LOS,B1}$. Detected wake situations of turbine T04 are shown with coloured markers: wake-free (grey), partial-wake (blue) and full-wake (red). The 10-min periods, where the mast is affected by wakes are shown in green markers.

19) Pg. 13, ln. 3: ": : :shows low bias at farther beams": Please clarify what you mean by low bias in this case. Bias between the two types of lidars?

The following text is replaced on page 13 line 3:

From: "The inter-comparison between PL and CW filtered turbulence in wake situations shows low bias at farther beams, where larger probe volume averaging effects are expected for the CW lidar (Dimitrov et al., 2019)."

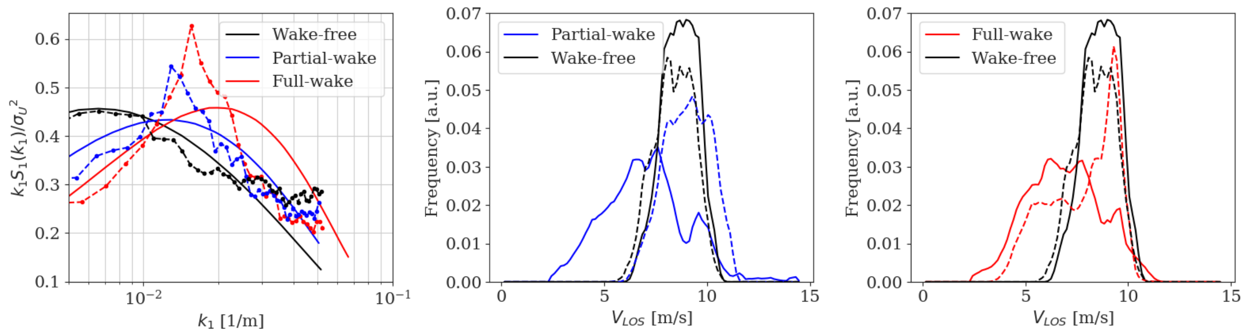
To: "The difference between PL and CW filtered turbulence in wake situations (circle and star markers) decreases at farther beams, where larger probe volume averaging effects are expected for the CW lidar (Dimitrov et al., 2019)."

20) Pg. 14, ln. 29: "Small-scale turbulence is also responsible for increasing the width of the Doppler spectrum." Can you explain how the turbulence scale impacts the Doppler spectrum width? I would expect this to be a function of the standard deviation, but it isn't clear how the length scale directly impacts this.

The small-scale (high frequency) fluctuations will be filtered out by the probe volume of lidar measurements, especially if the characteristic turbulence length scale is lower than the probe volume size. However, the analysis of the Doppler radial velocity spectrum can recover information of the turbulence at the different scales. It is expected that small-scale turbulence from wakes result in broadening of the Doppler spectrum compared to ambient flow conditions. The widening of the Doppler spectrum relates to increased variance (or standard deviation) of the LOS velocity. These effects were reported in Held and Mann (2019).

21) Pg. 14, ln. 32: "It can be noticed that broadening effects are present only in b3: : :" Where is the evidence of broadening effects? Wouldn't this require the velocity spectrum in freestream conditions for comparison?

We added the Doppler spectrum for a wake-free case with similar inflow conditions.



We replaced the following text on page 14 line 29-32:

From: "Small-scale turbulence is also responsible for increasing the width of the Doppler spectrum (Branlard et al., 2013; Held and Mann, 2019). We show an example of a 10-min ensemble-average Doppler spectrum obtained from the radial velocity of the CW lidar using bins b3 and b8 (see Fig. 2 for notation) at 1.3 D in partial-wake and full-wake in Fig. 7 (middle and right). The relative free wind speed measured at the met mast is 9 m/s.

To:" The small-scale turbulence generated within wake flows generally leads to a significantly larger broadening of the Doppler spectrum compared to that in the ambient flow (Branlard et al., 2013; Held and Mann, 2019). We show an example of a 10-min ensemble-average Doppler spectrum obtained from the radial velocity of the CW lidar using bins b3 and b8 (see Fig. 2 for notation) at 1.3 D in partial-wake and full-wake in Fig. 7 (middle and right). We also provide the ensemble-average Doppler spectrum in free wind for reference."

22) Pg. 15, ln. 18: Please compare with the coefficient of determination equation in Dimitrov et al. 2019. It appears there are some typos in the equation listed here.

The coefficient of determination is now corrected

23) Pg. 16, ln. 5: Why is only MXBCmin investigated, as opposed to MXBCmax?

This is due to the convention used in the strain gauges. The increasing flapwise bending moment results in negative loading. We keep the same convention as in (Dimitrov et al., 2019).

24) Pg. 17, ln. 8: "DeltaR / DeltaR,Ref": Should this be "DeltR - DeltaR,Ref"? Or "DeltaR / = 104%"?

We perform comparisons between indicators from different cases and refer to the relative error between the load uncertainty in free wind conditions and wake situations. Therefore, it should be interpreted as DeltaR - DeltaR,Ref. We corrected the text accordingly.

25) Pg. 17, ln. 10: "induction effects are dominant at these ranges: : :" Doesn't the WFR in Eq. 6 already account for induction effects to estimate the freestream (or in this case wake) wind speed?

We corrected and replaced the sentence to:

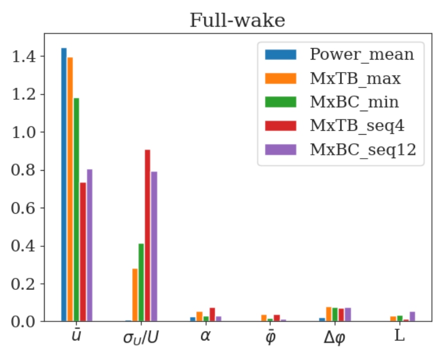
The power predictions deviations in partial-wake drop to approximately 1%, when the PL lidar-estimated wind characteristics using measurements up to 1.3 D are used in the simulations. This result indicates the sensitivity of the reconstructed wind field characteristics to the upstream ranges in a strongly inhomogeneous wind field as a partial-wake situation.

26) Pg. 17, ln. 17: ": : :as low as 3%": Should this be "as high as 3%"? This seems to be the highest % over DeltaR,Ref, not the lowest.

We added the signs in text to differentiate between over and underestimation and also specified in the text if it is under- or over-prediction.

27) Pg. 18, ln. 7: "The obtained linear regression coefficients for Power: : :" This is a nice analysis. Are the trends the same for MXBCmin and MXTBDEL (which aren't shown)?

We show the linear regression coefficients for all the channels in the figure below.



28) Table 3: It would probably be worth repeating the (short) caption from Table 2 here.

This is now done as suggested.

29) Pg. 19, ln. 1: ": : :slightly higher sensitivity of L in full-wake compared to partial wake and free-wind conditions." The partial wake loads still show a strong dependence on L (although less than full wake). Any thoughts on why the regression coefficient for L is almost zero for partial wake?

The results in Fig. 10 should be interpreted as the coefficients of a linear regression model between the inputs and the output sensors. The regression models are fitted to a dataset of 850 10-min periods, which are characterized by different inflow conditions (i.e. wind speed, turbulence, shear, etc.) as measured by the PL lidar and the L parameter, which was varied between the defined boundaries (see figure below for the partial-wake). Note that we show the absolute input values in the figure, although the inputs are normalized such that their values are scaled between zero and one, when fitting the regression model. The resulting coefficients give an estimation of the slope of a linear model between inputs and outputs. If a strong dependency is seen between output and inputs, as for the mean wind speed and turbulence in the figure below, a strong sensitivity of output sensor to the input is indicated. In the case of L, we do not see a linear relationship, but rather a scatter plot. This is because, while we vary L, the wind speed and turbulence levels are also varied, and the impact of the latter two is higher to that of L. However, when all inputs are fixed and only L varies, as the results in Table 2 and Table 3, we can estimate the effective influence of L on the predictions.

There might be several reasons why the regression coefficient for L in partial-wake is lower than that for full-wake. One is explained above. Another reason is that the absolute values of the coefficients depends on the dataset used. In fact, we use different data for the partial- and full-wake. A third one is that the L parameter is varied between 15 and 30 m for partial-wake, but between 7-30 m in full-wake.

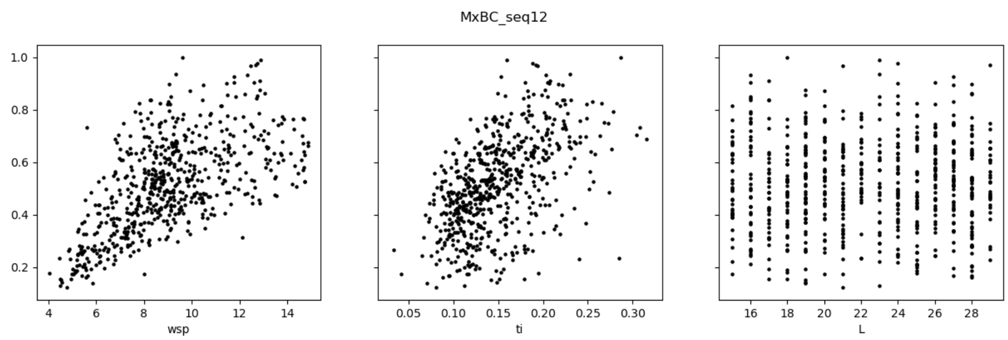


Figure 2: Scatter plot of the normalized fatigue loads at the blade-root as function of inflow wind conditions (mean wsp, turbulence and turbuelence length scale L)

30) Pg. 23, ln. 7: "Further investigation is necessary to verify that the observed uncertainty of predictions are comparable with results using state-of-the-art wake models: : :" How would this be investigated? Would you need freestream measurements of the inflow conditions to use as inputs to the wake model? If so, how would the freestream conditions be measured, given that the mast appears to be waked for much of the sector where the turbine is waked?

The intention is to state that the observed uncertainty based on the proposed method is comparable to that from IEC wake models/approaches. The dataset at the NKE site could be potentially used to quantify the uncertainty of load predictions based on the, e.g., DWM model. As the reviewer mentioned, the validation should be limited to directions and the statistics where the mast is wake-free. Other dataset could also be used for this analysis. For example, the SWiFT experiment at Sandia provides high spatial and temporal resolution lidar measurements of the wake flow field and concurrent measurements of the ambient flow from a meteorological mast. We are currently analysing SWiFT experiment data.