Author response to reviewer 1

The authors response is shown in red

We thank the reviewer for the valuable comments and suggestions, which we consider very important and help us to sharpen and improve the manuscript. Here our response to each comment.

The paper has been improved greatly. It now contains stronger motivation for the work, clearer explanations of the methods, and an improved summary of the results. Given that this is a long paper and there were many changes made to it, I do have many mostly minor comments that I believe should be addressed for the final version.

Comments on Author Responses:

18. Pg. 8, ln. 193-194: There is also a 4-beam Windar CW lidar, and the grid configuration pattern is based on the SWE pulsed lidar. Can you explain why you classified these scan patterns as pulsed and CW, respectively? Furthermore, since you are only modeling a single measurement range, it is unclear how you model CW and pulsed lidars any differently in you simulations. Can you explain this further? Lastly, you are giving up additional measurement points (and therefore potentially wind field reconstruction accuracy) by only using a single range for the pulsed lidars. Why didn't you use multiple range gates?

AR: We have removed the paragraph describing the currently available nacelle lidars. The previous classification between CW and PL lidar was only made to reference the existing type of nacelle lidars. Still, it did not influence the simulation results, as we mainly simulate the probe volume effects by a pre-defined weighting function. The reason for using a single range is conditional on the fact that we use DWM model-based fields as target fields. Indeed, the DWM model predicts quasi-steady wake deficits, which are computed according to a specified downstream distance. These deficits are meandered transversely, advected in stream-wise direction with the mean wind speed using Taylor's assumption, and superimposed on random turbulence field realizations (we have now described that in detail in Sect. 3.2). As the DWM model does not simulate turbulence evolution, we cannot simulate multiple range gates. This analysis would be suitable using an LES-based wake field. Another aspect to consider when using multiple ranges is that the wake recovers and expands with farther downstream distances; therefore, the wake field characteristics observed further upstream of the rotor may be considerably different from those approaching the turbine rotor.

"As the DWM model does not simulate turbulence evolution, we cannot simulate multiple range gates": The simplification to one range is fine for the paper. But it would still be possible to simulate multiple ranges without wind evolution. This is done frequently when assessing lidar-assisted control. It just adds an additional assumption about the wind field.

As the reviewer said, it is correct that multiple ranges could be simulated with the DWM model while neglecting wind evolution. However, we did not analyze multiple ranges as part of this work.

Also, since many readers will be familiar with pulsed and CW lidars, please discuss the assumptions used in the paper (e.g., that you are modeling a pulsed lidar at one range, a CW lidar, or that your model is not specific to one type of lidar).

Since we removed the whole paragraph, there is no specification on the type of lidars. In order to clarify that our model is not specific to one type of lidar, we now added two lines in section 3.3.1. Ln. 261, as: "The lidar simulator is assumed to scan the selected patterns at the same single range upwind of the rotor. Pulsed and continuous-wave (CW) lidar technologies apply different approaches at scanning multiple ranges. Pulsed lidars can scan multiple ranges along the LOS simultaneously within a single sample, while CW lidars typically sample much faster at a given range but need to refocus in order to change the sampling range. In the present paper, we only consider a single focusing range that is achievable with both lidar technologies. Further, a time lag between each sampling beam is simulated to mimic lidars' sampling frequency. "

20. Pg. 8, ln. 204: "A probe volume with an extension of 30 m in the LOS direction is assumed" Can you provide some references for how you chose 30 m for pulsed and CW lidars? Furthermore, how is the probe volume extension defined? For example, the std. dev. of Gaussian weighting function?

AR: We have added that the probe volume length is here defined as the standard deviation of the Gaussian weighting function, and added references. The probe volume length of 30 m does not identify a specific lidar system, but it is an estimate that is comparable with the current CW lidar technology measuring at distances beyond 120 m [2]. Further, we conduct a sensitivity analysis by varying the probe volume lengths in Sect. 4.3.2, to analyze how these lengths influence the accuracy in power and load predictions.

"but it is an estimate that is comparable with the current CW lidar technology measuring at distances beyond 120 m [2]": This is a reasonable simplification, but I would provide an explanation like this in the paper. Further, a 30 m probe length is commonly used to model pulsed lidars, so that might be a better justification to use. For example, 30 m is used as the full-width-at-half-maximum probe volume in:

Schlipf, D. Lidar-Assisted Control Concepts for Wind Turbines. Ph.D. Thesis, University of Stuttgart, Stuttgart, Germany, 2016.

We have rephrased it as: "A probe volume with an extension of 30 m in the LOS direction is assumed for all the analyzed patterns, which is comparable with the current continuous-wave lidar technology measuring at distances beyond 120 m (Peña et al., 2015). Further, a 30 m probe length is commonly used to model pulsed lidars (Schlipf 2016) [1].".

23. Pg. 10, ln. 226: What do you mean by 'The u-velocity fluctuations are recovered from the target wake fields?'

AR: We have rephrased to: 'Only the u-velocity fluctuations are reconstructed from the target wake fields.'

Consider "... are reconstructed from the lidar measurements of the target wake fields."

This has been corrected.

25. Eq. 10: I'm confused about how $K_{def,lidar}$ is defined. From Fig. 1, K_{def} is presented as a scaling factor applied to the ambient wind field (= 1, when wake losses are not present). But here, it appears to be defined as the normalized deficit (= 0, when wake losses are not present). Can you clarify this and make sure the definitions of K_{def} are consistent?

AR: That's correct, we now define K_{def} as the normalized deficit (= 0, when wake losses are not present) and keep this definition consistently.

This is clear now in Sect. 3.4.2. However, the definitions of U_{def} and K_{def} in Eqs. 13+14 do not appear to be consistent with the definitions in Fig. 2 and Eq. 6. I.e., in Eqs. 13+14, U_{def} and

K_{def} are defined as 0 outside of the wake deficit region. But in Fig. 2 and Eq. 6 it appears they both equal 1 outside of the wake region.

That's correct. For consistency, we have corrected Eq. 6 to: $u'_{i,K_{def}}(x,y,z) = \bar{U}_{amb}(z)(1 - K_{def}(x,y,z)) + u'_i(x,y,z) - \bar{U}_{amb}(z)$. Further, we re-plotted Fig. 2, so U_{def}= 0, when wake losses are not present.

30. Fig. 7: On the left plot showing U_{eff}/U_{amb}, can you explain why the ratio converges to 0.93 at high wind speeds? As wind speed increases, the turbine thrust should keep decreasing causing wake losses to continue to decrease, so I would expect the ratio to approach 1.

AR: It does not converge to 1 because although the trust coefficient decreases for higher wind speeds, the ambient turbulence is relatively low, and therefore the wake field does not fully recover at a distance of 5D, which is the one analyzed in this study. The ratio U_{eff}/U_{amb} will converge to 1 for higher ambient turbulence or farther downstream distances due to the increased turbulence mixing. We have now described that in the paper.

The lower turbulence at higher wind speeds does explain part of why the wake would not recover as much as expected by 22 m/s. However, since the ratio plateaus at 0.93 for several wind speed bins, it seems like something else is happening. Is U_{amb} treated as the mean freestream wind speed at hub height? If that is the case, then maybe even in freestream conditions, U_{eff} will be $0.93 * U_{amb}$ because of wind shear.

We checked the simulations' results, and the contribution of the wind shear to the ratio U_{eff}/U_{amb} should account for up to 3% in free-stream conditions ($U_{eff}/U_{amb} \approx 0.97$). Another aspect influencing the U_{eff}/U_{amb}'s trend at high wind speeds (18–22 m/s) is that the thrust coefficient does not vary with the same rate compared to below rated wind speeds but it is nearly constant.

We have rephrased the text as: "However, the wake deficit does not fully recover at high wind speeds ($U_{eff}/U_{amb} \approx 0.93$), as we simulate relatively low ambient turbulence levels, the spacing between the turbines is short (i.e., 5 D), and the thrust coefficient of the turbine is nearly constant at high wind speeds. Further, the contribution of the wind shear to the ratio U_{eff}/U_{amb} accounts for up to 3% in free-stream conditions, i.e., $U_{eff}/U_{amb} \approx 0.97$."

34. Pg. 21, ln. 469: "It should be noted that the structural resonance occurring at low wind speeds, which excites the tower can potentially affect the correlation results." Can you discuss why this resonance appears? Could it be removed by improving the controller tuning?

AR: It appears because of the structural design of the DTU 10 MW, which is a reference (theoretical) turbine model. At low wind speeds (thus low RPM), the 3P rotational frequency (0.30.48 Hz) excites the eigenfrequency of the tower (0.25 Hz). Considering that the wake induces unbalanced load distribution on the rotor, which in turn amplifies the rotor harmonics (1P, 2P, and 3P), this results in structural resonance. Besides that, we also observe that the bending moment of the tower bottom for large turbines is highly driven by the 3P frequency, as also shown in Fig. 13 (where the imprint of the turbulence wind is almost non-existence). Some internal work at DTU has been conducted to reduce the resonance, and the controller utilized in this work should be optimized to reduce resonance effects, which are still present and amplified under wake conditions. Future studies that evaluate these lidar-based reconstruction approaches can be conducted with different wind turbine designs that do not experience these resonances.

A sentence about the cause of the controller resonance would be insightful in the paper.

We have rephrased the sentence in page 21 as: "The X_R values of MyTB and MzSh are significantly higher than other load sensors. The cause of the former is structural resonance occurring at low wind speeds for which the 3P frequency (≈ 0.3 Hz) excites the tower's natural frequency (≈ 0.25 Hz) (Back 2013). This effect originates from a design aspect of the DTU 10 MW turbine, and is amplified under wake conditions due to the induced unbalanced aerodynamic load distribution at the rotor. Nevertheless, structural resonance is independent of the wake-field reconstructing approach."

Additional Comments:

1. Ln. 55: "Further, to accurately reconstruct wake meandering time series, it is essential to ensure accurate power and load predictions in a load validation analysis"? This seems to make more sense the other way around: "to accurately predict power and loads in a load validation analysis, it is essential to accurately reconstruct wake meandering time series." Is this correct?

This is correct. We replaced the sentence.

2. Lns. 74-77: Would "monitoring wind turbine performance" make more sense as "condition monitoring of wind turbines"? Additionally, brief examples of how lidar-based power and load validation under wakes would improve the listed application areas would be appreciated.

We have rephrased the paragraph in Lns. 74-77 as: "Overall, developing lidar-based wake wind field reconstruction techniques that reduce the modeling and statistical uncertainties in the inflow inherent of low-order engineering wake models can improve loads and lifetime estimations accuracy (Rommel et al.,2020), enhance power curve testing in wind farms (Lydia et al., 2014; Wagner et al., 2015), and promote lidar-assisted wind turbine and wind farm control strategies (Bossanyi et al., 2014; Raach et al., 2017; Simley et al., 2018; Schlipf et al., 2020).". We added two references regarding lidar-based control strategies that use wake-tracking (Raach et al., 2017 [2]) and turbulence estimation (Schlipf et al., 2020 [3]) as input to the controller.

3. Fig. 2: In the middle plot, k_{mt} appears to be 1 outside of the wake deficit region. But if this represents wake-added turbulence, should k_{mt} be zero outside of the wake region?

This is correct. We have replaced the figure accordingly.

4. Ln. 197: "... scales the residual field of a Mann-generated turbulence field". What is the TI or std. dev. of the turbulence field. i.e., if k_{mt} is used to scale the turbulence, then what is the baseline turbulence level that it is scaling?

This information was missing. We added that the std. of the turbulence field is 1 m/s, as described in the IEC standard [4].

5. Lns. 221-222: "from fitting the free-stream observed turbulence velocity spectra with the Mann model with the use of pre-computed look-up-tables". It isn't clear how look-up-tables would be used for this.

The look-up-tables (LUT) are used to efficiently compute the Mann model spectra (F_{uu}, F_{vv},

F_{ww}, F_{uw}) given the Mann parameters ($L, \Gamma, \alpha\epsilon^{2/3}$). Indeed, a bivariate spline approximation is carried out to determine the spectra from the LUT instead of analytically computing the spectra. The LUT approach is useful when extracting Mann parameters through an optimization procedure.

We added an explanation in the manuscript, which specifies that LUT is used to speed-up the fitting procedure.

6. Lns. 226-228: Similarly, what is the std. dev. of the $u_j^{'}$ time series?

We added that $\sigma_u = 1$ m/s [4] .

7. Eq. 9: How are the elevation and azimuth angle defined? If azimuth is defined as the azimuth angle in the rotor plane (similar to azimuth angle of a blade), it is hard to see why the cos(theta) appears in the estimate of u_{lidar}.

We have rephrased the sentence as: "where ϕ is the elevation and θ the azimuth angle of the scanning pattern, which refer to the rotations about the y and z axes, respectively." The y and z axis are defined in Sect. 3.1 and also shown in Fig. 2.

8. Lns. 275-279: Since Gaussian weighting functions are typically used to model pulsed lidars and Lorentzian functions are used to model CW lidars, I would mention this point in the paper.

We have now mentioned it in the paragraph.

9. Section 3.4: Is it correct that the high-frequency wake added turbulence is not explicitly included in the 2 wake field reconstruction methods? I wasn't sure while reading the section, so it might be good to highlight this point.

Theoretically, the *CS* algorithm can reconstruct the high-frequency wake added turbulence. However, this would require sampling the wind field at a high temporal frequency (e.g., $f_{sampling} >$ 6 Hz as seen in Fig. 7) without probe volume effects. In contrast, the *WDS*-method cannot explicitly reconstruct the high-frequency wake-added turbulence. We added two sentences in the discussion of the manuscript to highlight this aspect, as in Sect. 3.4, we haven't yet introduced the methods.

10. Ln. 290: "set A and set B": It might be good to remind the reader that set A is used for the target fields.

We now added that in parenthesis.

11. Ln. 309: The constraint set is hard to understand. For example, what is the dimension of H? Should "r" be "r_i" in the definition of H, if the constraint is for a specific location? Finally, if each constraint is a measured time series, then should c_i be written as $c_i(t)$? And is M the number of points in the scan pattern?

We corrected the notation to $\boldsymbol{H} = \{h_i(\boldsymbol{r})|_{r_i} = c_i, i, ..., M\}$, following the notation of Dimitrov et al. 2017 [5]. Each constraint is a measured value of the wind speed for a particular spatial location $\boldsymbol{r}$, but not at time-series; we corrected the text accordingly. M is the number of scanned points within a 10-min period, and we now specify that in the text.

12. Ln. 377: "The normalized RMSE indicates if the lidar-reconstructed fields are unbiased compared..." How would RMSE indicate the bias? The mean error would indicate bias, whereas RMSE could be caused by variability in the error.

That's correct. We have rephrased it as: "The normalized RMSE provides a measure of the quality of the lidar-reconstructed fields with respect to the *target* fields; values closer to zero indicate a high precision and accuracy (see Fig. 5-top row)."

13. Ln. 415: "These effects are not fully recovered in the reconstructed fields, mainly due to the lidar probe volume..." Also because the method fits the lidar measurements to a standard Mann turbulence field, without the small-scale wake-added turbulence being explicitly included.

This has been added.

14. Ln. 540: "underpredicted by Δ_R 2-3%". Based on Fig. 12 the bias can be up to 6%.

This has been corrected.

15. Ln. 554: Is it accurate to call the power time series the "$Power_mean$" time-series? Mean would suggest the mean over the 10-minute period, but you are looking at the full time series, correct?

This is correct, we have replaced "Power-mean" with "Power".

16. Lns. 606-608: "This indicates that when L is low,..." In addition, the turbulence structure sizes become small relative to the lidar probe volume, causing the lidar measurements to average out more of the turbulence.

This is correct and it has been added.

Minor Comments:

1. Ln. 73: "power and load" $->$ "power and loads"

This has been corrected.

2. Ln. 451: "2 m/s" $->$ "2 m/s bin width"? Also, consider adding "respectively" at the end of the sentence.

This has been corrected, and 'respectively' is added at the end of the sentence.

3. Ln. 491: "40-60% estimates" $->$ "40-60% accuracy"?

This has been corrected.

4. Ln. 537: "fictitious biases" $->$ "fictitious lack of biases"?

This has been corrected.

References

[1] David Schlipf. *Lidar-assisted control concepts for wind turbines*. PhD thesis, 2016.

[2] Steffen Raach, David Schlipf, and Po Wen Cheng. Lidar-based wake tracking for closed-loop wind farm control. *Wind Energy Science*, 2(1):257–267, 2017.

[3] David Schlipf, Feng Guo, and Steffen Raach. Lidar-based estimation of turbulence intensity for controller scheduling. *Journal of Physics: Conference Series*, 1618(3):032053, 2020.

[4] International Standard IEC61400-1: wind turbines—part 1: design guidelines, Fourth; 2019. Standard, IEC, 2019.

[5] Nikolay Krasimirov Dimitrov and Anand Natarajan. Application of simulated lidar scanning patterns to constrained gaussian turbulence fields for load validation. *Wind Energy*, 20(1):79–95, 2017.

Author response to reviewer 2

The authors response is shown in red

We thank the reviewer for the valuable comments and suggestions, which we consider very important and help us to sharpen and improve the manuscript. Here our response to each comment.

The manuscript seems improved from the first version. Here two additional comments:

- Fig. 2 does not add any crucial information for the manuscript and could be removed, together with the respective text (L220 on the marked-up version).

We agree that the figure does not add any crucial information for the manuscript; however, we decided to keep it as it facilitates the understanding of the DWM model for those readers who are not familiar with the model.

- Cross-check the second term in Eq. 4

This has been corrected.

References