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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Optimal tuning of engineering wake models through LiDAR measurements" *by* Lu Zhan et al.

Lu Zhan et al.

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The authors are greatly thankful to the Reviewer for insightful comments. Our replies are reported in the following. References to pages and lines are based on the revised marked-up manuscript.

General comment

In this paper, the authors use the LiDAR measurements collected for individual wind turbine wakes to determine the optimal values of the tuning parameters in four different wake models. The manuscript is well written, and the problem is well defined. Determining the tuning parameters in the engineering wake model using the field data is of great interest and use for the wind-energy community. The paper can be accepted.

However, there are a few issues that should be addressed before that
R: We thank the Reviewer for the positive feedback on our research strategy and the results achieved. We have done our best to address the comments arisen from the Reviewer.

Specific comments

Point 1 In figure 1, the layout of the wind farm is shown. However, it is not well explained that which turbines are considered in the analysis.

R: We agree with the Reviewer that this detail is missing in the manuscript. At line 101, it is now added: “According to the wind farm layout and the prevalence of southerly wind directions (Fig. 1(b)), for wind directions within the sector 145° and 235°, the wakes produced by the turbines from 1 to 6 evolve roughly towards the LiDAR location, which is a favorable condition for the LiDAR to measure with close approximation the streamwise velocity through single-wake PPI scans. Furthermore, according to the layout of Fig. 1(a), for the considered wind directions, these wind turbines are not affected by upstream wakes”.

Point 2 It is not clear from the manuscript that the extent of the wake measurements behind the turbine. In particular, the range of downstream distance (x/D) that is used in the optimization of wake models should be provided

R: We thank the Reviewer for this comment, which is now addressed at line 121: “... the objective function of the optimization problem is the mean percentage error (PE) calculated over the measurement domain with x-coordinates between 1.25 D and 7 D, while r between $\pm$1.5 D. PE is defined via...”.

Point 3 In the manuscript is mentioned that “About 10,000 plan-position indicator (PPI) LiDAR scans of isolated wind turbine wakes have been processed to provide the non-dimensional average velocity fields used for this study”. How are the wind-turbine wakes isolated?

R: This point should be now clearer according to the revision provided for Point 1. Indeed, for the considered wind directions the turbines 1 to 6 are not affected by upstream wakes.

Point 4 In equation (9), the authors provided a relation for expansion rate (k) as a function of turbulent intensity (TI). Based on the results in Fig. 2, it seems that the data points are not enough to support that relationship. This point should be addressed in the paper.

R: It is noteworthy that the points of Figure 2 are not single measurements but the result of the averaging process within each cluster based on incoming wind speed and turbulence intensity. Indeed, the actual number of used PPI measurements is about 10,000. To clarify this point, at line 173, it is now reported: "The parameter kopt is proportional to TI, even though the R-square value of 0.85 may seem quite low due to the limited number of points used for the linear fitting. However, it is noteworthy that the data reported in Fig. 2 are obtained from the mean velocity fields of each cluster of the LiDAR measurements including about 10,000 PPI scans".

Point 5 Following the previous comment, there is no error bar in the measurement data. Error bars should be added to all figures.

R: The data reported in Fig. 2 are the results of the optimization process described in Sect. 2. Therefore, they are not statistical values that can be reported with an error bar.

Point 6 The authors assumed that the wake growth rate solely is a function of TI. However, recent studies show the dependency of k to both TI and Ct. This point should be addressed in the paper.

R: We completely agree with this statement. At line 179, it is reported "This different variability of k_{opt} with TI indicates that this model parameter reflects not only effects of the incoming turbulence intensity on the wake evolution, but of the wake-generated turbulence as well". At line 216, it is reported for the Bastankhah model "a secondary trend with the rotor thrust coefficient is observed, which is an effect of the wake-generated turbulence. Indeed, the operative conditions with U^*_{hub}> 0.9 are characterized by a slightly smaller k_{opt} ".

Point 7 In figure 4b, it is assumed that ϵ_{opt} is only a function of TI. However, in the original model, ϵ_{opt} is a function of Ct. The authors should elaborate on the dependency of this model coefficient to both TI and Ct. This is important since the authors report a wide range for Ct from 0.5 to 1.3 in the Gaussian model.

R: We thank the Reviewer for raising this important comment that deserves further discussions. Indeed, the parameter Ïţ of the Bastankhah model is, by definition, only a function of Ct ($\epsilon = 0.2\sqrt{\beta}$,where $\beta = 1/2(1+\sqrt{(1+C_t)}/\sqrt{(1-C_t)}$, as it is now reported at line 207. However, already in Bastankhah et al 2014, it was noticed that varying Ct between 0.42 to 0.8, ϵ only changes from 0.219 up to 0.257. Our study on optimization of the model parameters based on wake LiDAR measurements confirms that Ïţ has small variations with Ct, while the main variability is connected with TI, which might be an effect of the modulation on wake recovery induced by the atmospheric stability. In the manuscript at line 225, it is now reported: "The offset of the standard deviation of the velocity profile, Ïţ, which is by definition only a function of Ct, slightly decreases with increasing U^*_{hub}, suggesting that lower Ct values associated with high U^*_{hub}, i.e. for operations in region 3 of the power curve, lead to a narrower and shallower velocity deficit in the near wake. However, the results of the model optimization show that the main variability of Ïţ is connected with the incoming turbulence intensity and, specifically, ϵ decreases with increasing TI. This effect on Ïţ might be due to the modulation induced on wake recovery by the atmospheric stability".

Point 8 Following the previous comment, the authors showed the optimal value for Ct in the Gaussian wake model in Fig. 4c, which is larger than 1. This contradicts the 1D momentum theory. The authors should better elaborate on this point in the manuscript.
R: We thank the Reviewer for this important comment. We have emphasized that the

Ct obtained through the 1D momentum theory significantly underestimates the respective value obtained by applying the streamwise momentum budget to the LiDAR data or the respective value obtained through the wake-model optimization. These results confirmed results from a previous work (Iungo et al. 2018b), where we leveraged LiDAR measurements and a RANS solver to prove that the thrust coefficient of a wind turbine, so that associated with a wind turbine wake, is generally larger to that predictable through the 1D momentum theory and, in turn, to that associated with the power generation. Indeed, the wake of a wind turbine is the result of the thrust force due to the transformation of the wind kinetic energy in the mechanical rotation energy of the wind turbine, but also includes the drag connected with the bluff body behavior of the nacelle, the tower and blade stall. At line 235, it is reported: "In Fig. 3, the optimized thrust coefficient, Ctopt, is generally higher than the respective values predictable with the 1D stream-tube assumption (Eq. 5), because including contributions to drag due to the bluff-body behavior of the turbine tower, nacelle and blade stall".

Point 9 Fig. 5. Add the error bars to the measurement data.

R: Based on the ensemble averaging and clustering analysis of the LiDAR data (Zhan et al., 2019), the standard error on the weighted mean is always lower than 0.8%, as now reported at line 111. If we would add the error bars, the plot will result to be more confusion without adding significant information, as shown in the figure attached.

Point 10 In all figures with the discrete data (e.g., fig. 7), use dashed lines instead of full lines.

R: Figs. 2, 4, 7, 8 have been revised accordingly.

Point 11 Line 218: the issue regarding the divergence of Eq. 12 has been solved in the following papers that can be mentioned in the manuscript. Abkar, M., Sørensen, J.N. and Porté-Agel, F., 2018. An analytical model for the effect of vertical wind veer on wind turbine wakes. Energies, 11(7), p.1838. Shapiro, C.R., Starke, G.M., Meneveau, C. and Gayme, D.F., 2019. A wake modeling paradigm for wind farm design and control.

Energies, 12(15), p.2956.

R: These papers are now cited in the manuscript.

Point 12 It would good to provide the physics behind the optimization of the model coefficients in different wake models, and how different parameters contribute to the shape of the wake. For example, in the Gaussian wake model (Bastankhah and Porte-Agel) changing k^* tries to match the width of the wakes while Ct tries to match the maximum velocity deficit. Hence, the best combinations of k^* and Ct give the best fit to the velocity deficit profiles. You can provide a similar analysis for the other wake models.

R: We have added comments on the physical contribution of the model parameters to the characteristics of the wakes throughout the manuscript. For instance, for the Jensen model, at line 159 it is reported "The wake expansion coefficient, k, is defined in analogy with the jet spreading within shear flows (Pope, 2000)". For the Bastankhah model at line 222 "(Eq.11), ϵ, which is by definition only a function of Ct, slightly decreases with increasing U^*_{hub}, suggesting that lower Ct values associated with high U^*_{hub}, i.e. for operations in region 3 of the power curve, lead to a narrower and shallower velocity deficit in the near wake". For the Larsen model at line 287 "As mentioned in section 3.3, x_0 is defined as the distance between the rotor position and the origin of the used coordinate system. Nonetheless, it can also be denoted physically as the position where the initial wake width equals to one rotor diameter. Therefore, a faster wake recovery rate due to higher incoming turbulence makes this condition occurring closer to the turbine rotor" or at line 291 "Regarding the wake recovery rate reported in Fig. 7(c), we can see the enhancement of turbulent mixing as a function of increasing turbulence intensity, which can be modeled through a linear function with a slope of 0.68 and interception of 0.01". At line 321 for the Ainslie model "the wake-generated turbulence is taken into account through the parameters k_l and K_M".

Please also note the supplement to this comment:
https://wes.copernicus.org/preprints/wes-2020-72/wes-2020-72-AC1-supplement.pdf

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Discussion paper

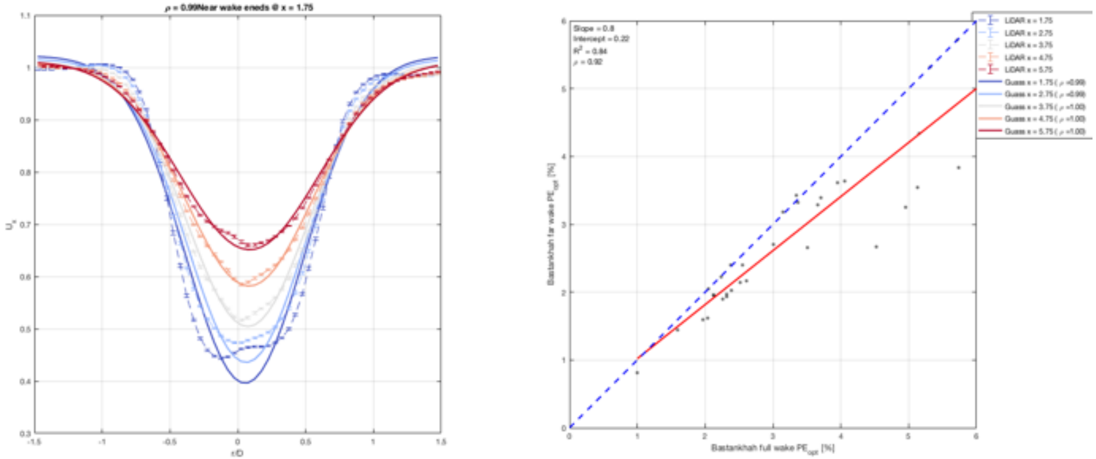

Fig. 1. Figure Point 9