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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Aero-elastic load validation in wake conditions using nacelle-mounted lidar measurements" *by* Davide Conti et al.

Anonymous Referee #1

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This is an interesting and thorough paper about the use of nacelle lidar-derived wind conditions for load validation in waked conditions. The paper builds on the work of Dimitrov et al. 2019 "Wind turbine load validation using lidarâĂŘbased wind retrievals" to extend the load validation procedure from freestream conditions to waked operation. This is a relevant area of work because of the uncertainty that exists in current methods for performing load validation in waked operation, relying on wake models, as opposed to site-specific measurements. Opportunities to improve the validation of loads in waked conditions are therefore of great interest. Overall, the paper is well-written, but I have a couple general comments and many mostly minor comments that I feel should be addressed.

-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).

-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.

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"?

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?

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.

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?

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

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?

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?

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?

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?

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

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

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?

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?

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.

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

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?

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.

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.

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?

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.

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?

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.

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

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

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?

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

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

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

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?

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?

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