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Interactive comment

Interactive comment on "Wind turbine load validation in wakes using field reconstruction techniques and nacelle lidar wind retrievals" *by* Davide Conti et al.

Anonymous Referee #1

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This paper presents methods for performing load validation by reconstructing the wind field upstream of a waked wind turbine using nacelle lidar measurements. The load validation methods are simulated using the dynamic wake meandering (DWM) model and aeroelastic simulations, and compared to the performance of the standard IEC-recommended DWM method for load validation. The paper is a nice extension of previous work by the authors "Aeroelastic load validation in wake conditions using nacelle-mounted lidar measurements," where the authors use wind parameters based on lidar measurements in wake conditions to evaluate the accuracy of load validation as part of a field experiment.

The paper is well written and clearly organized. Furthermore, the topic is relevant given the interest in using nacelle lidars for applications such as power performance and load validation in the wind industry. By investigating the proposed load validation methods in a controlled simulation environment, the authors are able to isolate the impact of the wind field reconstruction methodology, without worrying about aeroelastic model uncertainties. Although there are no major issues with the paper, there are several smaller comments that I believe should be addressed by the authors.

First, more motivation for the proposed lidar-based load validation methods should be presented. For example, if the goal is to achieve load prediction biases that are the same as the baseline method but with lower statistical uncertainty, how will this improve the wind turbine design process? And can you discuss current problems with the IEC-recommended approaches for load validation in wake conditions?

Comments:

1. Title: Instead of "using field reconstruction techniques," which is somewhat vague, consider "using wind field reconstruction techniques"

2. Pg. 2, ln. 34: "the 10-min statistical properties (mean and variance) of the simulated ambient and operational conditions are set to match the measured ambient wind statistics": This doesn't quite make sense. How can the simulated "operational" conditions be set to match measured "ambient" conditions. Wouldn't you only need to match the ambient conditions?

3. Pg. 2, ln 45: "...which increases the amount of validation data." This could use a little more explanation (contrast this to a fixed met tower where only a small sector is valid).

4. Pg. 2, ln. 49: "The recent work of Conti et al. (2020) demonstrated that lidar-based load validation procedure in wakes should account for a model of the wake deficit and its dynamics." Since this paper builds on the work of Conti et al. 2020, please discuss

this work in a little more detail, especially why it was concluded that lidar-based load validation in wakes should include a wake model.

5. Pg. 3, ln. 83: "through a field reconstruction technique": "wind field?" Or "wake field?"

6. Pg. 4, ln. 98: "large number of simulations" How many?

7. Pg. 4, ln. 101: "The mean bias of load predictions... is of the same order of that obtained with the baseline". Please be more specific about how close the lidar-based simulations should be to the baseline. "Of the same order" is a subjective criteria and makes it hard to tell if the new methods are successful.

8. Pg. 4, ln. 111: "the IEC recommends, i.e., the Mann uniform shear spectral tensor model..." This is one model that is recommended. There is also the Kaimal spectral model, etc.

9. Eq. 1: the symbol "i" is used twice, for the spatial location as well as to indicate imaginary numbers. Can you choose unique symbols?

10. Eq. 2: Should the bold "k" argument on the left hand side be "k_1"?

11. Section 3.2: Can you explain more about the tools you are using to implement DWM? In other words, is DWM a software tool that you are using (if so, a reference would be appreciated)? Or is it a model described in the literature that you are implementing yourselves?

12. Fig. 1: What wind speed is used for the middle plot?

13. Pg. 6, ln. 157: "The latter increases the uncertainty of the procedure." It is unclear what "the latter" refers to here.

14. Pg. 6, ln. 166: "while the spatial resolution in the longitudinal axis depends on the simulated wind speed." Then what is the temporal resolution of the wind field?

15. Pg. 7, ln. 171: "continuous-wake" -> "continuous-wave"

16. Pg. 7, ln. 181: Can you provide a reference for the 4-beam Leosphere lidar?

17. Pg. 8, ln. 185: "7-beam lidar can potentially increase the accuracy of reconstructed wind fields." Increase the accuracy compared to what?

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?

19. Pg. 8, ln. 199: "A preview distance of 0.7 D is assumed." In addition to the lidar measurement accuracy arguments, there seems to be an interesting dilemma when measuring the wake deficits upstream of a turbine. On one hand, I imagine you would want to measure close to the turbine to capture the true wake velocity deficit at the rotor plane. On the other hand, measuring too close will introduce induction zone effects. Can you discuss how you approached this issue?

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?

21. Pg. 9, ln. 218: "obtained by simply scaling an isotropic turbulence field. . ." Can you clarify if the scaling depends on the radial location from the wake center, as shown in Fig. 1?

22. Pg. 9, ln. 221: How might the ambient wind conditions be measured in practice?

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

24. Pg. 11, ln. 256: "By denoting. . . as the constrained turbulence field that incorporates lidar measurements. . ." It seems that in Eq. 9, uˆprime_CS,B,i, represents the turbulent fluctuations with the mean ambient wind profile removed. Do you first remove the mean ambient wind speeds from the lidar measurements before they are used to generate the constrained turbulence field?

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?

26. Eq. 10: Since the left hand side of this equation is being fit to the Gaussian function, they are not actually "equal." It would make more sense to present this equation as a minimization objective function (e.g., based on the difference between the measured deficit and the Gaussian model) Also, should U_amb(z) have the mean operator applied to it, like in Eq. 9?

27. Eqs. 10 and 11: The explanation of Eq. 11 is confusing. In your final method are you using the Gaussian fit from Eq. 10 as part of Eq. 11, or does Eq. 11 entirely replace Eq. 10? It would help to present both equations as minimization problems, so it's easy to see where the lidar measurements are being used and what exactly is being fit to the Gaussian profile.

28. Section 4.1: In addition to the analyses presented, a nice way to quantify the accuracy of the reconstructed wind fields could be to compare the RMSE of the rotor average wind speed u_eff as well as the best-fit linear horizontal and vertical shear coefficient time series between the target and reconstructed wind fields. These variables should play a large role in determining the turbine loads.

29. Pg. 12, ln. 308: "run at the downstream distance of 5 D" Please be more specific. The turbine of interest is located 5 D downstream of the upstream turbine?

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.

31. Pg. 18, ln. 440: "In addition, improved estimates of both rhoˆ2_E and sigmaˆ2_u are seen in Fig. 10c,d." There seem to be improvements at low wind speeds, but slightly worse performance at high wind speeds. Can you comment on this in the paper?

32. Figs. 10 and 11: I would suggest full captions.

33. Pg. 20, ln. 463: "focus the analysis on the SL, Grid, and Grid* configurations" These might be the most promising scan patterns, but also not the most likely, given currently available commercial lidar technology. It would be interesting to analyze the time series for one of the commercially-available lidar scenarios as well.

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?

35. Pg. 22, ln. 481: Usually magnitude-squared coherence is written as gammaˆ2 = abs(S_x,y)ˆ2/(S_x*S_y). Therefore, I would expect your definition to be gamma = abs(S_x,y)/sqrt(S_x*S_y). Is this correct?

36. Fig. 14: On the left plot, why is the baseline coherence so high at low frequencies (above the noise floor)?

37. Pg. 25, ln. 538: As mentioned earlier, the "need for reducing the statistical load prediction uncertainty" in wake conditions could be motivated more clearly in the paper.

More discussion or references talking about the need for improved methods would strengthen the message of the paper.

38. Pg. 26, ln. 562: You say that the lidar-based predicted load statistics are comparable to the results from the baseline DWM method (Delta_R between 0.97 - 1.01). However, from Figs. 9 and 11, it seems more accurate to say that Delta_R is between 0.92/0.94 and 1.01. Is 0.94 still an acceptable difference?

39. Pg. 27, ln. 610: Similarly, the range of Delta_R with the lidar-based method is more like 0.94-1.01 instead of 0.97-1.01. When saying that this is comparable with the baseline method, please be more specific about what "comparable" means.

40. Pg. 27, ln. 615: In addition to these lidar parameters, the load prediction accuracy is sensitive to the turbulence intensity as well.

41. Pg. 28, ln. 628: "largest energy content at higher frequencies (> 3P $\sim$ 0.3 Hz)." From the plots, the largest energy content is at very low frequencies and right at 3P, but > 3P does not contain as much energy content.

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