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Comment on wes-2022-62

Anonymous Referee #2

Referee comment on "Evaluation of lidar-assisted wind turbine control under various turbulence characteristics" by Feng Guo et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-62-RC2, 2022

This is an interesting manuscript that uses stochastic wind fields and aeroelastic simulations to examine the effectiveness of lidar-assisted control in reducing loads, as well as the lidar measurement coherence, for three different stability classes using both the Mann and Kaimal turbulence models. This is an important topic because lidar-assisted control is typically only evaluated using the default Mann and Kaimal turbulence conditions, based on neutral stability. Evaluating lidar-assisted control in different conditions more accurately indicates how well the control strategy will work in the variety of conditions encountered by turbines during their lifetime. The authors provide a detailed overview of the turbulence models and simulation process and show relevant metrics when presenting the results. However, I have one major technical comment on the manuscript as well as many smaller comments that I believe should be addressed.

When simulating wind fields for all three stability classes, the same turbulence intensity is used for all cases (IEC class 1A). But in reality, stable atmospheric conditions will typically have much lower turbulence levels than unstable conditions, with neutral being somewhere in between. Therefore, the conditions being simulated likely don't represent stable, neutral, and unstable conditions very well. Would you be able to include more realistic TI values for each stability? Or can you discuss why you are using the same TI for each stability class? Further, how accurate is the wind evolution model in the extended Mann model when using the unrealistic TI values for some stability classes? I assume it was developed using field measurements, but how well do these field measurements represent the class 1A turbulence simulated here for each stability class?

Another non-technical general comment is that there are many places in the manuscript where sentences are broken into two sentence fragments. For example, line 192: "It is clear that a larger coherent eddy structure… While the eddy structure is much smaller…", line 211: "It can be seen that the turbulence… While the variation in the anisotropy…", line 402: "To include the turbulence evolution… Four-dimensional stochastic turbulence…" I would suggest reviewing the manuscript and combining sentence fragments like these into single sentences.

Specific comments

1. Introduction: Much of the paper compares lidar measurement coherence between the Mann and Kaimal models. Since there has been some previous work in this area (e.g., Dong et al. (2021)), it would be helpful to discuss how this research compares to the previous work.
2. Line 34: "The lidar measurements can be contaminated by lateral and vertical wind speed components": to understand why lateral and vertical wind speed components "contaminate" the lidar measurements, it would be helpful to explain what you are trying to estimate (i.e., how do you define the REWS you are trying to estimate. The rotor average of the longitudinal component?)
3. Line 54: Can you provided a reference for ROSCO?
4. Fig. 1 caption: Can you provide a reference for how the length scales were chosen for each stability class?
5. Line 70: "Because the turbulence spectrum peaks…" This is an incomplete sentence.
6. Section 2.2: Are you assuming zero spatial coherence for the lateral and vertical velocity components? It would be helpful to discuss this here.
7. Section 2.3.1: The extended Mann model with evolution clearly shows a dependence on length scale (e.g., Eq. 14). Can you discuss how other wind conditions, such as turbulence intensity, affect the coherence? For example, in Simley and Pao (2015) there is a strong relationship between TI and coherence, but it isn't clear how this is captured in the extended Mann model.
8. Line 186: "impact on filter design for LAC": I would suggest explaining what filter you are referring to here.
9. Eq. 20: Why is the real number operator needed here? By definition, won't the coherence be a positive real number? Otherwise, can you explain how coh_{11} can contain imaginary components?
10. Line 212: "while the variation in the anisotropy Gamma does not show a clear trend towards the atmospherically." Based on Table 1, there is a clear trend between Gamma and stability. Are the values for Gamma and length scale in Table 1 switched perhaps?
11. Line 219: "we use three sets of gamma = 200, 400, and 600 s" Why did you choose these three values?
12. Line 222: "which is the median separation for a commercial lidar measuring in front of the turbine" Can you provide a reference or list some examples of commercial lidars and their measurement ranges?
13. Line 229-231: It is unclear what you mean by "rarely large a_x" and why this suggests you should use gamma = 600 s for the unstable case. More generally, can you discuss in more detail why you chose 600 s to represent the unstable condition (e.g., why not 500 s or 800 s)? Further, can you discuss how accurate the selected gamma values are for the class 1A turbulence intensity used in the simulations? And how would gamma change for different TI values? (e.g., Simley and Pao (2015) observed a strong relationship between TI and coherence).
14. Line 257: "azimuth angle phi and elevation angle beta" The math is hard to follow in this section without understanding how the azimuth and elevation angles are defined. Can you define these angles or show them in a figure?
15. Eq. 27-32: Shouldn't the angles phi and beta be a function of the lidar beam and therefore depend on the index "i"?
16. Eq. 31: I think there should be the imaginary number "i" in front of "k_1 Deltax_i". Also, as written, because Deltax_i equals x_i, it seems that S_RL(k_1) won't contain the phase delays between the measurement points and the rotor because the k_1 dependence of the exponent simplifies to exp(i(k1*x1 - k1*x_1)) = 1. Should Deltax_i

in the equation simply be replaced by x_R to model the correct phase delay?

17. Line 288: "the ith lidar measurement position" Can you clarify whether the index "i" refers to the lidar measurement position (e.g., combination of beam and range gate) or just the lidar beam? Earlier on line 267, the index "i" was described as representing the beam number.
18. Line 302: "typical four-beam pulse lidar trajectory": Can you discuss why this is "typical"? Are there commercial examples you could reference?
19. Table 3: As mentioned in an earlier comment, the azimuth and elevation angles haven't been defined. Can you define these or show them in a figure?
20. Line 330: What are the units of the cutoff frequency 10^-3?
21. Line 330: "This also indicates that the filter design is not sensitive to the change in turbulence parameters… and a constant filter design is robust." How does the filter design depend on the wind speed? Do the cutoff frequencies change?
22. Table 4: Please specify the units of the frequencies
23. Line 340: "to make each controller module as standard alone as possible," This sentence is a little confusing. What do you mean by "standard alone"?
24. Section 4.2: Do you model the time delay between measurement points due to the sequential scanning of the lidar in the simulations or assume that each point is measured at the same time?
25. Line 354: "the blockage effect" usually refers to the reduction in wind speeds upstream of a wind farm. Is this what you are referring to here? If not, I would suggest clarifying or using a different term.
26. Line 367: "we have chosen the option of constant power mode": Can you explain this control mode for readers unfamiliar with the term?
27. Eq. 36: This equation is hard to understand. Wouldn't the feedforward pitch command simply be theta_{ss}(u_{RR}) (i.e., the steady-state pitch angle as a function of wind speed)? Otherwise, please discuss why this equation is used and how it is derived.
28. Line 385: What is the value of the actuator delay that is used?
29. Eq. 39: This equation is also hard to understand. It seems like it is missing the actual time delay that you are trying to solve for. Also, should the 1-second lidar averaging delay be included here too? Further, is there enough lead time to account for the filter, pitch, and lidar averaging times for all cases analyzed?
30. Line 411: What is the mean flow field used for the Kaimal model-based wind fields? Is it the same as the power law shear mean flow field used for the Mann wind field?
31. Line 414: "The lengths in the y and z directions are both 150 m": It would be good to discuss why these lengths are smaller than for the Mann wind fields.
32. Line 416: "hub height wind speed from 14 m/s to 24 m/s with a step of 2 m/s are considered." It would be helpful to include simulation results for 12 m/s because this is above rated for the NREL 5 MW turbine and lidar-assisted pitch control would be active. Additionally, Table 4 lists the cutoff frequencies of the lidar filter for 16 m/s. How do the cutoff frequencies change for the different wind speeds simulated?
33. Fig. 7: Since you are using the constant power control mode (where typically torque is controlled to maintain constant power regardless of generator speed), it is surprising to see such high power fluctuations. Can you discuss why this is the case?
34. Line 454: "The spectra are averaged by different samples corresponding to the simulated results by different random seed numbers." This sentence is hard to understand.
35. Line 461: "In the unstable case, the RWES spectrum does not reduce a lot compared to a single point u spectrum…" To illustrate this point, it would be helpful to include the single point spectra in Fig. 8.
36. Line 474: "…which can be summarized as higher spectra in the rotor motion by the Kaimal model than the Mann model." It would be easier to see this if you used the same y axis limits in Figs. 9 and 10.
37. Line 481: "The reduction in the blade root out-of-plane motion is not very observable from the plots…" But significant reduction between 0.02 and 0.2 Hz can also be observed.

38. Section 5.2.3: It would be nice to add some more discussion to this section, for example, providing some reasons why the load reduction from lidar-assisted control might be different for the different stability classes.
39. Line 491: Can you provide a reference for "rain flow counting"?
40. Line 495: "For rotor speed, pitch rate… the standard deviation… is calculated". What is the significance of the std. dev. of pitch rate? Is this a common metric for pitch actuator damage? Why would this be used instead of the std. dev. of pitch angle, or the average pitch rate, etc.?
41. Line 522: "As for the electric power STD…": Again, why is there any significant power fluctuation, since the constant power control model is used?
42. Line 524: Why is there a significant reduction in mean power at 14 m/s?
43. Line 546: "the electricity productions are similar either using LAC or not…" Again, there is a significant drop in power at 14 m/s with LAC. What causes this?

Minor comments:

1. In many places throughout the manuscript, there are citations without parentheses, for example line 44: "Mann (1994)." If the reference is actively used as part of the sentence, it is ok to leave the parentheses out, such as lines 46-48. Otherwise, I suggest using parentheses, for example, as is done in line 25.
2. Eq. 15: Should the second "1" in F_11 be a written as a subscript as well?
3. Eq. 23: Can dk_2dk_3 be replaced by the symbol used in Eq. 8?
4. Line 315: "If a filter with the gain…" This sentence is hard to understand and appears to be incomplete.
5. Line 321: "natural" -> "neutral
6. Line 436: "Karmann"-> "Kalman"
7. Line 512: "14 m/s" -> "16 m/s"?
8. Line 541: "16 m/s" -> "18 m/s"?
9. Line 627: The paper "Dong et al. 2021" has been published as a full paper, so the reference should be updated.