Authors' response to Review 2 (2nd revision): Evaluation of lidar-assisted wind turbine control under various turbulence characteristics

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We would like to sincerely thank all the reviewers for taking their valuable time to read our manuscript and provide constructive comments. Special thanks to Review 2 for your careful review of the paper and further constructive comments in the second round of review.

Please find below our response to the reviewer 2's comments. The reviewer's comments in the report are repeated in black text, our response should be given in blue text, and if necessary, the corresponding corrections are provided in red text.

Response to comments of Anonymous Referee #2 (2nd round)

Overall comments

The revised manuscript has been greatly improved. I do have some remaining comments that I feel should be addressed, though. The main comments are related to a) including more discussion about how accurate the wind evolution parameters (gamma) used in the analysis are for different TI values, since you are just using a single value of gamma to represent each stability class regardless of the TI, and b) the assumptions made in the lidar spectrum and lidar-REWS cross spectrum equations. Please see responses to the remaining comments below (note: the comment numbers are different than the original numbers). Several new, mostly minor, comments are provided afterwards as well.

We would like to thank the referee for the positive feedback after the first round of revision on the manuscript.

1. Reviewer comment 1: 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.
Author response: Thanks for the your comments. We have reviewed the paper and modified the fragmentary sentences.
Reviewer comment 2: Many of these issues were resolved. However, there are still some incomplete sentences throughout the manuscript. For example, Figure 1 caption "Simulated using the 4D Mann..."; Ln 192: "To include the exponential longitudinal coherence model..."; Ln 357: "Apart from the case that all measurement gates..."; Ln 432: "Because the pitch curve has much higher..."; Ln. 503: "If we do not consider..."; Ln. 522: "Because the turbulence field has a..."
Thanks a lot for the reviewer's careful reading. We have modified the text to fix these issues. We also further examined the full text of the revised manuscript.

2. Reviewer comment 1: 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. Author response: Thanks for the your comments. The extended Mann model (space-time

tensor) assumes stationary process in time, the turbulence intensity is not affected by the wind evolution. Actually the wind evolution parameter is determined by the parameter "γ". So, based on specific situations, one can adjust γ and $\alpha\epsilon^{2/3}$ independently to reach a target turbulence intensity and evolution level. Reviewer comment 2: I understand that the turbulence intensity isn't affected by the wind evolution, but I am wondering how the wind evolution parameter gamma in Eq. 15 depends on the turbulence intensity. For example, Simley and Pao (2015) observed a strong relationship between turbulence intensity and the a_x parameter (in Eq. 18). Although Davoust and von Terzi (2016) didn't observe as strong of a relationship, there may be some dependence of the gamma parameter on TI. See comment #5 also.
Thanks a lot for the reviewer's comments. We have added some text in the end of section to point out the current shortage of our assumption. And propose suggestions for future research. The added text are: In addition, it is worth mention that we do not consider the dependence of the turbulence evolution parameters on TI level. The selection of turbulence evolution parameters is based on relevant studies and typical values are chosen. As studied by Simley and Pao (2015), the TI values can be different for the same atmospheric stability, and the evolution parameters show some dependence on the TI values. In the future, a joint probabilistic study on the turbulence spectral parameters, TI levels, and evolution parameters is necessary for defining more realistic simulation scenarios for LAC.

3. Reviewer comment 1: 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 coh11 can contain imaginary components? Author response: Thanks for the your comments. Indeed, the magnitude squared coherence is real and positive. In terms of the least square fitting in Equation (21) (previously 20), we are fitting the co-coherence. We have corrected the equation now and added Equation (10) to explain the definition of co-coherence. Reviewer comment 2: Can you explain why you are fitting the co-coherence instead of the magnitude squared coherence? Thanks a lot for the reviewer's comments. We chose to fit the co-coherence because the exponential coherence model (used for Kaimal spectra) is a real function and it only has co-coherence. We have added explanations in Ln 220.

4. Reviewer comment 1: Line 219: "we use three sets of gamma = 200, 400, and 600 s" Why did you choose these three values? Author response: Thanks for the question. We have added the reason as: "The reason for choosing these values for γ is that they result in coherence close to observations in existing literature, as will be discussed later". Reviewer comment 2: I have one minor comment, which is to be more specific about which section this will be discussed in "later". Thanks a lot for the reviewer's comments. We have added text to point out the explanation is given at the end of the section.

5. Reviewer comment 1: 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). Author response: Thanks for the question. We use the term "rarely" according to the probability study by Chen et al. (2020), but we did not write it clearly previously. We chose 600 s because it gives higher ax in the unstable condition than the neutral and stable conditions (in accordance with the LES-based observation by Simley and Pao (2015)). Overall, 600 s is chosen because it gives a reasonable ax value in terms of probability and relative difference with a_x from other stability. Now we have modified the sentence to be more clear. Regarding the second question, as explained in the general comment, we have not consider the variation in the TI to emphasize the study on the changes in turbulence length scale. The γ value is independent from the turbulence intensity in the space-time tensor. Also, since the turbulence intensity is adjusted by the parameter $\alpha\epsilon^{2/3}$, which is just a proportional gain. The changes in the $\alpha\epsilon^{2/3}$ will not affect the coherence of velocity components or lidar measurement. In reality, one can design simulations using different γ values to reach the target longitudinal coherence.
Reviewer comment 2: The added discussion helps clarify the choice of gamma = 600 s for the unstable case a lot. Regarding the comment about how accurate these values are for the class 1A turbulence intensity used, I understand that gamma is independent from TI in the model and you are free to use any combination of TI and gamma. But since gamma is an additional free variable, it has to be tuned, as discussed in the manuscript. It therefore could depend on TI (or other variables) in addition to stability. You chose three values of gamma for the three stability classes, but are these choices of gamma valid for all TI values within a certain stability class? It would be insightful if you could discuss how accurate you believe the choices of gamma are for the class 1A TI values you use in the paper and it would help to acknowledge that the three values selected may not be accurate for all TI values (including the class 1A TI used in the paper) if that is the case.
Thanks a lot for the reviewer's comments. We have added text in the end of section 2. We specify the realistic phenomenons that we did not consider and propose suggestions for further works. Please see Comment 2.

6. Reviewer comment 1: Eq. 31: I think there should be the imaginary number "i" in front of "$k_1\Delta x_i$". Also, as written, because Δx_i equals x_i, it seems that $\mathrm{SRL}(k_1)$ won't contain the phase delays between the measurement points and the rotor because the k_1 dependence of the exponent simplifies to $\exp(\mathrm{i}(k_1 \cdot x_1 - k_1 \cdot x_1) = 1$. Should Δx_i in the equation simply be replaced by x_R to model the correct phase delay? Author response: Thanks for the careful review. The reviewer is correct, the imaginary number "i" should be included in front of "$k_1\Delta x_i$". This has now be added. As for the second question, we added the detailed derivation below:...

Reviewer comment 2: Thanks for providing the derivations. I also see that the equation is nearly identical to the equation presented in Held and Mann, 2019. However, it still isn't clear why there doesn't appear to be any phase rotation for the k_1 frequency component due to the time shift between the lidar measurements and the REWS at the rotor plane in Eq. 32, assuming Taylor's hypothesis (i.e., $\exp(j \cdot k_1 \cdot \Delta_x)$), since the $k_1 x_i$ terms cancel out in the equation. Further, if the lidar-estimated REWS contains measurements at different range gates, I would expect the phase differences between the measurements at each range gate and the REWS at the rotor plane to appear in the equation. To better understand Eq. 32, as well as Eq. 29 for $S_{LL}(k_1)$, can you explain the assumptions in the derivations in more detail? For example, is the cross-spectrum in Eq. 32 derived assuming the lidar-estimated REWS is delayed in time according to Taylor's hypothesis so it is in phase with the REWS at the rotor plane? Similarly, when averaging lidar measurements at different range gates (Eq. 28), do you delay the measurements at different range gates in time so they are in phase with the nearest range gate, according to Taylor's hypothesis, before averaging? If not, how is the phase delay between measurements at different range gates accounted for in Eq. 32 (it seems like it is already included in Eq. 29)?
Thanks a lot for the reviewer's comments and suggestions. We have added text in Ln 313 to specify the assumptions we have made when deriving Equation 32. As for Equation 29, we kept the Equation but explain that the data is phase shifted in practical LAC so that there is no phase shift caused by the longitudinal seperations. Please see Ln 298.

7. Reviewer comment 1: 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? Author response: Thanks for pointing this out, we have removed Table 5 and added Figure 5, which shows the cutoff frequencies as a function of the mean wind speed. We also added the discussions around the line 340 as: "The cutoff frequencies as a function of mean wind speed are calculated by fitting the GRL and are shown in Figure 5. Firstly, both turbulence models indicate that the cutoff frequencies depend on the mean wind speed linearly. Therefore, the cutoff frequency of the filter can be scheduled based on this linearity"
Reviewer comment 2: When varying the wind speed to determine the cutoff frequencies, are you keeping the TI constant or changing it according to the IEC standard?
Thanks a lot for the reviewer's comments. We have clreaify this in Ln 354. We indeed adjusted the TI by the mean wind speed according to IEC standard.

8. Reviewer comment 1: 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? Author response: Thanks for your comment. The reason of the lower mean power at 14m/s has been explained in 42. We now analyzed the reason for power drop at 14m/s in line 582. However, we did not get the reviews

opinion that the EP is reduced with LAC. In the plot, the right side y axis is the relative reduction compared to FB-only control. If it is negative value, it means the FFFB gives higher value compared to FB-only control. So the EP is actually increased (very slightly) with LAC. However, since the increment is marginal, our conclusion is that LAC has marginal impact on the EP.
Reviewer comment 2: My mistake. I misinterpreted the meaning of the negative reduction in energy production in the plots. Thanks for your feedback.

9. Reviewer comment 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. Author response: Thanks for the careful suggestion. We have went through the paper and corrected relative citations.
Reviewer comment 2: The citations have been improved significantly. There might be a few that still are missing parentheses, however. For example, Ln. 315: "Schlipf et al." Thanks for your feedback, we have checked again the citations through the manuscript.

10. Reviewer comment 1: Line 315: "If a filter with the gain. . . " This sentence is hard to understand and appears to be incomplete. Author response: Thanks for pointing this out, this sentence is now rewritten as: "If a filter is designed to have a gain of GRL(f), it turns out to be an optimal Wiener filter (Simley and Pao (2013), Wiener et al. (1964)), which results in minimal output variance for a multi-inputs multi-outputs system."
Reviewer comment 2: The phrase "results in minimal output variance for a multi-inputs multi-outputs system" could use some explanation. What does this mean in the context of the LAC application, and what specific variance does the filter minimize in this application? Thanks for your feedback, we have added the explanation in Ln 345 as: "For example, in LAC, if the system is modeled as a system with two inputs: REWS and lidar-estimated REWS, and one output: rotor speed, the Wiener filter leads to minimal rotor speed variance (Simley and Pao, 2013).

New Comments:

1. Ln. 87: Consider providing a little more information about ROSCO here. For example, that it is a reference controller representing an industry standard control system.
Thanks for your feedback, we have added the explanation in Ln 61 as: "ROSCO is an open, modular, and fully adaptable baseline wind turbine controller with industry-standard functionality.

2. Ln. 206: "In this work, we emphasize analyzing the impact of turbulence length scale on turbine loads and LAC benefits": Would it be more accurate to say that you are analyzing the impact of turbulence length and the Gamma anisotropy parameter as well, since Gamma is different for the three stability classes?
Thanks for your feedback, we have added the anisotropy in the sentence.

3. Ln. 220: Please define the quad-coherence

Thanks for your suggestion, we have added the definition in Ln 144.

4. Ln. 225: "In their study, a smaller intercept was found for a more stable class. Also, Simley and Pao (2015) studied the turbulence...": It would be worth discussing whether the longitudinal coherence from the modified Mann model also shows different intercepts for different stability classes, even though the Mann model may not explicitly have an intercept parameter like b_x.

Thanks for the comment, actually we have shown these in Figure 2(c). The blue lines are the longitudinal coherence from the three stability conditions. It can be seen that the blue lines have a lower interception than the other two lines. To make a connection to the figure, we have added a bracket in the end of the sentence to indicate that the results are shwon in Figure 2.

5. Figure 2: It would help to state what mean wind speed these spectra are generated for.

Thanks for your suggestion, we have added the mean wind speed in the Figure caption.

6. Section 3.2: I think more details about how the lidar-estimated REWS is formed should be provided here to better understand the spectrum calculations. In particular, how do you combine measurements at different range gates? Do you delay the measurements from the farther range gates according to Taylor's hypothesis so they are in phase with the measurements from the closest range gate before averaging? Or do you average all measurements at the same time? This decision should affect the spectrum equation in Eq. 29.

Thanks for your suggestion, we have added more discussions after Ln 300.

7. Eq. 28: Is this equation missing a negative sign? According to the angle definitions in Fig. 3, cos(phi) will be negative. If the LOS velocity $v_{los,i}$ is positive, then u_{LL} will be negative as written.

Thanks for your question. Indeed the first element of the unit vector is negative. In our implementation, we always use the inertial coordinate system shown in Figure 3 and use Equation 27 to calculate the LOS speed. So LOS speed is also negative. Then, the u_{LL} will be positive.

8. Figure 4: Can you list what wind speed these coherence and transfer function curves are generated for? Thanks for your suggestion, we have added the mean wind speed in the Figure caption.

9. Ln. 403: "$M_g = P_{\mathrm{rated}}/(\eta\Omega_{\mathrm{gf}})$": the way this is written, it is unclear whether Ω_{gf}) is in the numerator or denominator. This comments applies to line 503 as well.

Thanks for your careful reading, we have added a bracket to correct the formulas.

10. Ln. 412: "lidar-assisted pitch forward signal": Be consistent about "forward" vs. "feedforward" throughout the text.
Thanks for your careful reading, we have searched whole text to unify the wording.

11. Ln. 419: "where f_c is the cutoff frequency": I think it would help to mention that this is the same cutoff frequency that is discussed in Sect. 3.4.
Thanks for your suggestion, we have added the connection.

12. Ln. 420: "The pitch forward signal is then sent to ROSCO after accounting for the pitch actuator delay...": Should the filter delay T_{filter} also be mentioned in this sentence?
Thanks for your suggestion. You are right. We have added the filter delay.

13. Ln. 421: "and the half of the time averaging window". Consider clarifying by saying this is "half of the lidar scan time averaging window" or similar.
Thanks for your suggestion. We have added definition that time averaging window equals to the lidar full scan time.

14. Ln. 425: "...is chosen to be half of the time averaging window". Why is T_{window} set to half the time averaging window? Doesn't the factor of one half already appear in Eq. 40, meaning T_{window} should be the full averaging time window?
Thanks for your careful reading. It was a mistake and we have corrected this. The new sentence is:Here, Twindow=1 s is the time averaging window equivalent to one full scan time tof the lidar. It is multiplied by 1/2 in Equation 42, because of the phase delay property of the time averaging filter (Lee et al., 2018).

15. Ln. 457: "Each 4D turbulence field has a size of 4096 x 11 x 64 x 64 grid points...": Can you mention the time resolution here?
Thanks for your suggestion. We have added the time step (0.5s) in Ln 489.

16. Ln. 616: "The electrical power generation is not affected by introducing LAC.": There is a small change, so perhaps "not significantly affected" would be more appropriate.
Thanks for your suggestion. We have added 'not significantly'.

17. Ln. 669: "also reduces the variation in rotor speed, pitch rate, and electrical power clearly": Why are reductions in pitch rate mentioned here for the Kaimal model, but not on line 664 for the Mann model?
Thanks for your careful reading. That was a mistake and we have added it for Mann model.

18. Ln. 675: "Overall, we found the benefits of lidar-assisted control by the Kaimal model are slightly different from the results obtained using the Mann model.":

Are there any interesting differences to mention here?
Thanks for your suggestion. We added more discussion as: The benefits of lidar-assisted control simulated using the Mann model is slightly better than that using the Kamal model, which can be caused by differences in the turbulence spatial coherence between two models. The lidar preview quality modeled using the Mann model is generally superior to that modeled using the Kaimal model.

New minor comments:

1. Ln. 57: "acting" − > "acts"? Thanks for your suggestion. We corrected this in the second revision.

2. Ln. 61: This may be a personal preference, but I think it is helpful to spell out acronyms like "ROSCO" in the body of the text the first time, even if they are defined in the abstract as well. Thanks for your suggestion. We corrected this in the second revision.

3. Ln. 63: can you provide references for FAST and OpenFAST? Thanks for your suggestion. We added references.

4. Ln. 67: "tool" − > "tools"? Thanks for your suggestion. We corrected this in the second revision.

5. Figure 1 caption: Be consistent on the use of "pulsed lidar" vs. "pulse lidar" throughout the paper. Thanks for your suggestion. We corrected this in the second revision.

6. Ln. 71: "value" − > "values"? Thanks for your suggestion. We corrected this in the second revision.

7. Ln. 140: "interested" − > "interesting"? Thanks for your suggestion. We corrected this in the second revision.

8. Ln. 244: "gives large values of a_x": To make the point more clear, should this say something like "gives unrealistically large values of a_x"? Thanks for your suggestion. We corrected this in the second revision.

9. Ln. 380: "propriety" − > "property"? Thanks for your suggestion. We corrected this in the second revision.

10. Ln. 501: "even the constant" − > "even though the constant"? Thanks for your suggestion. We corrected this in the second revision.

11. Ln. 503: "$M_g = P_{\mathrm{rated}}/(\eta\omega_{\mathrm{gf}})$": In section 4.3, the generator speed is written as capital Omega. Should it be the same here? Thanks for your suggestion. We

corrected this in the second revision.

12. Ln. 511: "less low-frequency rotor speed fluctuation": This is a little confusing. Consider rephrasing as "reduced low-frequency rotor speed fluctuations" or similar. Thanks for your suggestion. We corrected this in the second revision.

13. Ln. 521: "RWES" − >"REWS" Thanks for your suggestion. We corrected this in the second revision.

14. Ln. 531: "most interested" − > "most interesting"? Thanks for your suggestion. We corrected this in the second revision.

15. Ln. 544: "1 p": Usually I see this written with a capital "P" Thanks for your suggestion. We added definition of 1p and 3p but kept lower case.

16. Ln. 556: "statics" − > "statistics" Thanks for your suggestion. We corrected this in the second revision.