Response to Referee #1

We thank the referee for their review and their thoughtful comments. Point-to-point responses can be found below, and the relevant changes will be made to the manuscript during the revised manuscript submission stage.

Main comments:

Comment #1
I sincerely thank you for your invaluable contributions to the research field. Your manuscript contains notable and novel contributions that further the state of the art in wind farm control. I find the scientific relevance and level of detail both excellent. I very much enjoyed the thorough literature review in the introduction, and how this article combines both a high-quality large-eddy simulation study with an advanced wind farm control solution. Important findings regarding wind direction forecasting, real-time model parameter estimation and yaw optimization is likely to shape the next step in wake steering after open-loop control. Generally, the paper is dense in information but does not go too far in that matter. I only have a handful of minor remarks.

Response
Thank you for your thoughtful feedback. We appreciate your insights and provoking questions which will help guide the present and future work.

Point comments:

Comment #1
Can you say more about the inherent assumptions in Equation (1)? Are you assuming wakes to propagate instantly? Is there anything you can do to include time dependency in this optimization?

Response
In our approach, we use a steady-state wake model to optimize the yaw set-points. This inherently assumes that the model-optimal yaw set-points resulting from maximizing power in the wake model are appropriate yaw set-points to use in the wind farm LES. Given the steady-state wake model, the primary assumption in Eq. (1) is that the flow is statistically steady-state from t_0 to $t_0 + T$, where t_0 is the current time and T is the yaw update period. This inherently neglects the time delay associated with the yaw maneuver (i.e. from the current yaw state to the yaw state γ_s resulting from Eq. (1) [see ref. 1]). It also neglects the energetic cost associated with the yaw motor actuation. We assume that wind direction variations over t_0 to $t_0 + T$ can be accounted for with $f(\alpha)$. We have added more discussion of the inherent assumptions in Eq. (1) to the manuscript.

Comment #2
Equation (2), near line 150, you state that the wind direction is assumed to be uniformly distributed. In other work, the wind direction is often assumed to have a Gaussian distribution (Rott et al., Simley et al.) or a Laplace distribution (Quick et al.). Could you explain your choice?

Response
Thank you for noting this. Yes, previous authors, Rott et al., Simley et al., and Quick et al used Gaussian or Laplacian distributions to describe variations of the wind direction about a statistically stationary

mean. We used uniform distributions for $f(\alpha)$ primarily because the wind direction is statistically non-stationary in our simulations (see e.g. Figure 3 in the paper). For a wind direction which is evolving in time (e.g. over the window t_0 to $t_0 + T$), assuming a normal distribution about a stationary mean state may underpredict the probability of deviations from the mean state. The best choice of $f(\alpha)$ will also inherently depend on T in transient flow. For smaller values of T, a normal distribution is more appropriate, as shown by previous authors (e.g. 5 minute window in Rott et al). Using a uniform distribution is likely not the best choice. We recommend future work to investigate the appropriate representation of $f(\alpha)$ in evolving ABL conditions with mesoscale and diurnal cycle driven non-stationarity. We have added more discussion to the manuscript.

Comment #3

Line 177: "The standard approach … direction filter used." You mention a "low-pass moving averaged filtered wind direction". A moving average filter also falls within the class of lowpass filters, as far as I understand it. The way you specify it; do you mean that you used two filters, one to lowpass filter the signal and one to additionally calculate a moving average from those filtered values? Similarly, you cite Simley et al. (2020) to use a "first order filter", but this is also a lowpass filter. You state that the results do not really change much based on which filter is used, and I assume you are already aware of the things I stated here, but this paragraph was not completely clear for me.

Response

Thank you for this note, we apologize that the paragraph was confusing to read and we have modified the language to improve clarity. Yes, a moving average is a type of low-pass filter, as is the first order filter in Simley et al. (2020). We do not include an additional low pass filter beyond the moving average.

Comment #4

Line 185, top of page 7: I really appreciate the simplicity of assuming a linear trend for the wind direction, if a certain threshold is met. Is there a theoretical motion that would support the decision to model this as a linear process?

Response

To the best of the authors knowledge, there is not a justification to model the wind direction trend as a linear process other than the empirical results of the present diurnal cycle simulation. We recommend further investigation of short-term wind direction forecasts in future work. Such methods could include either physics- (based on the momentum equations) or data-driven approaches. We have added discussion of this topic to the paper.

Comment #5

Line 195, Section 3: To de-condense the text somewhat, please consider putting important details of the LES simulation in a table.

Response

We have put the simulation details in a table.

Comment #6

Line 214: Perhaps remove footnote 1 and instead add an entry to the reference list.

Response

We have added the code *PadeOps* as a reference item.

Comment #7

Page 8, Figure 1, and lines 219 until 227: I was wondering if this information is essential in the main text. I do see the value of explaining why and how the wind direction changes, but perhaps it is not essential to the story you are trying to tell in this article. Showing figure 3 should provide the reader with sufficient information in how the ambient conditions will change and under what conditions the turbines and the wind farm controllers are subjected. Perhaps some of this information can be moved to an appendix.

Response

Thank you for this comment. We have moved Figure 1 to the appendix as the referee has suggested. I appreciate the need to write concisely to not complicate the primary narrative of the paper. However, I believe this short discussion regarding the wind condition variations is useful information to understand the character of the atmospheric boundary layer conditions that the wind farm will experience and to provide physical intuition for the time-varying nature of the flow. I hope that this discussion will help to elucidate the physical mechanisms which cause ABL variations to which the controller reacts in the results section.

Comment #8

Figure 4: I think this figure is really interesting. You may consider it moving to an appendix, as stated in the previous remark. Also, could you please add a legend defining the time window for each vertical line.

Response

We have added a legend specifying the time window for each vertical line. As with **Comment #7**, we believe the nature of the wind conditions and their variation in time are of importance to the wind farm control strategy and also the interpretation of the results.

Comment #9

Figure 5: this is a very informative figure. It is a little difficult to see at the current resolution. Could you perhaps update the xlims/ylims, zooming in to the region of interest? Also, if can consider removing the yticks and ylabels from subplots (b) and (d), and similarly for the x-axis for plots (a) and (b).

Response

We have added a zoomed figure in the Appendix. I prefer not to put only the zoomed figure in the main text as this may confuse readers into thinking that is our full computational domain, but it is important readers are able to see the details in the zoomed figure, so we have added it to the Appendix.

Comment #10

Line 225, Section 4: After reading "Case A" (and "Case D" later on), I was expecting to also see a "Case B" and "Case C". I later realized what they were actually supposed to mean. To clarify this and also to de-condense the text somewhat, please add a table defining the various cases. I think with that table,

you can keep the case naming convention you have now. A table would really make it easier for the reader to see what cases were tested and what combinations of controllers, prediction vs. past-time window- averaged wind direction estimates, with and without uncertainty.

Response

We have added a table describing each case.

Comment #11

Line 307: The default cases are presented with a control update period of T=30 minutes. This seems very high to me, especially if you are anticipating a change in the mean wind direction. It there a reason you picked such a high value to start with?

Response

Thanks for this question. The timescale with which the yaw misalignment should be updated inherently depends on the incident wind conditions and the timescales of their variability. We have submitted a separate paper to *ACC* to investigate the influence of the stability in the ABL on the best yaw update period T, which was not the primary focus of the present study. In the present study, we investigated T=15 min and 30 min. In the present idealized diurnal cycle simulations, the variation of the mean wind conditions are relatively slow (diurnal timescales) and a longer update period is more justified. Future work should investigate the influence of mesoscale structures in the atmosphere (not present in microscale LES) on the optimal update period T.

Comment #12

Page 14: explanations are very clear, really excellent.

Response

We thank the referee for this comment.

Comment #13

Figure 9: plots are somewhat small, while the xlabel and ylabel are large. Could you enlarge the actual plots? Also, could you export these plots in vectorized format (.pdf, .eps) so that I can zoom in at a high resolution?

Response

We have enlarged the plot in the revised manuscript. The plots are in vectorized format (.eps) which I believe should be available when the paper is posted online? If not, please reach out to me at mhowland@mit.edu for the plots. Alternatively, the dataset is already posted online here: https://doi.org/10.5281/zenodo.5160943

Comment #14

Figure 10: Can the EnKF estimate the model parameters if there is no wake interaction? Will your estimates drift off if you are without wake interaction for an extended period of time?

Response

We have a simple geometry-based wake detection algorithm (based on the wake diameter and wake expansion as a function of downwind distance) that avoids calculating wake contributions when there are no interactions for reduced computational effort. So our EnKF does not estimate parameters if there

is no wake interaction. Regarding the details of the EnKF if we did not have this flag, the wake model parameters would not be statistically altered if there were no wake interactions because the power prediction deviation matrix $\widehat{\Pi}' = 0$ (see Ref [2]).

Comment #15
Line 356: "The wake model… exhibit low error." If you use power production measurements in your EnKF, is the model-predicted power production a fair value to use in validation? Had you calibrated your EnKF to have a much lower measurement noise covariance matrix than the process noise cov. matrix, then would you not only further improve this quantity?
Response
The referee is exactly correct that this is simply a statement that the calibration (training) error is low and that it could be even further reduced. We did not intend this statement to reflect a validation of the EnKF approach. In fact, this is the focus of Figure 12 where we compared wake model predictions (out-of-sample) to the LES output. The EnKF reduces the error of the predictions (out-of-sample) in addition to the reduction of the calibration error. We have further clarified this line in the manuscript.

Comment #16
How large is the sample pool in the EnKF, and what did you base this on?
Response
I assume the referee is referring to the number of ensemble members, for which we use 100, a commonly used number [3] which we also used in Part 1 [2]. We selected 100 based on offline hyperparameter tuning experiments (see Ref. [2]).

Comment #17
Figure 12 says "Case SF", but no such case was introduced in the text. Did you mean OOU-F?
Response
Thank you for catching this typographical error. We have fixed it.

Comment #18
I would very much like to see you separate the update/averaging sampling time for the model estimation part from the control setpoint update rate. Naturally, I can see that you may not need to update the model parameters very frequently, notably since perhaps the wake expansion parameter need not change very fast. However, the optimal yaw setpoint may need to change much at a much higher frequency. I think there still may be a lot to gain here. You rightfully mention it in your text, but I wanted to emphasize that I am very curious to see how your results would change. Again, this is not something I expect you to address in this manuscript.
Response
Thank you for this comment. I agree entirely that it is likely the parameters could be updated with a slower frequency than the yaw, and that we can/should decouple these two periods. We have begun an investigation in this direction, and we anticipate investigating it more thoroughly in future work. There are several technical and logistical challenges which need to be overcome to enable this decoupling.

Future work should primarily consider what are the atmospheric determinants which affect the wake spreading rate to identify with what frequency they should be updated.

Comment #19

Line 448-465: I wonder if these two paragraphs could be omitted. They seem to be a general summary of the methodology and background information. I think you can assume that the reader has read at the very least the introduction of your manuscript. This would make the manuscript's results and conclusions stand out more.

Response

Thanks for this comment. We have removed the first paragraph from the conclusions to, as the referee has suggested, allow the conclusions and discussion stand out.

References

[1] Macrí, Stefano, Sandrine Aubrun, Annie Leroy, and Nicolas Girard. "Experimental investigation of wind turbine wake and load dynamics during yaw maneuvers." *Wind Energy Science* 6, no. 2 (2021): 585-599.

[2] Howland, Michael F., Aditya S. Ghate, Sanjiva K. Lele, and John O. Dabiri. "Optimal closed-loop wake steering–Part 1: Conventionally neutral atmospheric boundary layer conditions." *Wind Energy Science* 5, no. 4 (2020): 1315-1338.

[3] Howland, Michael F., Oliver RA Dunbar, and Tapio Schneider. "Parameter uncertainty quantification in an idealized GCM with a seasonal cycle." *arXiv preprint arXiv:2108.00827* (2021).