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 submission stage (https://www.wind-energy-science.net/peer_review/interactive_review_process.html), colored in red.

Comment #1

Wake model. Overall, the inclusion of the wake model seems to be the weak, but predominant, part of the article. That is not a criticism of the wake model itself. The lifting line wake model definitely seems to have merit, and engineering models are important. It is fully understandable that model developers wish to spread their model and test their capabilities. However, the present application should be better addressed and motivated. Does the analysis actually strengthen or weakening the general use of the wake model? Does the inclusion of the wake model improve the potential for closed-loop wind farm control, or are the results so model dependent that one should rather use a more model-free approach?

Response

Thank you for this comment. We will improve the motivation of the wake model in the manuscript and we will briefly summarize that justification and rationale here. In general, model-free wind farm power maximization has significantly slower rates of convergence than model-based optimization and may therefore be less well suited for closed-loop control in a practical, transient wind farm setting (see recent discussion by [1,2]). Model-free formulations have not been vigorously tested in transient simulation mean conditions aside from the recent paper by Ciri et al (2019) [3].

Given computational cost considerations for real-time control (i.e. computational limitations at an operational wind farm), a focus of the wind farm controls community has been to leverage existing, computationally efficient steady-state wake models for closed-loop control (see, e.g., review by Doekemeijer, Fleming & Willem van Wingerden (2019) [1]).

As the referee has suggested, an overarching objective of the submitted manuscript is to examine the sensitivity of closed-loop wind farm power maximization control to common wake model parameters and assumptions. More specifically, the assumptions invoked in the wake model presented here are functionally similar to assumptions in the popular FLORIS model software. The results in this manuscript suggest that wake models do lead to beneficial power production increases for turbine arrays, provided that the parameters in the wake models can be accurately estimated *a priori* or in a closed-loop fashion.

While model-free wind farm control is an interesting line of research moving forward, generalizations of such approaches to transient atmospheric conditions require further rigorous testing. Future work in this community (as recently discussed at the IEA Topical Expert Meeting on Wind Farm Controls) should establish standard test problems and benchmarks for the comparison of various optimization formulations such as the presently proposed methodology and model-free formulations.

Comment #2

It seems that the calibration of model parameters frequently ends up compensating for the missing physics, which affects several of my following points. This is also one of the major self-contradictions within the article. On one hand, the authors repeatedly write that the model is "physics-based", which in isolation is correct. However, the current application of the model also includes several other models/assumptions, which in combination is questionable. It also leads the authors to give statements like:

- "the two parameter lifting line model may be overparameterized, which leads to overfitting."
- "The ability of a one or two parameter analytical wake model ....may enforce unreal-istic model parameters to represent neglected physics.".

Clearly, the model does not capture all the physics. Hence, the desirable physics of the original model appears to become a limitation, rather than an benefit, when various additional models and assumptions are combined, see below. The results and the aforementioned quotes in-dicate these concerns, but it is not reflected in the abstract nor in the conclusion, which hence seems somewhat selective. I will try to elaborate on a number of points, where this seems to have a significant impact.

Response

Thank you for this thorough comment. We will update the abstract and conclusions to further reflect the sensitivity analyses which were a major component of this research.

Due to the underlying assumptions of wake models and yaw misalignment wake deflection models, these methods cannot be expected to capture all relevant physics for full-scale wind farm operation. In fact, the use of state and parameter estimation methodologies to correct unresolved physics or imperfect model assumptions have been commonly used for wind farm controls applications [see e.g. 4, 5].

Within the current closed-loop framework, the goal of the EnKF model parameter estimation is to accurately fit to a current timestep and then to apply those parameters for optimization only for the very next timestep. While it is reasonable to question the generalization of the site-specific fitting, the purpose here is not to create general parameterizations of the model parameters to be used for other conditions or wind farms but to only apply the parameters from one control step to the next. This objective and fitting consequence is thoroughly discussed in the text of this article (and will be detailed in the abstract and conclusion of the updated manuscript as per the referees comments).

More importantly, the ***predictive*** ability of this approach was tested here in Figures 21 and 22. As discussed in the manuscript *Section 5.4,* the present wake model approach with EnKF state estimation significantly outperforms the predictive capabilities of the commonly used Gaussian wake model with pre-defined empirical parameters with 5x lower predictive error. Therefore, with the site-specific fitting, the EnKF estimation of the model parameters significantly improves the model predictive ability. The improvement in the predictive ability of wake models with optimized model parameters is also shown in the very recent paper [2] (published online April 29, 2020).

Future work will focus on the constraining of the state estimation methodology to reduce model flexibility to test whether this improves the predictive success.

Comment #2(a)(1)

Model Description and Assumptions:
- The assumption of thrust following cosˆ2gamma could potentially have a big impact on the results. The article investigates PP ,but this seems to be an equally important assumption and the impact should not be neglected.

Response
The authors agree that the wake model assumption of thrust following cosˆ2(gamma) could also have an influence on the closed-loop control performance. This was not tested in this study since the actuator disk model used directly enforces the thrust to follow cosˆ2(gamma) and since this assumption is enforced in the lifting line model derivation [6]. In general, the thrust would follow cosˆTp(gamma), where Tp is likely less than 2 (see e.g. Bastankhah and Porte-Agel (2016) [7]). In this event, the closed-loop model parameter estimation would likely compensate by predicting smaller wake expansion coefficients (which leads to stronger wakes) than the true value given the model expectation of less streamwise thrust than is imposed in reality. We will update the manuscript with further discussion about this wake model assumption.

Comment #2(a)(2)
The article states "Future work should focus on methodologies to robustly estimate PP from SCADA data". That's in principle good, but I question the statement"there is no accepted framework for determining PP"(line 635). It seems that it could relatively easily to perform simulations using an actuator disc model or an aero-elastictool, e.g. FAST, to fit both PP and the coefficient for CT as function of yaw for the specific turbine. This would render Section 5.3 obsolete as it stands now. The sensitivity to PP is obviously interesting, but it would seem more appropriate to address it using your reference to Liew et al. as also commented in line 650.

Response
As discussed by Liew *et al.* (2020) [8], both actuator disk theory and blade element momentum theory (e.g. FAST) return Pp=3. Wind tunnel experiments [e.g. 9] and large eddy simulations [e.g. 10] have shown that Pp is more typically between 1.5-2.
Further, Pp is likely turbine specific (see e.g. Liew *et al.* (2020) [8]) and has functional dependence on shear, veer, atmospheric stability, wake impingement, and likely other parameters. Field experiments (on-going by the authors) to robustly calculate Pp require weeks or months of experimentation due to the functional dependencies mentioned above and are therefore expensive and time consuming. Therefore, the authors believe that the quantification of the influence of Pp under model parameter uncertainty are critical to future wake steering deployments.

Comment #2(a)(3)
- Linear superposition: This is a highly questionable assumption, which will also affect the results in terms of the overfitting and parameter compensating for lack of physics. An improved wake superposition has recently been proposed by Zong and Porté-Agel,2020, which is physically consistent and shows improved results, also for wake steering

Response

While the linear wake superposition method (or sum-or-squares superposition commonly used in FLORIS) are often used, the referee is correct that their physical justification is challenging. Zong and Porte-Agel (2020) (which was published after this article was submitted for review) have thoroughly addressed this long-standing open-question in the literature. The method of Zong and Porte-Agel (2020) leverages an iterative approach and assumes an empirical, pre-defined prescription of the wake model parameters which differs from the present model approach. Future work will incorporate the new superposition methodology with optimal parameter estimation.

Comment #2(a)(4)
- Please define the "effective velocity" (line 136). Does that correspond to rotor averaged or based on the power production?

Response
The effective velocity is the rotor averaged velocity, we will adjust the language in the manuscript to clarify this definition.

Comment #2(b)
Advection time: This section could be significantly reduced and rephrased. Most of the explanation deals with Taylors hypothesis, but at the end the advection time is adjusted to be twice this "to account for errors associated with the simple advec-tion model". This appears as a somewhat random choice in the current context. The proper explanation could/should involve (conservative) estimates of the wake propagation velocity, i.e. the wake propagates slower than the mean flow. There are numerous references for this.

Response
Thank you for this comment. We will rephrase this section for brevity and include additional wake propagation references in the manuscript. The invocation of twice the Taylor's hypothesis advection time was to have a strongly conservative estimate. Since the flow is quasi-statistically stationary (i.e. statistically stationary except for inertial oscillations which occur on Coriolis time scales), conservative estimates for the advection time and time-averaging of statistics have been taken. The influence of the advection time scale assumption was tested in the initial manuscript where it was shown that it did not play a significant role in the power production output of the closed-loop control, and the results are given in Table 1. Part 2, which studies the transient diurnal cycle, will investigate these times scales in more detail.

Comment #3
Introduction. Although a good review is appreciated, it could be shortened and more focused. The section on derating seems too long for an article focusing on wake steering. Wind farm control is a very active field, so new articles are constantly being publish. Hence, I will just recommend the inclusion the recent review by Kheirabadiand Nagamune, 2019, which also quantifies the potentials of wind farm control. As also concluded by Kheirabadi and Nagamune, wake steering has been shown to have the largest potential, but their Figure 4 also reveal that power increase from wake steering is not guaranteed. Hence, some statements could be less assured, e.g. line 116 in the article.

Response
We will rephrase the introduction and streamline the discussion. We will include the reference to the review paper the referee has noted.

Comment #4
Steady vs dynamics: Generally, it seems "unfair" to use a steady state model in a dynamic framework, although one can argue if 30min is even dynamic. And by "unfair", I mean on the analytical model. How would the performance be if it was applied on a more realistic control scenario of 5-10min? I suspect this is given in Part 2, but this should be a self-contained article. There are several places, where the wording appears inconsistent. This is particular the case in Section 4, where "the flow is stationary" is used several times and at the same time "the dynamics of the wake" and "counter rotating vortices". Such statements appear contradictory. LES simulations of the flow behind a disc/rotor should not be stationary, even with uniform inflow. The wake will be dynamic (as you mention), e.g.meandering. This is also seen in Figure 5. There it appears as if the two turbines are positioned so close that the first wake doesn't start to meander/breakdown, but the second does. Could the quasi-steady behavior be caused by the coarse resolution of 5-6 cells per disc?

Response
Thank you for noting this language confusion. The statements should read "statistically stationary" rather than just "stationary" and we will modify the manuscript to improve clarity and we apologize for the confusion this language has caused.
The starting point for closed-loop wake steering control is to leverage existing computationally efficient steady-state wake models and to invoke quasi-stationarity in the flow statistics (see e.g. very recent paper by Doekemeijer et al. (2020), [2]). While the test case in Part 1 explicitly allows for a well-defined assumption of quasi-statistical stationarity, Part 2 will investigate this assumption's utility in a realistic transient ABL state.
Future work should also focus on the synthesis and validation of computationally efficient, transient models for wind farm power prediction.

Comment #5
Several question arise from Section 4:
- Is uniform inflow a good and representative case? Does it truly provide validation of optimization, because it turns the turbine in the right direction?
- Line 285 states that AD is good for far wake. But is 4D far wake in uniform inflow? A quick back-of-the-envelope assessment for where the far wakestart can be done using the engineering expression in Sørensen et al., 2015. For a 0.01% inflow turbulence and propagation velocity of 0.7, I get an estimate of 13-14D downstream, before it is fully transitioned to far wake with a Gaussian velocity deficit, which is another of the model assumptions. What is the implication of this assumption?
- Please rephrase for clarification line 377: "reduced in size and intensity". What is reduced in size and what intensity? The deficit? It would be difficult(=impossible) to see from an instantaneous velocity contour. Similarly, it is not possible to see how the figure "indicating that

for larger columns of turbines the potential for power increases due to wake steering are larger" due to the dynamics of the instantaneous flow.

Response

Thank you for this comment. While not physical, uniform inflow (zero freestream turbulence) represents a helpful, well-defined case for algorithmic testing and code validation. As the referee has noted, due to the small turbulence mixing in the wake shear layer, the top-hat profile wake will remain further downstream than in a realistic wind turbine wake. Therefore, this example is not a precise representative case where the assumptions of the wake model are exactly matched.

We will rephrase line 377 for clarity and improve the quality of the figures.

To reduce confusion of this section with physical results (atmospheric boundary layer simulations) we will move this section to the Appendix in the revised manuscript.

Comment #6

Quantification. Several issues relating to quantification could be improved and clarified.

- Are the power productions in Table 1 normally distribution? Otherwise, the standard deviation could be biased and misleading.
- The inclusion of whether results are significant can only be applauded. Line 434 defines "significant superior...if the mean array power production ....is more than one standard deviation larger than the other". However, the use is biased, because the analysis is essentially only one-sided. The authors only compare controlled mean power - 1 standard deviation to the meanpower of the baseline. The authors should also include the standard deviation of the baseline, because it's two overlapping distributions. That essentially means none of the cases are significantly superior if using the authors definition of statistical significance.

Response

The Figure 1 shows the probability distribution of the six turbine array over the control update steps (from LES case ND2). The sum of the six wind turbine array power production is approximately normal as a function of control update steps. Given the sample data deviation from an exact Gaussian distribution, we will use two statistical tests to check the statistical significance of the LES power production data below.

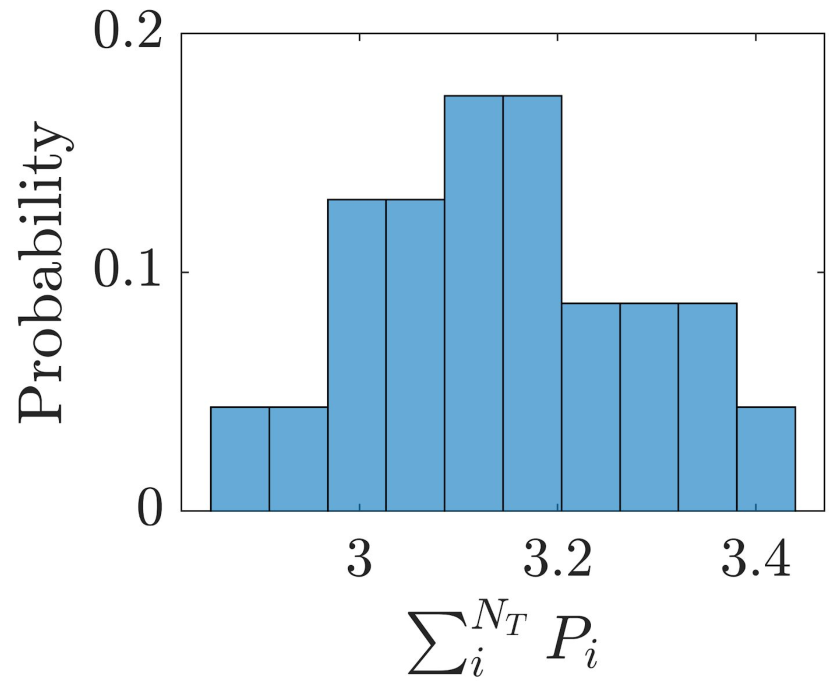

Figure 1: Probability distribution over the control update steps for the sum of the six turbine array power production from LES Case ND2.

While the total turbine array power production is approximately normal as a function of the control update steps, we will demonstrate two statistical tests here to show statistical significance for Cases NA and ND2 by assuming normal distributions or not. The null hypothesis is that the two temporal signals of the sum of the six turbine power production are random samples from the same underlying probability distribution.

Two-sample T-test (assumes normal distributions):
P = 6.13E-4, and therefore the null hypothesis is rejected at a 5% significance level.

Two-sample Kolmogorov-Smirnov test (does not assume normal distributions):
P = 1.16E-4, and therefore the null hypothesis is rejected at a 5% significance level.

Therefore, the differences between Cases NA and ND2 are statistically significant at a 5% significance level. We will update the manuscript with these further statistical tests for all cases.

Comment #7(a)
Zong and Porté-Agel also shows the secondary steering effect, which other recent articles are also investigating, see e.g. King et al., 2020. The secondary steering effect essentially implies that the next wake will also be partly steered for "free"(without power loss). The Liew et al. 2020 reference states that McKay et al. 2013, Bartl et al. 2018, and Hulsman et al.2020, all show that the local inflow direction changes, andthe waked (2nd) turbine should adjust to the inflow. Hence, it would indicate that if the second turbine also yaws 20deg, it actually corresponds to a

larger relative yaw angle. So why would the optimization give the same(or relatively larger) yaw angles for the first 3 turbines?

Response
As demonstrated by the recent studies the referee has discussed, secondary steering is an important aspect of wake steering control. Modeling efforts to capture secondary steering are on-going (King et al. 2020 in review in *WES*) or very recently published (Zong and Porte-Agel (2020), which was published after this manuscript was submitted). The wake model methodology used in this study's simulations did not model secondary steering and therefore this effect will not be directly reflected in the model-optimal yaw angles, although the authors note that the lifting line model with EnKF estimation predicts a generally decreasing trend of yaw misalignment values deeper into the array of turbines (e.g. Figure 8). Future work should examine the efficacy of secondary steering models in closed-loop wake steering control, but is out of the scope of the present study since all LES cases would need to be re-run.

Comment #7(b)
Should the yaw angles not be reduced at least by the secondary effects or is this not capture by the LES?

Response
The yaw angles reported in this manuscript are the local turbine relative yaw misalignment values (i.e. the yaw misalignment of the turbine with respect to its local wind direction measurement), and therefore the secondary steering effect is accounted for in the reporting of the yaw misalignment values from LES but not in the model optimization as discussed in the response to **Comment #7(a)**.

Comment #7(c)
A quick estimate of the power yaw loss with PP = 3, yields the following table (see table in review). That means the first turbine yawing 15-20deg looses approx. 10-17% of the power compared to its own baseline(also shown in Figure 16). Line 480 states "power production penalty...is significant beyond 40deg". The 40deg seems "random", as one could just as well argue that losses of 10-17% are significant. In order for the entire farm to produce more, this loss should be recovered by the next turbine. However, the next 3 turbines experience similar losses. Therefore, increase in velocity have to compensate for all these turbine losses. If I use estimates from Figure 16, I read an initial power production of approx. 35% and an optimal power production of approx. 50% for the second turbine relative to the first. Hard to see from this rough estimate if the second turbine actually gain more than what the first turbine losses. Archer and Vasel-Be-Hagh also concluded there needs to be at least 2 turbines downstream a yawed turbine to regain the power loss.

Response
Thank you for this comment and for your analysis. We will remove Line 480 for clarity.
Figure 16 only plots the power for the first two turbines in the 6 turbine array (to reduce clutter in the Figure). Since it only plots the first two turbines, it should be noted that the gains at turbine 2 do not directly compensate the losses as turbine 1, since the objective is to maximize

the 6 turbine array, not focus on two-turbine sub-optimization problems which reduces global array optimality.
The power production for turbine 2 shown in Figure 16 accounts for the total power, i.e. the power production of turbine 2 accounts for power reduction due to its own yaw misalignment as well as power increases due to turbine 1's yaw misalignment. The largest relative gains in the 6 turbine array will be the turbines further downwind since their relative yaw misalignment is lower (see Figure 14). The total power production for the 6 turbine array is compared to baseline control in Figure 11.
As the referee correctly notes, the conclusion of Archer and Vasel-Be-Hagh (and Howland et al. (2020)) has motivated the use of the 6 turbine array to test closed-loop wake steering in this study rather than a 2 turbine array.

Comment #7(d)
In order to recover the power loss, the wake velocity have to increase by V2/V1 =(0.50/0.35 * CP0/CPyaw)^(1/3) where CP0 corresponds to my previous table of 0degyaw, and CPyaw is the remaining. Hence, the velocity increase(V2/V1) on the second turbine needs to be (see table in review). So a 17-20% increase in mean velocity. It would be nice to see some kind of velocity plots, either contour plots before and after control to verify this. Therefore, it would also be nice to see the actual power production of the individual turbines compared to baseline for the various scenarios, because it's difficult to assess which turbines actually make up for the losses and increase the total production in the accumulated plots, e.g. Figure 9. The authors have attempted to do so in Figure 21, but it's very difficult to distinguish. It seems turbines 2, 4 and 5 recover the losses for PP = 3? Perhaps use bar plots with clearer standard deviations?

Response
We will add further plots, tables, and discussion to clarify the quantitative results in these simulations. To address the referee's specific concern, we will articulate some quantitative results here, and include a thorough discussion in the revised manuscript.
Focusing on Case ND2, the power production for each turbine is normalized by the baseline control power production (of the same turbine) and is averaged over the control update steps. The resulting normalized power production values are:

Turbine	Power (normalized by baseline greedy control and averaged over control steps)
1	0.844
2	1.102
3	1.182
4	1.159
5	1.192
6	1.106

As can be seen from the power production values for Case ND2 (normalized by Case NA), turbine 1 loses approximately 15% of its power production but each downwind turbine generates 10-20% higher power production than baseline control. Since turbine 1 generates larger power, its losses influence the array sum more than the gain of the downwind turbines, but collectively, the average array power production increases 4.6% over greedy baseline control in this case. As can be seen, with more turbines downwind, the potential for wake steering to increase relative power production over greedy control also increases.

Minor comments

Minor comment #1
The inclusion of FLORIS (Section 5.4) seems superfluous. I suggest to leave it out to keep the analysis more focused, and shorter.

Response
The authors do not agree that Section 5.4 is superfluous. This section tests the influence of the state estimation methodology and FLORIS is a commonly used wake model whose parameters have been empirically defined such that model-based parameter estimation is not used. This comparison shows that without model parameter estimation the wake model does not fit the baseline power production, and as such, *predicts* power given a yaw misalignment strategy with 5x larger error (Figure 21(b)).

Minor comment #2
- The definition of 0deg north seems redundant. Where is it used? It seems to be in conflict withFigure 6, as the wind direction with 0deg north would not be 16deg, but a 254deg.

Response
Thank you for this comment, we will modify the manuscript to clarify the terminology.

Minor comment #3
Terminology might change over time and for different researchers. Some use kidneyshaped, others curled wake. However, the vortex pair has been observed in the wake behind a yawed turbine prior to 2016 (line 131). The earliest reference I could find was Mikkelsen, 2004, see Section 8.3.

Response
We will add the reference to Mikkelsen 2004.

Minor comment #4
- Please introduce/explain Table 1 more when it is first presented.

Response
We will modify the manuscript accordingly.

Minor comment #4
Please provide a proper description of the turbine. Line 398: "selections based on NREL 5MW" does not seem reproducible by other scientists.

Response

The reference to the NREL 5 MW turbine was to clarify the selection of the turbine diameter. All relevant computational parameters of the turbine are detailed in Section 3.

Minor comment #5
Details are difficult to see in many of the figures. For instance, reduce the axis limits on Figure 15 and similar. Figure 21 is very hard to distinguish.

Response
Thank you for this comment. We will modify the figures accordingly to improve visibility.

References

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