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Interactive comment on "Optimal closed-loop wake steering, Part 1: Conventionally neutral atmospheric boundary layer conditions" *by* Michael F. Howland et al.

Anonymous Referee #1

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Review comments for "Optimal closed-loop wake steering, Part 1: Conventionally neutral atmospheric boundary layer conditions" by Howland et al. in Wind Energy Science, 2020.

Overall: The article presents a framework for closed-loop wind farm control, particularly focused on wake steering. The framework consists of a lifting line wake model and an ensemble Kalman filter, where the wake model is continuously calibrated to previous results, and used to optimize the wind farm control settings. The framework is tested using LES and the sensitivity of various parameters is examined.

The topic is interesting and important. However, the article is quite long, and certain

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places it seems unfocused and occasionally self-contradictory, or even biased. Sensitivity studies on model parameters are generally very important, but it seems that the application of the wake model itself within the framework is the main drawback here. I recommend major revisions.

General comments: Given that the articles requires major revisions, I will only provide major comments, which are lengthy enough.

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?

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 unrealistic 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 indicate 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.

a) Model Description and Assumptions: - The assumption of thrust following cosˆ2 gamma 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. 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-elastic tool, 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.

- 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.

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

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 advection 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

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references for this.

2. 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 Kheirabadi and 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.

3. 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?

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 wake start 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.

4. 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 mean power 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.

5. Optimal yaw angles The determined optimal yaw angles seem quite large, although others report similar values. In figures 8 and 14 the yaw angles of the first 3-4 turbines are 15-20deg. The following comments concern additional required information, because it is difficult to assess from the article in its current form.

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, and the 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? Should the yaw angles not be reduced at least by the secondary effects or is this not capture by the LES?

b) A quick estimate of the power yaw loss with PP = 3, yields the following table:

yaw CP

0 1.0000

5.0000 0.9886

10.0000 0.9551

15.0000 0.9012

20.0000 0.8298

25.0000 0.7444

30.0000 0.6495

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

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Vasel-Be-Hagh also concluded there needs to be at least 2 turbines downstream a yawed turbine to regain the power loss.

c) In order to recover the power loss, the wake velocity have to increase by V2/V1 = (0.50/0.35 * CP0/CPyaw)ˆ(1/3) where CP0 corresponds to my previous table of 0deg yaw, and CPyaw is the remaining. Hence, the velocity increase(V2/V1) on the second turbine needs to be:

yaw V2/V1

0 1.1262

5.0000 1.1305

10.0000 1.1436

15.0000 1.1660

20.0000 1.1985

25.0000 1.2427

30.0000 1.3005

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?

Shorter comments: - The inclusion of FLORIS (Section 5.4) seems superfluous. I suggest to leave it out to keep the analysis more focused, and shorter. - The definition of 0deg north seems redundant. Where is it used? It seems to be in conflict with Figure 6, as the wind direction with 0deg north would not be 16deg, but a 254deg. - Terminology might change over time and for different researchers. Some use kidney shaped, 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. - Please introduce/explain Table 1 more when it is first presented. - Please provide a proper description of the turbine. Line 398: "selections based on NREL 5MW" does not seem reproducible by other scientists. - 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.

Additional references:

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4. Sørensen, J. N., Mikkelsen, R. F., Henningson, D. S., Ivanell, S., Sarmast, S., and Andersen, S. J., 2015. Simulation of wind turbine wakes using the actuator line technique. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences

5. King, J., Fleming, P., King, R., Martínez-Tossas, L. A., Bay, C. J., Mudafort, R., and Simley, E.: Controls-Oriented Model for Secondary Effects of Wake Steering, Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2020-3, in review, 2020.

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