Author's reply to 'Review of wes-2020-31', Anonymous Referee #2

Thank you very much for your detailed and valuable review of our manuscript. Below you find a copy of the referee's comments together with our responses marked in red.

Overview

"Integrated wind farm layout and control optimization", written by Mads Pedersen and Gunner Larsen is generally well organized and well written. A wind farm layout and control optimization methodology is presented and analyzed. An approach to separating the control and layout optimizations is presented that is seemingly useful, but could use some further validation. The problem formulation is generally well presented, but could benefit from more detailed descriptions to make it easier for researchers to reproduce the results. There are a number of typographical and grammatical corrections that would improve the quality of the work, though none of them are extremely drastic. Finally, more discussion of the flow-field assumptions made may significantly reduce any doubt of the methods from the community.

High-level Comments

The use of two "example" studies to provide some intuition on the optimizing functions is a good way to tell the story, but the second example could provide more. By considering a single row of WTs, you are effectively removing a degree of freedom for the optimization solver to handle. It would be nice to see whether the AEP differences between the sequential and nested optimizations are still small for a minimal working example that doesn't remove design variables, but just has a few turbines and an appropriately constrained space. This would help to remove any doubt that the sequential approach is sound.

We agree that a minimal example with two location degrees of freedom pr. WT would remove some doubt. To remove any doubt, however, the full example is required as the effects of both optimal layout and optimal derating is highly dependent on the number of turbines and the admissible site area and geometry. We have tried to optimize a minimal wind farm with 3x3 WTs, but the optimizer did not manage to find a trustworthy solution. We are therefore hesitating about adding such an example.

There is no mention or discussion of the wake model. This would be good to know more about, given that the proposed control method is axial induction control. Additionally, there is brief mention of the existence of wind shear and turbulence – details on this would be useful.

Rotors and wakes are modeled as actuator discs in a linear CFD framework - which in the paper is formulated as "The WTs are modelled as actuator discs, which in general can be vertically inhomogeneous, but often is assumed uniform". We will specify that this model is used to model the drag force, which in turn generates the wakes. The actuator disc's are modeled using the aerodynamic model described in detail in Section 2.2.

The effect of the ambient mean wind shear and turbulence characteristics on the wakes are specified in terms of a terrain roughness height, which in turn implicitly dictates the ambient turbulence conditions via the turbulence closure of the CFD model as based on Monin–Obukhov theory for neutral

atmospheric stratification, which is assumed. We have included the applied roughness height and the turbulence intensity it dictates at WT hub height in the revised manuscript.

Especially because the optimized Lillgrund WPP has turbines that are so close together, some discussion of the validity of the linear CFD solver's ability to accurately represent the near wake region of upstream turbines would be very helpful. This goes hand in hand with the need for a discussion on the wake model.

The linear CFD model does not rely on the assumption that the wind speed in the wake is U(1-2a) as e.g. the classical N.O. Jensen wake model. In principle, the wake model is therefore also valid in the near wake. The uniformly loaded actuator disc formulation, however, implies simplifications visible in the near wake region (no azimuthal or radial force variations etc.). Some distance downstream the effect from such variations vanishes, and we expect the model prediction to be acceptable 2D downstream.

In the very beginning you note that "The purpose of this paper is to investigate the influence of optimal wind farm control on the wind farm layout". To me, the word influence feels misleading. If I am not mistaken, this work really is investigating the joint optimization of the layout and control, not specifically the influence of one on the other. It may be worth it to consider re-phrasing this, or restructuring the paper a bit make it clear that the influence is in fact being investigated.

You are right. We will change this formulation.

You briefly mention that the only location constraint is a minimum of 2D from the nearest WT and the wind farm boundaries. What motivated this distance? This (and any other constraints that exist) might better fit in the problem formulation, not in the conclusion.

The minimum-spacing constraint is applied to avoid the turbines to be placed unrealistically close together. The minimum distance of 2D is chosen because it is around the minimum distance we have seen in a real wind farm.

We will list the applied constraints in the problem formulation where it belongs.

The introduction offers a fairly good review of the relevant literature but could benefit from some revision and restructuring. There are a lot of sentences that are extended through a series of commas, semicolons, and dashes and can feel tedious.

We will review the introduction and try to make it easier to read

The presentation of the use of a "detailed aero-servo-elastic model" for the optimization approach (P4.L26) is a bit of a stretch. It seems that the optimization itself uses simplified models (so-called "surrogates"), that are rooted in steady-state BEM solvers, but complete aero-elastic analysis is not done for the optimization. There certainly does not seem to be any dynamic "servo models", just the assumption that the employed individual WT controller is capable of perfectly tracking the derated Cp/Ct.

We are using the aero-servo-elastic tool HAWCStab2 with a detailed aero-elastic model of the turbine (i.e. no dynamic servos in the model) to establish a surrogate relationship between power and thrust. We will clarify this in the revised manuscript.

More detailed comments & formatting

Thank you very much for these comments, corrections and suggestions. We will replace and rephrase as suggested.

In my opinion, the use of italics to emphasize words is over-used. Sometimes it is useful, but caused a little confusion for me at times as well.

There are a lot of leading and trailing hyphens that are unnecessary throughout.

P1.L10 – Should be clear that you are focused on controlling the turbines for wind-farm wide AEP maximization, not just doing standard wind turbine control. “A priori” is unnecessary in this sentence.

P1.L20 – “… the a Swedish offshore wind farm …”

P1.L19 – “…the capability ies of the developed …”. Double check singular/plural adjectives throughout paper.

P1.L30 – “capital costs that depends on the WPP layout”

P2.L10 – “A priory” -> “A priori”

P3.L2 – modal or model?

P3.L10 – “tip speed ration”

P3.L22 – I’m not sure I see how this work is specifically “guided by” the statement in 2). Confusingly, you state that , 2) suggested that more “realistic” studies introduce a lot more uncertainty, and then you say that you are attempting to get more “realistic” results. I am admittedly not very familiar with the work in (Kheirabadi and Nagamune, 2019), so perhaps I am missing something here.

P3.L23 – “CDF” -> “CFD”

P3.L32 – “CDF” -> “CFD”

P4.L16 – “justifies”

P5.L23 – “relies on an extended”

P7.L4 – “(3, 10, and 15 m s^-1 are marked). The marking of the rotor speed limits is probably unnecessary and clutters the figure.

P7.L17 – “this is assured to the highest possible degree” – what do you mean by this?

P7.L17 – “implementation of the shortcuts”

P8.L18 – what do you mean by “both of the above … will eventually lead to the same result”? Only one of the optimization approaches is sketched “above”

P8.L1 – “.. there are three common ways…”

P9.L10 – “A few local optima” is vague. This statement should be elaborated upon and/or justified more.

Figure 4 – make sure the figure caption is on the same page as the figure

Figure 5 – what do the black up/down arrows represent?

Figure 6 – Having the axis derating percentages range from 0%-100%, but the power percentage range from 40% - 100% in the colorbar is confusing

Figures 8 and 9 – The titles on these two figures should probably be the same, or similar. At least the y-axis label, and perhaps the titles, should reflect the fact that the percentage value plotted is percent of relative power.

P14.L7 – “considerablye”

P14.L11 – case (4), a the (see major comments about my concerns with the lack of 2-D dimensionality in this second sanity check)

Table 3 – caption should be on same pages as table

P17.L10 – “Inherit” –> “inherent”

P17.L17 – “Introductory”, This sentence is generally very confusing, and I am not sure I see the “clearly exaggeration