Author's reply to 'Review of wes-2020-31' by Anonymous Referee #1

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.

Review of wes-2020-31

Overview

The manuscript, "Integrated wind farm layout and control optimization" submitted by Mads M. Pedersen and Gunner Chr. Larsen offers an analysis on wind plant layout and control optimization, finding that the two procedures may be treated separately without significant reductions to the benefits to AEP. The work decoupling the numerical operations offers some real potential to engineering processes for wind plant design and operation. However, the results lack generality and insufficient detail is provided on the means by which results are attained. The manuscript would be greatly strengthened by a discussion of whether the control and layout optimization steps can always be safely decoupled. This would help to simplify wind plant optimization in general, which is an NP-hard problem. In summary, the manuscript offers some results that have a potential benefit to the wind energy research community and industry, but more information is required before results can be confidently and generally reproduced.

It is correct, that it would greatly strengthen the generality of the manuscript if we could 'prove' that control and layout optimization can always be safely decoupled. This, however, implies that all possible generic integrated topology and wind farm control problems have this property - i.e. an arbitrary number of WTs, related arbitrary area constraints (i.e. WT 'density'), area shapes, wind climates etc. This is, unfortunately, not possible for this complex optimization problem, where an analytical solution providing such results are not possible.
This is also stated in the paper referring to the conclusion of Fathy et al., (2001): "A priory, it is not possible to evaluate whether the coupling between system design variables and system control variables is weak or strong for a complicated physical system like a WPP".

Major Comments

Wind plant control and operation by derating (i.e. axial induction control) is understood to be strongly dependent on the nature of the wake model used in wind plant performance modeling. No detail is provided on how wakes are modeled in the current work. Specifically, many well-known velocity deficit and wake-added turbulence models exist and provide different estimates on the benefits of pitch-based derating.

Rotors 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 add that this model is used to model the wakes.
We are not modelling wake-added turbulence, as we are not considering WT loading but only WT production.

The optimization procedure is discussed only briefly and will probably not be well understood by researchers that are not already familiar with the methods. Consequently, reproduction or verification of the results will be virtually impossible by other groups.

We have added more details on the optimization procedure in the revised paper.

Finally, the layout optimization produces some results that do not seem to be appropriately constrained for implementation in reality, and may lead to overestimating the AEP gains. Turbines in the modeled wind plant appear to have been placed closer together than would be allowed in reality and in some cases are certainly operating in the near wake region of upstream turbines. Constraints or bounds on optimization parameters are not sufficiently clear in the manuscript.

The optimization is constrained by a minimum allowable distance to the nearest neighboring WT of 2D together with the convex boundary around the initial WPP layout. We will write this explicitly in section 2.4.

Page 1: Authors state that the procedure outlined in the manuscript is, “the fastest possible optimization procedure”. How is this determined? Can it actually be shown to be the fastest, or is it simply faster than a given alternative?

The point is that de-composed nested approach is much faster that the fully integrated optimization approach. We will reformulate this phrase.

Page 1: Increasing AEP for any wind plant by 4% is a very substantial change. Is this estimate derived by comparing a modeled baseline production to a modeled controlled case, or is power production reported by the SCADA system used at all. There is no discussion of the effects of turbulence on wind turbine wakes or wind plant performance anywhere in the paper. This is a crucial consideration for wind turbine wake interaction and mixing and plays a huge role in the outputs of wake models used for power estimation. How is turbulence considered in the modeling, optimization, or estimation of AEP?

Yes, 4% is a substantial change, but of the same order of magnitude as reported in other papers using different approaches. This result, however, is based on the assumption that the inflow to the wind farm is homogeneous, stationary and well-known. Furthermore, controller-specific constraints such as tower-exclusion zone and smooth transition between regions are not considered. We will state this in the manuscript.

The 4% increase is relative to the model base line to obtain a consistent comparison. Comparing to SCADA data production would not make sense as model uncertainty then enter the 'equation'.

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. We have included the applied roughness height and the turbulence intensity it dictates in the revised manuscript.

Page 5: “divergence free,” isn't this another way of stating “conservation of mass” for incompressible flows?

Yes. We will add this.

Page 5: “linearity of the model, wakes from multiple upstream WTs can consistently be superimposed to construct the flow field further downstream”. How well do the authors expect this to reflect reality? From the governing equations, wake velocity deficits are highly non-linear. Other wake models use sum-of-squares superposition or maximum deficit approach. Why is a linear model assumed?

We use the linear sum because it is consistent with the linear perturbation expansion of the Navier-Stokes equations in Fuga. We expect it to be at least as good as conventional empirical engineering models for wake summation. Full-scale validation studies of Fuga shows good agreement between model predictions and reality. We will add references to these studies

Equations (2) and (3): These relationships are derived through model outputs of HAWCStab2 by artificially limiting the power coefficient for a fixed wind speed. Am I correct in reading that there is no anlytical or empirical relationship to describe the modified values of Cp and Ct? Is this why the authors use look-up tables?

Yes, it is correct. Based on outputs from HAWCStab2 (numerical simulations of the rotor aerodynamics based on rotor/blade aerodynamic characteristics and using the BEM (Blade Element Momentum) approach) for a range of rotor speeds and pitch angles, the relationship between Cp and Ct is found by finding the rotor speed and pitch angle that, for each value of Cp, result in the lowest possible Ct. This relation is stored in look-up tables.

Page 6: “The results shown in Figure 1 can be used for the entire range of mean wind speeds requested for the system optimization,” Cp and Ct are both functions of wind speed as well. What makes the authors confident that that the results in Fig. 1 are applicable at wind speeds other than 8 m/s, where they were defined? Equations (2) and (3) clearly state that Cp and Ct are functions of the mean wind speed, conditioned on tip-speed ratio and blade pitch angle.

Thrust scales with U^2 and the thrust coefficient is normalized with U^2. Power scales with U^3 and the power coefficient is normalized with U^3. These relations, however, assumes that the deformation of the blades are not wind-speed dependent. In reality the static blade deformation depends on the wind speed – that is why Ct and Cp depends on U in equations (2) and (3). In this study we simplify the model by using the static rotor deformation corresponding to 8 m/s for all wind speeds, and we do not expect the optimization result to be significantly affected by this simplification. We will add this information to the manuscript. For a more general formulation (also mentioned in the paper as a potential possibility when describing the rotor aerodynamics using HAWC2Stab), where the dependence of the static rotor/tower deflection with mean wind speed is taken into account, the Ct and Cp results will have to be computed for each relevant mean wind speed.

Equation (5): Define limits of integration, wind direction is not typically discussed in units of radians. Is a resolution of 1 m/s for the numerical integration sufficient to capture rapid changes in region 2?

We will replace radians with degrees.
The plot below shows the change in AEP when increasing the wind speed resolution from 0.1 m/s to 2 m/s. It is seen that the “error” in AEP for a wind speed resolution of 1 m/s is less than 0.15%. We consider this accuracy sufficient, and we expect the error to be much lower when comparing two cases,

which are both calculated with a resolution of 1 m/s.

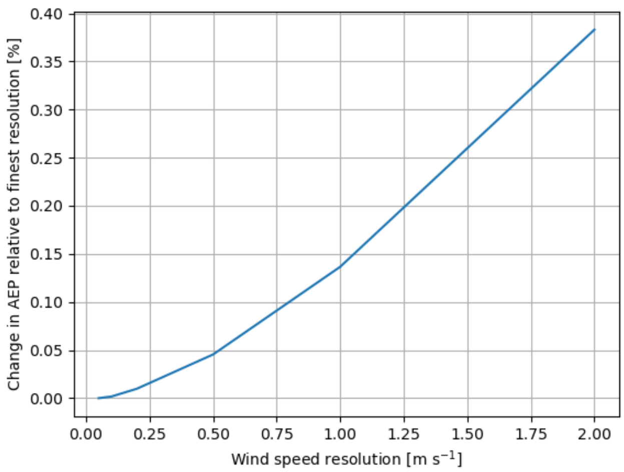


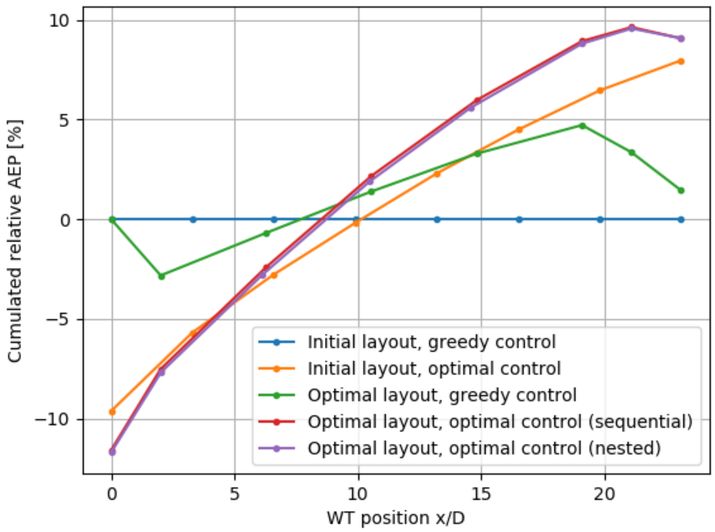
Figure 6, is the power production of each turbine relative to the nominal production at 8 m/s or is it relative to a different baseline value? Also, is the total power in Figure 7 just a sum of the plots in Figure 6?

Yes, both are correct, except that the power production refers to a mean wind speed equal to 10 m/s. We will clarify this in the manuscript.

Table 2: “Optimized Greedy 41.44 (+1.4%)” Results from 'sanity check' study indicate that optimal layout does not differ greatly from uniform spacing. Can the authors comment on the changes see in layout for case (2)? It also might be helpful to indicate computation time for each case, since that seems to be the justification for decoupling the layout and control optimizations.

The degree of freedom of the “sanity check”-example is limited to movement of the middle turbine. The result depends on the trends of of the power and thrust curves as well as the wind speed distribution. With the input used in this study, the optimal position is very close to the initial layout (equal spacing), and therefore only an infinitesimal increase is optained.

In the 8-WT example, the optimizer has the freedom to move all turbines, and in this case the optimal layout is different from the initial layout. The plot below shows the cumulated AEP of the 8 WTs relative to the base line (i.e. initial layout and greedy control) as well as the position of the WTs (indicated by the dot markers). The green curve shows the optimal layout with greedy control (Case 2). It is seen that the distance between the two most upstream and the tree most downstream WTs are smaller than in the base case. This allows larger spacing and thereby production of the middle turbines, which, in this case, results in an increase of the AEP of the whole row. Obviuosly, this strategy is not possible with only three turbines.



The computation time is indeed an argument for the decoupling. Otherwise, the problem solution for a WF of the Lillgrund size becomes a major computational challenge - even on a big cluster. The computation times for the 8-WT row are listed below and will be added to table 2 of the paper.

Case	Layout	Control	AEP [Gwh]	CPU time* [s]
0	Initial	Greedy	40.85	0.01
1	Initial	Optimized	44.10 (+8.0%)	4.20
2	Optimized	Greedy	41.44 (+1.4%)	2.84
3	Optimized	Optimized (sequential)	44.558 (+9.1%)	6.92
4	Optimized	Optimized (nested)	44.560 (+9.1%)	3731

* On standard laptop PC

Figure 13: Note case(1) on the left and case(3) on the right. Also case(3) results do not seem intuitively correct. There appear to be areas where a wind turbine could be placed within the central area of the wind plant that would reduce the need to derate to the same extreme of 19%. How closely spaced are the wind turbines on the western and southeastern edges? Given that Lillgrund is already a tightly packed wind plant, these may be dangerously close or impractical. Are any bounds provided for wind turbine spacing? Is the same optimal layout used for all wind directions, or is layout different for each case?

Yes, we are using the same layout for all wind directions (as you cannot move WT when the wind direction changes), while the WT control settings varies with the wind direction.
The layout shown in Figure 13 is found to result in the highest overall AEP given the site conditions (wind speed distributions and wind direction frequency) used in this study. The inflow situation in Figure 13 reflects 10m/s from 223 deg, only, and for this particular flow case, the layout is obviously not optimal.
The optimizer places the WT very close on the western and south-eastern edges, but does not violate the 2D spacing constraint (indicated by the dashed circles)

Minor Comments

Throughout the manuscript:

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

The naming convention of 'topology' is somewhat confusing. Suggest a change to WPP layout and WPP control, for clarity, as in the tables.

Commas and nested clauses are used with excess. For simplicity and readability, consider rephrasing with simpler, more direct messaging.

Phases are needlessly italicized throughout the text. Please remove text emphasis unless absolutely necessary.

Page 1: rephrase “in-stationary” as non-stationary or transient

Page 2: “A priory” -> a priori Page 2: rephrase, “and if so then how”

Page 3: rephrase as statement of research challenge rather than as a question. “Is it possible to conduct WPP system optimization based on a full-blown CDF simulation of the complex WPP flow field with its complicated WT wakes interactions?”

Page 4: “wind direction and -speed” -> “wind direction and wind speed”

Page 4: rephrase, “that is possible for at the requested”

Page 5: Rephrase “but often is assumed uniform” as “but is often assumed to be uniform in practice” or similar.

Figure 2: cut in wind speed for a SWT2.3-93 is 3.5 m/s

According to Siemens, there is no well-defined cut-in wind speed for the wind turbine used in the paper. In this study, we calculate the AEP based on power calculations for the wind speeds, 3, 4, ... , 25. In any case, the impact of the lowest wind speeds on the AEP are minimal and, in our opinion, insignificant for a layout study.

Page 8: “i.e. the WT position coordinates”, are the coordinates (lat, long) the two design variables or is there another?

Yes, the two WT position design variables are the Cartesian x and y coordinates of the WT.

Page 8: “Both of the above-sketched optimization”, Only a single optimization strategy has been show so far.

In the first strategy, all N(2+Nd Ns) design variables are optimized in one process, which is infeasible within the current frame work. The second strategy is the two-step nested approach. We will try to make this more clear in the manuscript.

Figure 4: caption should be on the same page as figure. Also consider making Figs 3 and 4 subfigures.

Use the \citet{} command for textual citations throughout the manuscript.

Page 9: How is the global optimum identified? In other words, how are the authors sure that the solution represents a global solution rather than a local optimum?

We cannot be sure that the presented solution is a global optimum, but we are confident that the result is close to optimal as we ran several long-run instances of the random search without getting a better result.

Table 1, insert comma after "layout"

Page 10: "both WT1 and WT2 is" -> "both WT1 and WT2 are"

Page 11: "the Cp- and the Ct dependence" -> "the dependence of Cp and the Ct on wind speed"

Figures 8 and 9: update vertical axis label. Simply writing "%" does not indicate what relative value is being considered.

Page 14: considerable -> considerably

Page 14: only insignificantly -> not significantly

Table 3: "4 Optimized Optimized(nested) -", remove from table if not pursued in analysis. Caption should be on same page as table

Page 15: analogue -> analogous

Page 15 "more than doubled compared to the" -> "more than double that of"

Page 17: "Introductory," seems out of place.