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.

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.

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.

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.

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?

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?

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

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?

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?

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.

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?

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?

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.

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?

Minor Comments

Throughout the manuscript:
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

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

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

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?

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.