Wind Energ. Sci. Discuss., author comment AC3
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Reply on RC2

Jared J. Thomas et al.

Author comment on "Gradient-Based Wind Farm Layout Optimization Results Compared with Large-Eddy Simulations" by Jared J. Thomas et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-4-AC3, 2022

Referee 2 Comments and Author Response

Note: The text in quotation marks is from the referee. The indented bullet points are the author response.

Overview

"The authors present a study on gradient based wind-farm layout optimization and comparison of the optimized results with LES. An important novelty claimed by the authors is the fact that optimized layouts are compared to 'high-fidelity' LES over a full wind rose. Unfortunately, there is potentially a major flaw in the presented LES results (see point 1 below). Therefore I do not believe this work can be accepted for publication in its current form"

- Thank you for your comments. Your insights helped us improve the work, including corrections and additional studies. We believe it is now ready for acceptance for publication.

Detailed comments

- "LES are performed on a domain of 5x5x1 km. This domain is much too small, and given the forced inflow conditions, will lead to domain blockage and an artificial favorable pressure gradient. This is essentially a direct result from Newtons second law when domain boundaries do not allow the momentum of the flow to freely change. When optimizing the layout, and thus changing to total wind-farm thrust, this will then also lead to a larger favorable pressure gradient, hence artificially enhancing the benefits of the layout in the LES. This seems to be exactly what the authors observe when comparing their LES with the optimized wake models. Also the fact that gains that are calculated using the front row turbine's velocities as reference (as in Fig 13) are closer to the wake models than direct power (which is based on inflow reference) points to significant blockage effects. The authors should show in a revised manuscript that their selected domain size is sufficiently big to avoid blockage effects that are of the same order of magnitude as optimization gains. To this end, they should for a selected case show results on different domain sizes, showing that effects become negligible for the final selected size. It is my expectation that that size is considerably bigger than 5x5x1 km. Subsequently, all simulations should be performed on that domain size."
  - These are excellent points. We started work on a study similar to what was outlined above. However, the theoretical domain size required to avoid blockage effects for enough cases required more computational resources than we had at our disposal for this project. We used the NREL Eagle super computer, which has 36 core nodes.The 5 km-by-5 km domain had a total CPU time per simulation of approximately 756 days, or 36 hours on fourteen 36-core nodes. This study required 48 simulations (one for each direction, for each TI level, for both the baseline and the optimized layouts), for a total CPU time of about 72 days on fourteen 36-core nodes. We estimate that to reduce the artificial speedup effects sufficiently would require a domain size of approximately 15 km-by-15 km. However, even just increasing the domain to 10 km-by-10 km would require approximately 6 days on fourteen 36-core nodes per simulation, or around 288 days on fourteen 36-core nodes to complete all the simulations for all directions, which is more than the computational resources we had available for this project.
  - We have included a figure demonstrating the estimated directional blockages and a discussion of the possible implications of having too small of a domain size. We also adjusted some of the results based on a simple estimate of the speedup due to added blockage from the base case to the optimized layout. However, because the difference in estimated blockage between the base layout and the optimized layouts is small (~1 percentage point on average), and the optimization algorithm was unable to exploit blockage effects because it was unaware of the LES, we believe that any speedup effects present due to the blockage do not change the primary conclusions of this paper, namely that the optimized layouts found using the simplified engineering model are actually good layouts.
  - See lines 425-465 in the revised manuscript excerpts attached
  - See lines 482-519 in the revised manuscript excerpts attached
  - See Fig. 13 in the revised manuscript excerpts attached
  - See Fig. 14 in the revised manuscript excerpts attached
  - See Fig. 15 in the revised manuscript excerpts attached
- "Overall, the discussion on the LES set-up in 2.4 and 2.5 is too brief. Based on this, the set-up is simply not reproducible. The authors mention buoyancy and Coriolis effects, but it is my impression that the simulations may simply consist of a pressure driven boundary layer. If not, what is the geostrophic wind, what is the stratification profile. In addition, what is the surface roughness, friction velocity, etc. What is the precursor set-up (domain size, grid size, initialization, spin-up time, etc). What is the simulation cost, …"
  - As suggested, we have included much more detail in 2.4 regarding the LES set up.
  - See section 2.4 in the revised manuscript excerpts attached

Smaller comments

- "Line 84 and also later section 2.1: better justify why the near-wake region can not be simply avoided by using a minimum distance constraint in the optimization, e.g. using a constraint that is larger than x_d obtained Eq.8. In case of interest in set-ups where turbines are placed more closely together, the near wake model may need improvement anyway, and the heuristic adaptation proposed may not suffice."
  - A constraint on the minimum turbine distance is included, but the size of the near wake region is variable and the turbine separation constraints are non-linear. SNOPT minimizes infeasibility for non-linear constraints, so infeasible solutions may be attempted during the optimization and the near wake model may be used at some point during the optimizaiton.
  - We have added clarification around
    - Lines 85-90 in the attached revised manuscript excerpts
    - Lines 151-171 in the attached revised manuscript excerpts
- "Throughout the paper: equations are part of the text, and phrases and punctuations should be used accordingly. Please check with other papers and publication standards to see how it is done"
  - We have made this change throughout the manuscript. Thank you for the suggestion.
- "Line 143: "To remove the discontinuity" --> please provide a mathematical expression"
  - Good suggestion. We have added clarification
  - See lines 154-161 in the attached revised manuscript excerpts
  - See Eqs. 9 and 10 in the attached revised manuscript excerpts
- "Line 152: If greater accuracy is desired --> speculative. Either provide data that prove this statement or remove"
  - We have adjusted this statement slightly to clarify that we are not making a claim, but rather providing a suggestion and reference for readers interested in a more accurate near-wake model.
  - See lines 162-171 in the attached revised manuscript excerpts
- "Line 212: sunflower pattern: please provide reference or formula"
  - Thank you for the suggestion. The formulas, along with citations and explanation, have been added.
  - See section 2.2.3 in the attached revised manuscript excerpts
- "Eq 19: provide units. Result is in kWh and not in J."
  - Thank you for the suggestion, we have clarified the units.
  - See Eq. 23 and related discussion in the attached revised manuscript excerpts
- "Eq 21: this is not a correct definition of TI. TI is based on magnitude of fluctuating velocity that includes components in all directions"
  - We used this definition as an estimate of the turbulence intensity. Our simulations did not include significant lateral or vertical flow components and so we included only the variations in the primary flow direction. We have added some clarification.
  - See lines 310-319 in the attached revised manuscript excerpts
- "Eq 22: to be technically precise, the 'i=1…38' should be added in the subscript below 'maximize'"
  - Thank you for pointing this out. To make things cleaner we have just removed the underset (x_i,y_i) because they are redundant with the shown objective inputs.
  - See Eq. 26 in the attached revised manuscript excerpts
- "Line 300: forward differentiation is used. What is the advantage of this over using SNOPT without providing the gradients explicitly? I do not believe that this will be significant, since in that case SNOPT constructs gradients based on FD? This can be even less expensive than forward differentiation, and accuracy loss is often not significant (depending on implementation choices). Please discuss in more detail the gains etc. Usually, significant speed-up would only follow from backward differentiation.

This should be better substantiated in the manuscript, in particular since, in the abstract, you seem to claim this as an important innovation."

- Thank you for bringing this up. Our explanation was not clear enough. ForwardDiff.jl does not use the forward finite difference method, but rather forward mode algorithmic differentiation (AD). We have added more detail in section 2.7 to clarify this point. AD has been shown to be much less computationally costly and more accurate than finite difference methods, such as the one provided in SNOPT.
- See lines 340-347 in the attached revised manuscript excerpts

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  - "Line 305: please discuss the WEC method in more detail."
    - Good suggestion, we have added more detail about the WEC method in section 2.7.
    - See lines 348-359 in the attached revised manuscript excerpts
  - "Also better explain why standard multistart methods do not work? If they are as good, why not use a standard from an optimization library"
    - WEC is a continuation optimization method, not a multistart method. However, using it with a multistart method is recommended. We have provided an explanation of, and motivation for, our multi-start approach in section 2.7
    - See lines 365-368 in the attached revised manuscript excerpts
- "Line 314: what do you mean with 400 optimizations? This is confusing. If I'm not mistaken, you solve Eq 22 only twice. Better clarify/make distinction"
  - This should be more clear with our additions made to section 2.7. We ran 400 optimizations for each case (high TI and low TI). The final optimized result shown is the best of the 400. Each optimization used a different starting layout, but all were compared to the single base case provided in the paper. Multistart is a common approach for wind farm layout optimization.
  - See line 365 (and all of 2.7) in the attached revised manuscript excerpts
- "Figures in general: please make as much as possible black&white friendly (some plots are not readable when printed in gray scale). For many figures this should be possible without losing attractiveness of the figure in color."
  - While we recognize that the color scheme may not be ideal for printing, we would like to leave it as is. The colors were selected for colorblind readers, as requested in the WES submission guidelines. Since most readers will be seeing the article online, we believe that having a colorblind-friendly palet is more important than having a printer friendly one. That said, we did adjust some figures to be more clear in black and white using marker styles instead of color.
  - See Figs. 14 and 15 in the attached revised manuscript excerpts
- "Wind directions throughout paper: add degree symbol"
  - Thank you for noting the inconsistency. We have added the degree symbol in the appropriate locations throughout the paper.

Please also note the supplement to this comment:
https://wes.copernicus.org/preprints/wes-2022-4/wes-2022-4-AC3-supplement.pdf