Wind Energ. Sci. Discuss., referee comment RC2
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Comment on wes-2022-4

Anonymous Referee #2

Referee 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-RC2, 2022

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

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.

- 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, …

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.
- 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
- Line 143: "To remove the discontinuity" --> please provide a mathematical expression
- Line 152: If greater accuracy is desired --> speculative. Either provide data that prove this statement or remove
- Line 212: sunflower pattern: please provide reference or formula
- Eq 19: provide units. Result is in kWh and not in J.
- Eq 21: this is not a correct definition of TI. TI is based on magnitude of fluctuating velocity that includes components in all directions
- Eq 22: to be technically precise, the "i=1…38" should be added in the subscript below "maximize"
- 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.
- Line 305: please discuss the WEC method in more detail. Also better explain why standard multistart methods do not work? If they are as good, why not use a standard from an optimization library

- 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
- 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.
- Wind directions throughout paper: add degree symbol