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Interactive comment on "Adjoint Optimization of Wind Plant Layouts" *by* Ryan N. King et al.

Anonymous Referee #3

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The authors present work on wind-farm layout optimization using RANS and gradient-based optimization using adjoints for the determination of the gradients. The work is interesting and merits publications. Nevertheless, I have a number of (relatively minor) comments that should be addressed first.

1. Page 5, Eq 1: formally the state u should be added under the min, so $\min_{m,u}$ (in this formulation, optimization is done over state and control, given the state and other constraints]

2. page 6: the formalism for the derivation of the adjoint should be cleaned up a bit. First of all, there is a distinction between $J(u, m)$ and $\hat{J}(m) \equiv J(u(m), m)$, where $u(m)$ is defined by $F(u(m), m) \equiv 0$. The authors want to derive the gradient of $\hat{J}(m)$ (to m), not the gradient of J to m, which is simply $\partial J/\partial m$. In fact, the authors do not solve (1)-(4) directly, but rather a reduced formulation, i.e. $\min_m \hat{J}(m)$ (s.t. to all constraints except the state constraint, which is now already implicitly containted in $\hat{J}(m)$). This is

a quite common approach in PDE-constrained optimization (cf. e.g. the book by Borzi and Schultz, Computational Optimization of Systems Governed by Partial Differential Equations). Please clarify your notations accordingly in the manuscript

3. page 7: optimization problem should also include the boundary conditions + explicitly express that the turbine forces are function of m. Also similar to above, this formulation requires $\min_{m,u}$

4. page 9: I find it very weird to talk about a smoothed thrust coefficient. This goes really against the normal definition of a thrust coefficient, which is a scalar value. Instead, please use the convention that the force is smoothed out over the RANS grid using a geometric smoothing function. This is the standard convention used in all Actuator Disk representations in literature (both RANS and LES – check any of the relevant papers)

5. page 10: given the domain size (height and width), please provide and discus the blockage ratio

6. page 12 , point 6: what optimization method is used?

7. page 14, line 5. Please discuss in more detail what optimization method is used to solve the QP problems: Newton, quasi-Newton, what precise method (truncated, BFGS, thrust-region, ...)

8. page 14, and results section: you claim the use SQP, but then in the results section, you seem to mention that you do not include the distance constraint. What is the point then in using SQP? Please elaborate. Why did the distance constraint not work? And if not included, why not use a simple box-constrained quasi-Newton method?

9. page 15, figure 4 and discussion: Reference data (experiments or LES) should be added to the plot (in particular to Fig 4b and c). The porous disk is well documented experimentally as well as numerically (and has recently also been used in an inter-comparison study Lignarolo 2016) – without adding reference data, the later statement

'Overall, the results presented here compare favorably to results reported elsewhere' is not verifiable. Given that the turbulence model used is really simple, there might be some differences with profiles from literature (which in itself is not a problem given that the authors develop a new approach) – this has to be properly discussed

10. Section 4.2: It would be interesting to also add a single wind direction + discussion. Even though such a case may not occur in reality, I believe it can yield extra theoretical insight

11. Section 4.2: please provide relative efficiencies of the optimized layouts (compared to all inflow turbines).

12. Section 4.4: Simulations over different wind speeds should not be necessary (since, if I'm not mistaken, you work with a constant thrust coefficient per turbine, so you perform optimization for region II). Your simulations when normalized with wind speed, are independent of wind speed, so relative power doesn't change. Therefore, per wind direction, only wind-speed is necessary. Power will then just scale with cube of wind speed. Please reconsider approach in section 4.4 accordingly

13. page 20 (and other locations earlier): the effect of flow-curvature is maybe a bit too much emphasized. It is certainly true that the current RANS approach allows flow curvature (e.g. not present in the Jensen model), but the authors do not substantiate the claim that flow curvature is an essential feature for optimal layout (i.e. an effect that makes a significant difference). Either substantiate (eg. Based on detailed comparisons with other models that do not have the effect) or tune down the statement.

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