Response to Reviewer #1

We appreciate the constructive and insightful comments from the reviewer. Detailed replies to each of the reviewer's points (in blue italics) are provided below. Given the opportunity, we feel that incorporating the reviewer's comments will produce a substantially clearer and stronger paper as a result.

general comments:

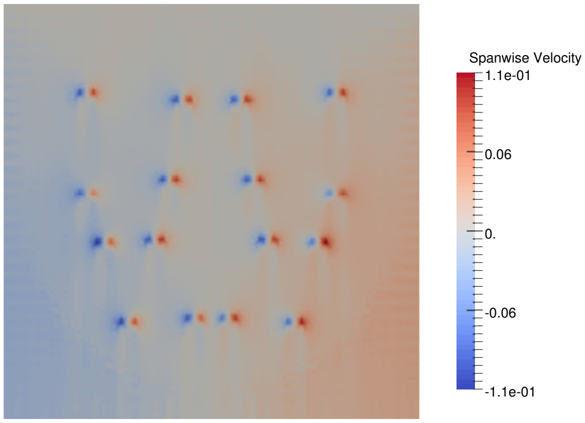
The paper handles the optimization of wind farm layouts using the discrete adjoint approach in CFD (solved via an FEM package in contrast to FVM codes). Instead of LES or 2D RANS, the paper uses 3D RANS in order to achieve a faster, but still reliable optimized layout, which is based on a high-fidelity model (in contrast to analytical wake models). The design parameters are the turbine positions (in contrast to control variables for fixed turbine positions) and the objective function is maximum power and AEP. The paper is very well written. The conception and scope are clear and the topic is of interest.

major specific comments:

section 4.2:

An intuitive guess for an optimized layout for Fig. 6(a) could be a staggered layout with two rows orthogonal to the wind direction. You say that this results from nonlinear effects, speedup and flow curvature, but it is still surprising, since the wakes are more or less straight. Could there be any numerical issues in the implementation, which leads to this final layout (e.g. too high/low tolerances of the optimizer, negligence of the turbulence model in the discrete adjoints)? Did you run a simulation with a staggered grid from the naïve guess? And is it really worse than the final positioning from the optimization?

> In response to this comment and suggestions by other reviewers, we will include a comparison of the optimization results to a baseline case with turbines arranged in two rows orthogonal to the wind direction. We also plan to better demonstrate the curvature effects by including a figure with spanwise velocity, such as the figure below showing the spanwise velocity for the test case in Figure 6d. The adjoint equations are discretized in the same manner as the forward problem and have the same solution accuracy. There is no frozen turbulence assumption in our formulation.



minor specific comments:

title:

The title uses the phrase "Adjoint Optimization", although the optimization is gradient-based and the gradients are computed using the adjoint approach (although other authors use a similar phrase, there is no adjoint optimization, since there are adjoints to the optimization are computed). I propose to use a title which considers this difference (e.g. "Optimization of Wind Plant Layouts using the Adjoint Approach").

> We appreciate the reviewer's nuanced terminology. We will change the title to "Optimization of Wind Plant Layouts Using an Adjoint Approach."

abstract/section 1:

It should be noted here that you use the discrete adjoint approach.

> We discuss the discrete nature of our adjoint solutions in Section 3.5, and will add a mention of this at the beginning of the paper.

section 1 et seqq:

It should be mentioned more often that the used RANS flow is steady-state (in contrast to unsteady RANS).

> We agree with the reviewer that it is important to convey that our simulations are steady-state, and we will include additional mentions of this point in the abstract, introduction, and conclusions.

section 2.1:

A note could be made on "…simple terrain with few turbines," since the paper does not deal with complex terrain, but in principle, the presented tool could be able to handle flows in complex terrain.

> This is an important point and we will explicitly point out in the methodology section and conclusions that the FEM approach and 3D flow solver are capable of handling complex terrain in future studies.

section 2.3:

1.) A note should be made that gradient-based optimizations can only find local minima (beside special, convex cases, where a local minimum is the global one).

> We agree that this is an important point and we have emphasized it in a number of places in the manuscript. In particular, we discuss local minima in the Background section on line 1 page 4, provide a paragraph on the local nature of our solutions in the Methodology section on lines 6-15, page 12, and readers are further reminded of the local nature in our Summary and Conclusions section on lines 12 - 15, page 21.

2.) It is a little misleading that you talk about "backward in time", but steady-state RANS is used later. A small note should be made on that.

In order to avoid confusion, we will add a clarifying comment that the "backward in time" discussion is intended to help readers develop a intuition for how the adjoint approach works in general, but that our specific study considers only steady state simulations where the adjoint variable is also steady-state.

3.) "The resulting adjoint gradients are typically more accurate than finite difference gradients". Gradients by finite differences can be of second order, but are the adjoint gradients of second order? If not, the FD could possibly be more accurate.

Since this is, as the reviewer correctly points out, a conditional statement depending on the specific details of how the finite difference and adjoint gradients are calculated, we will remove this statement from the manuscript in order to avoid confusion.

section 3.1:

Why don't you use a standard turbulence model like $k - \epsilon$*?*

We chose a mixing length turbulence model over a $k-\epsilon$ model because the mixing length model avoids the use of limiters and min/max functions that are non-differentiable and maintains a simple mathematical presentation. Mixing length models are familiar to the wind energy community through their use in eddy viscosity wake models [e.g., Ainslie (1988)] and in recent RANS models developed for wind plant controls [e.g., Boersma et al., (TORQUE 2016)]. The mixing length model results compare reasonably well to time averaged LES model results while maintaining a simple and optimization-friendly mathematical formulation. There are many improvements to the governing equations that could be made in future studies (more sophisticated turbulence models, buoyancy, Coriolis forces) but were beyond the scope of this paper as our focus was on the gradient-based optimization framework and adjoint techniques. In order to make our selection of turbulence model more clear, we will add a statement to the manuscript clarifying our decision.

section 3.3:

The layout is rotated instead of rotating the boundaries. So after each CFD, do you rotate the layout back for the use in your optimization algorithm? A note on that should be made.

The control variables that store the turbine positions are kept in a fixed reference frame, and are updated at each optimization iteration. To compute the turbine force corresponding to each inflow direction, we rotate the layout when calculating f_k. This rotation is a simple analytical function that is a part of calculating the turbine body force and is incorporated into the adjoint gradients. In order to clarify this, we will modify Eq. (13) to make the dependence of the turbine body force on the inflow angle and reference frame positions explicit.

section 3.5:

The gradients of minimum spacing could be derived analytically, couldn't they?

Yes, they are derived analytically in our implementation. We intended to convey this by stating that the gradients of the spacing constraint are "provided manually" in line 9, page 14, but will reword it to clarify that we mean analytically.

section 4/4.1:

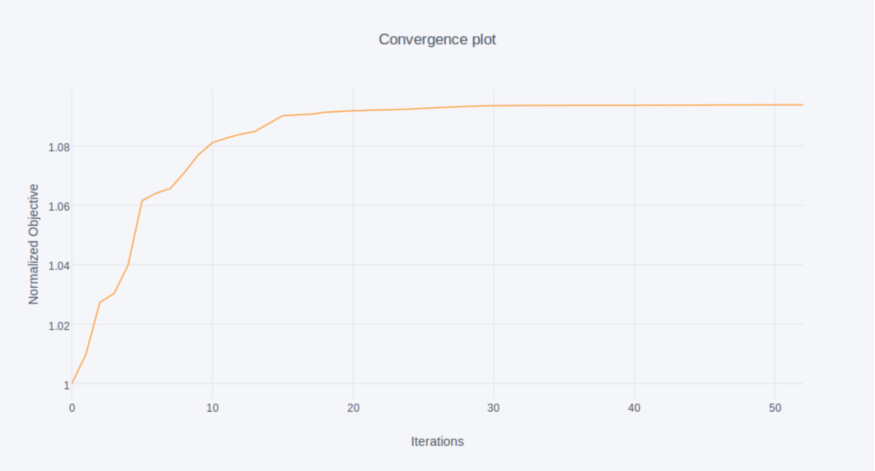
You claim that the "RANS flow solver accurately captures wind turbine wakes", but a verification/validation is not done (see also later: "providing confidence in the physical accuracy of the RANS flow solver"). You could mention that a verification/validation is not in the scope of the paper or you could refer to other publications, where the flow solver setup is verified/validated.

> We agree with the reviewer that a detailed verification/validation is beyond the scope of this paper, which is focused on the formulation of an optimization and flow solver framework that can be used with a wide variety of different RANS models. We will reiterate this point in the introduction and also tone down our statements of accuracy by noting that the RANS flow solver "qualitatively" agrees with prior studies.

section 4.2:

1.) A graph with the convergence of the optimization could be shown (if there's something interesting to see or in order to show that the optimizer is runs correctly).

> We considered adding just such a plot, but found it relatively uninteresting and not worth including in the submitted manuscript. Nevertheless, we have provided here for the reviewer a plot of the objective function convergence for the two direction optimization case described in Section 4.3 that is representative of all the optimizations.



2.) Is the Coriolis force included in the flow solver or why there is a curvature near the edges of the plant? Is it an effect of the domain size?

> A Coriolis force is not included in our governing equations. The wakes deflect away from the center of the plant due to slowing of the flow and spreading of the streamlines caused by the continuity equation. We will make this more clear by including a figure with streamlines that will show this effect.

section 5:

An idea for the future could be a comparison of your optimization setup (including a higher-fidelity *model) and an optimization using standard analytical wake models.*

We are working on exactly this topic for a followup paper and will make mention of it in our future work.

technical corrections:

section 3.2:

1.) It should be written, which quantity is shown in Fig. 1(c).

We will clarify this in the figure caption.

section 3.5:

Shouldn't there be a comma before "if"?

Thank you for catching this, and we will correct the sentence.

Sincerely, the authors.