Response to Reviewer 2

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First, we would like the express our gratitude for your time and effort in reviewing our paper and providing feedback. We have structured this response to be clear and easy to follow. Each of the reviewer comments will be shown in blue, immediately followed by our response in black. Please note that if the comments refer to specific pages, sections, or line numbers, they refer to the original submission. These references may be different in the revised manuscript.

The manuscript contains a description and numerical assessment of an algorithm for yaw angle optimization for wind plant wake steering. The authors claim to propose a new Boolean optimization method based on a greedy approach. The algorithm is not accompanied by any theoretical considerations regarding convergence and the ability of the algorithm to find feasible or optimal points. Hence, it should be classified and referred to as a heuristic and not as a method. Secondly, given the vast literature on heuristics for nonlinear 0-1 optimization problems it is surprising that the authors (i) do not include any references on that topic, and (ii) choose such a trivial algorithm and do not investigate other alternatives.

Thanks for this comment! In response to the first part of this comment, we agree. In the abstract and introduction we have replaced a mention of "method" with "heuristic." We have not made this replacement all throughout the paper because to our understanding, methods certainly include heuristics.

In response to the second part of this comment, we have included a brief addition of a Boolean approach applied to the wind plant layout optimization problem:

"Boolean approaches have been used in wind plant layout optimization, in which several potential turbine locations are defined (usually in a grid), and an optimizer is used to determine at which of these locations a turbine should be placed (Mosetti et al., 1994; Grady et al., 2005; Marmidis et al., 2008)."

In response to the trivial nature of our presented algorithm, we agree that it is very simple and that is the point! We have discovered a very easy way to approach the optimization of turbine yaw angles in a wind farm. As can be seen in the manuscript, our Boolean approach is described in less than a page, and the rest of the paper is used to demonstrate when it performs well compared to a more complex, traditional approach to the problem, and when it falls short. The purpose of this paper is to show that despite its simplicity, our Boolean approach can work very well and is extremely fast.

Based on the literature review and the simplicity of the problem formulation (e.g. lack of constraints) one can wonder if the considered approach reaches state-of-the-art in the field. It is of course sometimes relevant to use simplified models, but one of the main arguments in the introduction is to achieve a realism. The authors should clarify.

It is actually common in yaw control studies to only have bound constraints on the yaw angles. Additional possible constraints could be included to control loading on the turbines, but this could be (and often is) incorporated into the yaw angle bounds. In regards to the simplicity, our models are of similar fidelity to other papers published on wind plant wake steering using yaw control. The models presented in our paper are simply a means to demonstrate our new Boolean optimization method. In response to the simplicity of our new optimization method, please refer to the responses to the other comments on this concern.

In the introduction, we mention the error in wind direction measurement and wind turbine yaw measurement, which indicates that optimizing wind turbine yaw angle down to decimals of a degree is extremely unlikely to actually be achieved in plant operation. The following has been added to the introduction to help clarify this:

"This reality partially motivates using coarse discrete yaw angle possibilities in wind plant yaw optimization."

Several of the numerical experiments are based on artificial wind farms or wind scenarios that are unlikely to appear in a real world application, e.g. random turbine positions and turbines perfectly in-line with the wind and a single wind speed. Even if this is good for reproduction of the results, it is dubious if these examples can be used to make general conclusions that are applicable to real world wind farms. The authors are encouraged to extend the numerical experiments with these comments in mind.

We believe this comment greatly improves the paper. In the revised manuscript, we have added results comparing our method for a real wind farm layout with the associated wind resource (the Princess Amalia wind farm).

One of the main conclusions from the manuscript is that the proposed greedy algorithm is much faster than a traditional method, in this case SQP applied to a continuous version of the problem. There are several issues with this conclusion. Firstly, the two algorithms attempt to solve different problems and meet different requirements and are as such not comparable in a fair way, particularly when it comes to computational effort. Secondly, the implementation of the call to the SQP method is based on finite difference approximations of the gradient of the objective function. This is well known to have potentially very large implications on the computational time per iteration, the robustness of the algorithm, the achieved accuracy, and the number of iterations when the number of variables increase. How much the implementation choices affect the outcome is not reported. It is therefore possible that the differences in computational time are entirely attributed to the implementation rather than the method itself.

Excellent comment! In response to the second part of this comment, we have added the following to the revised manuscript to make sure the reader is aware that some of the difference in results could be from the continuous implementation:

"It is important to note that for all of the results in this paper, we have only used the continuous problem scaling, bounds, and finite difference gradients that we have described. We have not explored the sensitivity of the results to different implementations of the continuous optimization. It is possible that the differences in computational time are partially attributed to the parameters we have used while setting up the optimization, and not exclusively on the differences between the Boolean and continuous approaches."

In response to the first comment, yes! We definitely agree. And that is basically the point of this paper. With some basic understanding of the design space, we can greatly simplify the problem that we need to solve, getting almost as good of results with much less computational effort. They are different ways to approach the problem. We disagree with your assertion that the way we have compared the two is not fair. The continuous approach that we have compared to is common in the literature and an approach that many (including ourselves) have used and still use to solve for the optimal yaw angles in a wind plant. Therefore, comparing our new Boolean approach to solve the problem to this continuous gradient-based approach that is commonly used certainly makes sense. In the revised manuscript, we have not made any changes based on this portion of the comment.

The authors claim to propose a Boolean formulation. This formulation is not formally stated in the manuscript. The optimization problem that is stated is in fact the continuous problem on line 90. Please clarify.

The optimization problem defined in equation form is general and does not address how the problem is solved. This could be solved in a variety of ways, including with the continuous and Boolean methods discussed in this paper, or any other algorithm or problem formulation. The Boolean approach that we used to solve this optimization problem is described in Section 3.2, along with the algorithm. We believe that the confusion may have been from our use of the word “formulation,” which we have replaced throughout the paper to be more clear.

The authors argue that the proposed optimization formulation is novel. However, the literature review mentions that others have considered several discrete choices of angles. It seems that the Boolean approach would be a special case. Please clarify.

Great point. To our understanding, the space for possible yaw angles has been formulated as continuous or with finely discretized yaw angles in the past with the purpose of approaching a continuous design space. Our Boolean approach is a significant deviation from this assumption of continuous yaw angles that has been made in the past. After consideration, we have not made any changes to the revised manuscript based on this comment. In the literature review we have already specified that past studies have used a finely discretized design space which we believe is a sufficient explanation.

The statement on page 8 line 167 “... time is seen to increase exponentially with increasing design variables.” is not properly motivated and most likely not correct. It is much more likely that the increase in time is polynomial given the type of problems and the method employed. The authors should confirm that the increase is indeed exponential or revise/remove the statement.

This was reworded in the revised manuscript.
