Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-37-RC2, 2019

WIND
ENERGY
SCIENCE
DISCUSSIONS

Interactive comment on “Massive Simplification of the Wind Farm Layout Optimization Problem” *by* Andrew P. J. Stanley and Andrew Ning

Ju Feng (Referee)

jufen@dtu.dk

Received and published: 28 August 2019

This paper proposed an interesting parameterization method for wind farm layout optimization, that has the potential of largely reducing the number of design variables. In general, the paper is well written, the new method is useful and results seems promising.

However, there are some major concerns the reviewer has on the current paper that he recommend this paper for a major revision. The major concerns are as follows:

1. Missing details in the proposed boundary-grid parameterization

As the central contribution of this study, the boundary-grid parameterization is not presented in a complete and clear manner. After reading Section 2, the reviewer can't

figure out how exactly the 5 design variable can determine one and only one layout inside the specified boundary with a given number of turbines. For example, if dx and dy is too big, the number of turbines you can put in the inner grid will be very few, then there might be too many turbines placed on the perimeter, that violate the minimal spacing constraints. Also the same set of dx, dy, theta and b can define a set of grid points that actually shift in the boundary, which will correspond to different layout. So the reviewer would argue that dx, dy, theta and b alone can't have a one-to-one map to a exact location of grid point.

The selection of discrete values also seems a little bit arbitrary. It is stated in lines 87-88, the discrete values remain fixed, but then again, you have the situation that there are too many grid points inside the boundary (when dx and dy are small), if you have to put 45% turbine around the boundary, you will have to remove some grid points, then which ones to remove according to what rule?

2. Some shortages in wind farm modelling.

First, in lines 117, it says "the turbulence intensity is equal to 0.0325", but shouldn't turbulence intensity change upon the wind speed?

Second, according to Eqs.(3-4), you use the wake deficit at the rotor center to represent the average wake deficit on the whole rotor, since there is no integration over the rotor area in Eq. (4). This is problematic, as the profile of wake deficit is a Gaussian shape, and the one point deficit in the rotor center could be overestimating the mean deficit, if the two turbines are perfectly aligned.

Third, there are only 5 wind speeds, and 23 wind direction sectors used in the wind resource modelling, according to Eq. (7). It has been shown in some studies that you need finer discretization, for example in (Feng and Shen 2015) in your references. This kind of coarse discretization could give you artificially optimistic AEP gains. You may also check the follow paper for recommended discretization:

Feng, Ju, and Wen Shen. "Modelling wind for wind farm layout optimization using joint distribution of wind speed and wind direction." Energies 8, no. 4 (2015): 3075-3092.

3. The missing of comparison to gradient-free optimization technique.

I understand the focus of this study is on the proposed parameterization. But without direct comparison of the gradient based optimizer to some gradient free ones, e.g., GA or RS, it looks unfounded and somehow biased for a lot of claims that says the gradient free method will be infeasible, or perform worse. Also do you have bounds on the design variables? How are the constraints handled in the optimization process? Penalty function?

4. The calim on the infeasibility of gradient-free technique for large wind farm is unfounded.

AS stated in lines 9-10, Our presented method unlocks the ability to optimize and study large wind 10 farms, something that has been mostly infeasible in the past.". But I found this unfounded, you can check the following paper:

Wagner, Markus, Kalyan Veeramachaneni, Frank Neumann, and Una-May O'Reilly. "Optimizing the layout of 1000 wind turbines." European Wind Energy Association Annual Event 205209 (2011).

Also engineering wake models are very fast to run, it shouldn't become too heavy or even infeasible for a gradient-free optimizer applied to a wind farm with 100 turbines, even if needs 10000 evaluations.

5. Some very relevant references are missing.

Especially studies on grid-like layout optimization. The parameterization for the inner grid has been proposed in a similar way in some studies already. You may find the following two of interest:

González, Javier Serrano, Ángel Luis Trigo García, Manuel Burgos Payán, Jesús

Riquelme Santos, and Ángel Gaspar González Rodríguez. "Optimal wind-turbine micro-siting of offshore wind farms: A grid-like layout approach." Applied energy 200 (2017): 28-38.

Neubert, A., A. Shah, and W. Schlez. "Maximum yield from symmetrical wind farm layouts." In Proceedings of DEWEK. 2010.

Some minor issues:

1. It is stated in lines 20-25 that "Although these methods can be highly effective for small numbers of design variables, the computational expense required to converge scales poorly, approximately quadratically, with increasing numbers of variables. Because of this poor computational scaling, many companies and researchers have been limited in the size of wind farms they can optimize, as the number of variables typically increases with the number of turbines." But I doubt that's the case, since there are already large wind farms be designed and built in the world. Also optimization studies have been conducted for large wind farms, such as Horns Rev 1 with 80 turbines, as in one of your references (Feng and Shen, 2015).

2. Lines 31-32 "Power losses of 10–20% are typical from turbine interactions within a wind farm (Barthelmie et al., 2007, 2009; Briggs, 2013), and can be as high as 30–40% for farms with closely spaced wind turbines (Stanley et al., 2019)." This is somehow misleading, power losses of 30-40% are the worst wake case, which doesn't happen that frequent in reality. So the actually AEP loss due to wake effects should be usually lower than 10-20%.

3. Rosenbrock function is used to demonstrate the convergence of gradient based optimizer scales better than gradient-free methods. First, you need to show what is Rosenbrock function, or at least provide a reference. Second, this function is a function that we actually know where are the optimums, thus, we can easily see when it has converged to a local minimum. But in real life applications, we often can't analytically prove that we have reached a local minimum, such as in layout optimization. Third, for

such problem, converge faster (typically for gradient based methods) is just one aspect, the other aspect is the quality of the optimized results, i.e., whether the solution is close to the global optimum. Usually it is know that gradient free methods converges slower but has a higher probability to reach the global optimum, while gradient based methods converge faster but are also easier to be trapped in a local minimum.

4. Eq. (6), U_mean should be scale factor of the Weibull distribution. Note that the scale factor is not the same thing as the mean wind speed, instead the mean wind speed should be a function of scale factor and shape factor.

5. Line 275-276 states that "BG parameterization, cabling requirements can be clearly minimized by running cables across each of the rows, and around the boundary without the need for complex cabling algorithms". This is not true, as you still need to decide the location of sub-station, the exact topology of the cables and select cable types for different connections, thus, not necessarily easier than any random layout.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2019-37, 2019.