Exploring future production scenarios for the Italian offshore wind power
by
D. Medici, A. Tonna & A. Segalini

Comments to Reviewer #1:
(the text of the reviewer is in italic)

We appreciate the additional feedback regarding our manuscript. In the following we address the reviewer's suggestions for improvement, and point out the changes compared to the original manuscript. Parts that have been rewritten or added due to comments by the referees have been highlighted in red in the revised version of the manuscript.

I agree that neglecting wake losses reduces significantly the complexity when the Monte Carlo simulation is conducted, if the simulation is only a preliminary tool to estimate the probability of installation of a certain wind farm. However, even though I see the necessity of keeping the analysis simple at this stage, neglecting the wake losses is a limitation of this analysis that deserves to be mentioned in the text. The Monte Carlo analysis can stay as it is, just mention that wake losses are not considered at this stage.

We apologize for the lack of clarity of our answer. Wake losses are accounted for in each farm in our analysis. Without wake losses there would be not even need to decide a tentative layout.

I see the authors' point. However, labeling an optimization algorithm as "outperforming" with respect to another indicates that the former leads to a smaller cost function than the latter, whereas here it is used only to indicate cases when $L_{opt} > L$. I would avoid confusion and motivate these cases as characterized by a low number of turbines in the selected layout.

We agree with the reviewer and we have replaced the term with "where the optimization algorithm converges towards a spacing larger than the uniform solution" as initially suggested.

The new comments from the referee have certainly helped us to improve our manuscript and we hope that the comments have been taken into consideration satisfactorily.

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Comments to Reviewer #2:
(the text of the reviewer is in italic)

We appreciate the additional feedback regarding our manuscript. In the following we address the reviewer's suggestions for improvement, and point out the changes compared to the original manuscript. Parts that have been rewritten or added due to comments by the referees have been highlighted in red in the revised version of the manuscript.

> *I am convinced that maximizing the minimum distance and maximizing AEP give optimal layouts that perform similarly. This is good to know. But it was not my issue. My issue was that ignoring the wind rose is unacceptable because it means that you are assuming that all the wind directions are equally likely and all the wind speeds are equally likely, which does not happen anywhere on Earth. Let me say it again: no place on this planet has a wind distribution that is uniform along all directions and all wind speed bins. As such, optimizing any property, whether the AEP or the minimum distance, based on such a wrong wind distribution is fundamentally wrong and will give you a layout that underperforms. In other words, it is scientifically incorrect to optimize for a condition that is absolutely unrealistic, no matter which method or variable you use for such optimization.*

We agree with the Reviewer on this concern and that any layout not based on the wind rose will underperform compared to the optimal one. However, our goal is not to propose a new methodology to design wind farms or identify unexplored optimal layouts. Our goal is to perform a portfolio-scale and system-level analysis of offshore wind deployment, where simplified layouts are appropriate. As such, even an approximation of the wake losses magnitude is sufficient for the goal of the project, estimate that we obtain from the geometric optimization in a rapid way. We have shown to the Reviewer in the previous and present answer the accuracy of this rough estimate.

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Your results for Lillgrund, actually, confirm my statement because Lillgrund is the worst performing wind farm in the world, with wake losses of the order of 60%, due to the tight spacing (4.3D x 3.2D). Also, Lillgrund was designed originally for much smaller turbines and only at the very last minute they decided to install the Siemens 2.3 MW instead, which had just become available in the market. They were aware that the 2.3 MW turbines would have high wake losses (because of the larger diameter), but they were willing to accept it because the total output was going to be higher than the original anyway. So, the very fact that your algorithm gives you a layout at Lillgrund that performs similarly to the actual layout at Lillgrund means that your algorithm is indeed bad and produces underperforming layouts.

In our analysis we need the array efficiency of each farm (whether constructed or not in the Monte Carlo simulation) and therefore we estimated that based on geometric considerations rather than by maximising the AEP, as one usually does. The question is however, if our estimate is so bad to become unreliable. In the previous review of the paper we included Lillgrund as an example because it is a farm strongly affected by wake losses and we had SCADA data available over 10 years. The goal for us was never to identify the best performing layout but rather to assess the order of magnitude of wake losses in an reasonably good and efficient way.

However, by recognising the concern of the Reviewer about Lillgrund, we have performed a similar analysis on Horns Rev 1. There the intra-turbine spacing is 7 diameters and the farm should be better performing. Figure 1 shows the comparison between the current layout and the approximated one obtained from the geometric optimization. The comparison is qualitatively similar to what observed in the Lillgrund case: the wind directions deeps are removed since no turbine alignments are present in the optimized layout, the average array efficiency is similar to what obtained for the installed layout (actually better since the deeps are removed, but this is also to be weighted by the wind rose as the Reviewer pointed out). This figure will not be included in the manuscript because we deem the Horns Rev 1 estimate analysis potentially influenced by the used wake model (the Jensen model here), and therefore of lower quality than the Lillgrund analysis where SCADA data were used.

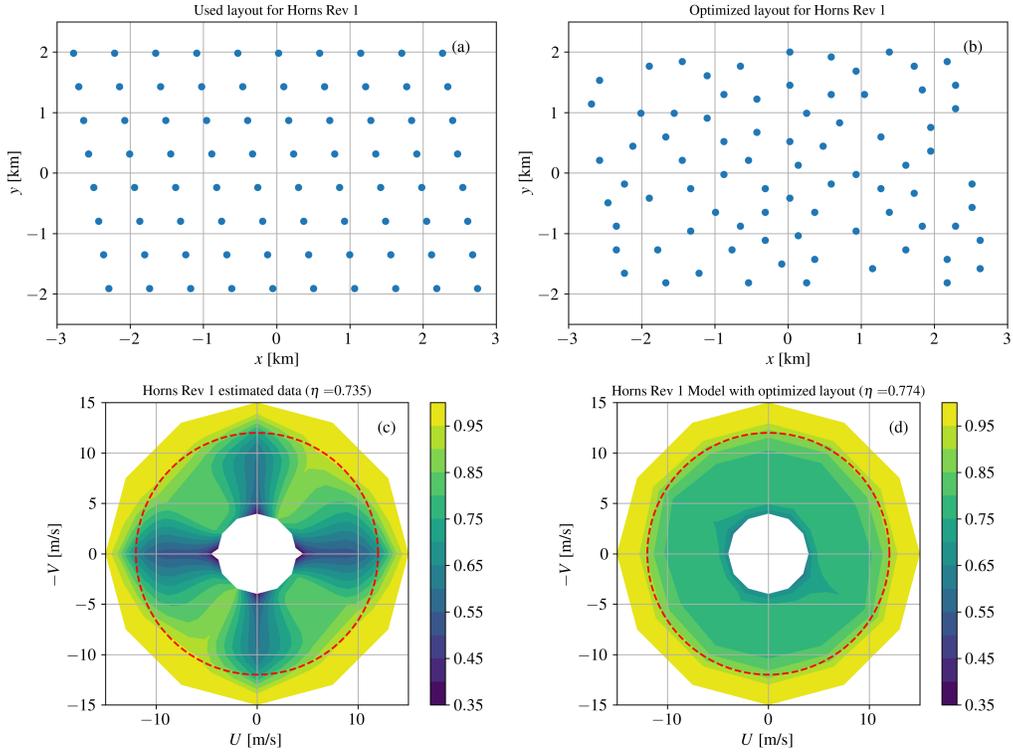*Not true. The prevailing wind direction is the most important*

Figure 1: Validation of the optimization method on the Horns Rev 1 wind farm. (*top row*) layout of the farm, (*bottom row*) array efficiency for different wind velocities and directions (the red dashed line indicates the rated speed of the installed Vestas V80 wind turbine, 12 m/s). (*left*) real farm with efficiency estimated by means of the Jensen wake model (*right*) optimized layout with the minimum distance fitness function. The title of the bottom row reports the average array efficiency of the farm for velocity below rated speed.

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parameter to build an optimal layout.

Correct. The comment that the Reviewer was referring to was included in the previous answer and only relative to the array efficiency of the Lillgrund farm with the layout estimated with the genetic algorithm. There the wind direction does not play a significant role compared to the velocity magnitude. The same consideration could be extended for the Horns Rev 1 analysis shown in figure 1.

> *This is irrelevant, the tests conducted with WindPro were comparing AEP versus minimum distance optimization for the same uniform wind direction distribution, so these tests cannot be used as an argument in favor of the choice of a uniform distribution.*

We feel that the Reviewer is wrong about this point. We clearly stated in the previous answer that "The wind farm area shows a prevailing wind direction, and the wake loss has been weighted by the frequency of the sectors.", so we consider the WindPro validation highly relevant and supporting our simplified approach.

> *What kind of an answer is this? You do not "desire to report" the equation or at least the definition of centroid? Please do it, because you want to make your paper more understandable and right now it is not.*

We have included the centroid definition in the revised manuscript by stating that "The centroid was calculated as the arithmetic mean of the latitude and longitudes of the bounding polygon".

> *Please add this to the text, especially the fact that the interpolation was linear with distance, not inverse square or other way.*

Good suggestion. We have added the sentence "Given the centroid (latitude-longitude), the velocity time series at the nearest grid points were downloaded from the CERRA database and linearly interpolated to the centroid location." to the revised manuscript.

> *I do not find it acceptable that you mention and use a dataset, but you refuse to describe it. Please describe it.*

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The EMD WRF dataset was not used in the present work. It was only preliminarily compared to the CERRA dataset to see whether they were consistent. Both datasets are publicly available and not part of specific simulation tools that we could modify by choosing simulation parameters. As such, the citation of the associated references to both datasets is sufficient.

> *And here you manifest the opposite behavior: when asked to remove something, an analysis that you do not even use, you want to retain it. Please remove it.*

We still consider the analysis interesting and we want to keep it. The Reviewer suggestion will make the paper more straight to the point, but we feel that the correlation analysis might be good also to plan the compound operation of wind farms in the Mediterranean Sea.

> *The definitions of crossover, mutation, and elitism must be added in the text, not just in the reply to me.*

In the previous manuscript we added references to a textbook about these operations typical of genetic algorithms. Describing what they do will make the paper unnecessarily focussed on the genetic algorithm.

The new comments from the referee have certainly helped us to improve our manuscript and we hope that the comments have been taken into consideration satisfactorily.