Exploring future production scenarios for the Italian offshore wind power
by
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Comments to Reviewer #1:
(the text of the reviewer is in italic)

We appreciate the 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.

> *The problem is well stated and addressed, the approach is original and delivers results with acceptable uncertainty levels.*

We thank the Reviewer for the support to our work.

> *Line 13: I would replace "power" with "energy" as it sounds more appropriate.*

We have replaced "power" with "energy" as suggested in the revised version of the manuscript.

> *Line 31: Please replace "address" with "addressing".*

Done.

> *Line 64: Which percentage of the total lease areas is shared between multiple projects?*

Given the coordinates of the wind farms, it was possible to identify the polygons containing the park area. The area of intersection between the different wind farms was then calculated. For each park involved in the intersection, the percentage that this intersection area represents relative to the entire park was computed.

> *Line 78: I feel that Fig. 1 and 2 deliver essentially the same message. Therefore, I recommend keeping only one of these two figures or, alternatively, organizing them as panels (a) and (b) of just one figure.*

Figure 1 gives an information about the mean wind while figure 2 shows the 90$^{\text{th}}$ percentile of the wind speed. The two figures look qualitatively as a first glance but they are not the same and it is impossible to reconstruct one from the other. Figure 1 shows the wind we can expect on average, while figure 2 provides a better description about the wind extremes. Since it is important to highlight the regions where wind farms are planned, we prefer to keep large and essentially as two separate figures rather than parts as the Reviewer suggests. We anyhow see the rationale for this suggestion and we thank the Reviewer for that.

> *Lines 104-105: What about the remaining 57% of the planned farms?*

We have realised that our percentages were wrong and we have now corrected to 18% of the farms located over a sea depth of less than 100 m, while 24% of the farms are placed over 500 m sea depth. The remaining 58% lie over an intermediate depth between 100 m and 500 m. We have corrected the percentages and updated the manuscript in its revised form.

> *Line 106: Is there any available turbine model featuring 100m hub height instead of 150m? I am a bit concerned about the uncertainty introduced by the shear exponent as it is strongly dependent on stability and, in some cases, wind direction. Alternatively, please report some literature references motivating your choice of shear exponent.*

The IEC 61400-3-1:2019 standard provides a reference shear exponent of 0.11 for offshore conditions. Our value just came from an educated guess and a shear exponent of 0.1 leads to an underestimation of 0.4% of the wind speed at 150 m, which is insignificant since higher uncertainties are present: as the Reviewer suggests, the shear exponent might be dependent on a variety of factors. One of the co-authors has recently performed a measurement campaign over the Atlantic ocean reporting the occurrence frequency of the shear exponent and this varied significantly (it can be both lower or higher than 0.1). Both the IEC and the mentioned work are now cited in the revised version of the manuscript. The advantage of assuming a fixed shear exponent is that it is just a constant factor making the assessment easy. We decided to go for a standard IEA 15 MW turbine since many simulations and data are available for this turbine, as explained within the manuscript. We therefore prefer to keep the turbine model as is.

Line 115: From Fig. 4, the transition between region II and region III is around 11 m/s, which is lower than the average wind speed at 100m shown in Fig. 1 for the selected areas. This means that, for a significant portion of the time, the chosen wind turbine will be in region III. Please address this aspect.

None of the selected areas has an average wind speed larger than 8 m/s. Maybe the Reviewer was hinting to Figure 2 (the 90^{th} percentile velocity). Yes, it is generally expected that the velocity should be for a significant fraction of the time above the rated speed (how much depends on the frequency distribution of the wind speed), so we don't deem necessary to state this clearly in the revised version of the manuscript.

Line 149-150: I believe it is useful to state which wind direction and speed you considered to quantify these correlation functions.

All wind directions and wind speeds were used to obtain the spatial correlation and this is now described in the revised version of the manuscript. We actually performed detailed correlation analyses for binned wind speed and direction but no clear added insight was obtained by the refined analysis so we preferred to keep it simple and robust.

Line 212: When I read Sect. 5 for the first time, it was unclear to me why you ignored wake losses so far and then you decided to introduce them. Only at the end it was clear that this result is preliminary towards the Monte Carlo simulation. I would explicitly mention at the beginning of Sect. 5 that, just like the previously introduced score range, wake loss modeling (and layout optimization) are instrumental to the Monte Carlo simulation.

The Monte Carlo simulations are generated to identify possible scenarios where some farms are built or not. Once a farm is built, its power production depends on

1. the number of turbines: this parameter was kept fixed according to the planned capacity of the farm;

2. the available wind resource: this information was created based on the historical data and did not change in the Monte Carlo simulation since no farm-farm interactions were accounted for;

3. the farm efficiency and how that is decreased because of wake losses.

The first two factors are sufficient to get directly an estimated power production under the assumption that all turbines operate independently from each other. We actually ran the first Monte Carlo simulations without even including wake losses. The advantage of neglecting wake losses is that the power production does not depend anymore on the farm layout, simplifying the analysis. However, whenever many turbines are expected to be installed in a small area, wake losses cannot be neglected anymore. Since several aspects are considered in the layout definition, we thought that a simple layout defined by maximizing the minimum distance (without considering the wind rose and the associated wake losses) was a simple enough choice that gave a simple estimate of the wake losses. Once again, wake losses are not a necessary ingredient to perform the Monte Carlo simulation, but rather they enhance the accuracy of the estimate. We have performed in the manuscript a critical assessment of the optimization technique and we have clarified that in the revised version of the manuscript.

> *Line 215: Is the number of turbines decided a priori? If so, which source did you use to obtain this value?*

The number of turbine has been decided based on the planned wind farm capacity. The total capacity of the farm comes from the presented projects at the MASE. The projects can have different types of turbines in the existing documents, but we chose to consider only a single type of turbine for all the projects, in particular the 15 MW model. The number of turbines is then calculated as the total planned farm capacity (in MW) divided by 15 (and rounding to the lowest integer digit).

> *Line 234-236: I am not sure that the current choice of fitness function is better than the AEP. It is true, as the authors state, that larger spacing between neighboring turbines is beneficial to the overall power production. However, the intra-wake region of a large operating wind farm is a place of complex flow interactions involving, for instance, speed-ups among turbines which are compelling features to enhance power production. Thus, I recommend showing at least one wind farm case where the optimization of the AEP leads to a similar layout as the optimization of the turbine spacing.*

We understand the concern of the Reviewer and we have considered a validation case provided by the Lillgrund wind farm to support our methodology. For this task we have used the SCADA data analysis performed by Sebastiani et al. (Wind Energy, 2021). Having at hand the bounding perimeter, the number of turbines used (48 Siemens 2.3 MW with diameter 93 m), and the power curve of the chosen turbine, an optimized layout was obtained as well as the array efficiency of the farm. While it is possible to identify differences between the array efficiencies (such as the maximum wake losses for wind direction 120 and 222 degrees, absent in the layout optimized with the present methodology), the two distributions are roughly the same. It is interesting to note that the average array efficiency in the optimized layout is 0.622, while in the real layout is 0.616: this is expected since two turbines were not installed in the centre of the real farm due to shallow water constraint, an aspect that was not considered in the present optimization, leading to a higher efficiency. Once again, our goal was not to propose a project of an existing farm but rather to roughly estimate the wake losses to assess the power production of still not existing farms knowing only the bounding perimeter. We have now discussed this interesting validation case in the revised version of the manuscript.

We have also run an independent test case for one of the planned wind farms featuring more than 35 wind turbines over a large area. The AEP optimised approach has produced a layout on 3 parallel rows with cross-wind distances in the order of 8 rotor diameters. This optimisation is not factoring electrical losses or cables costs, however. The genetic algorithm optimisation has been run for the same wind farm over the same area, leading to an irregular layout as expected. The wake losses have been estimated with the WindPro software and the Park2 wake model and the wind farm wake loss difference is only 0.6%, confirming in our opinion the quality of the genetic algorithm approach.

> *Line 254: I would not label the cases where $L_{opt} > L$ as "outperforming". The optimization algorithm always (hopefully) outperforms the uniform spacing solution in terms of finding the best layout, otherwise it would be detrimental. I suggest to rephrase this sentence saying, for example: "where the optimization algorithms converge towards a spacing larger than the uniform solution".*

A genetic algorithm is not a gradient method and it is fully stochastic.

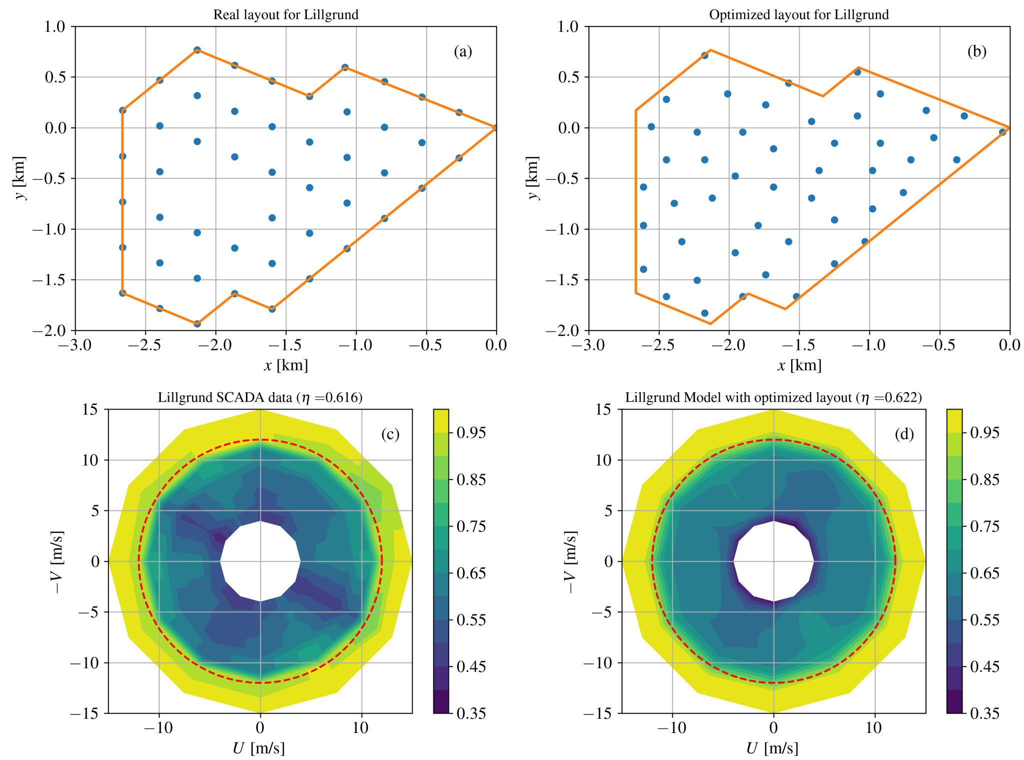

Figure 1: Validation of the optimization method on the Lillgrund wind farm. (*top row*) layout of the farm, (*bottom row*) array efficiency for different wind velocities and directions (the red dashed line indicates the rated speed of the installed Siemens wind turbine, 12 m/s). (*left*) real farm with SCADA data analized in Sebastiani et al. (Wind Energy, 2021) (*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.

We start with a population of possible layouts and iteratively generate new possible layouts and check the fitness function. The evolution can be quite long for so mane degrees of freedom and consequently the stop criterion is given by a given number of iterations (500*number of turbines). There is no guarantee that the algorithm has converged by then. If the bounding polygon is a square, the optimal layout is clearly only the one with uniform spacing. For other bounding boxes, it is expected that the uniform spacing is still the best although not realizable or easily identified. Therefore, the uniform spacing represents our target and most likely is the best performing condition. That is why we prefer to keep the text of the manuscript as is.

> *Also, if you believe there is a correlation between* $L_{opt} > L$ *and the number of turbines, it would be interesting to plot Fig. 10 as a scatter plot where each point is colored according to the number of turbines present on each wind farm.*

We agree about the fact that it would be of interest, but this will shift the focus of the manuscript too much on the layout identification part, which is only a coarse tool we used. Having more time available, the layout identification with a gradient-based method where the AEP is maximized would be better, but we thought that our choice was a good trade-off.

> *Line 268: Please make an explicit mention to Fig. 11b.*

Correct. We do it now in the revised manuscript.

> *Line 294: Since the unit on the y-axis in Fig. 13 is TWh/yr, I would replace "power production" with "energy production".*

Done.

> *Line 295-296: How do you explain this trend? Could it be due to the seasonal variability of the available wind resources? This point deserves further explanation*

Yes correct, the wind resource in Italy is higher during winter months and therefore the energy production is higher.

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