

2nd Review of:

”Data-driven optimisation of wind farm layout and wake steering  
with large-eddy simulations”

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New comments have been added in red.

## General Comments:

The article present two different studies on optimization of wind farm performance, one on layout optimization and one on wind farm control using wake steering. The optimizations are performed using high-fidelity (LES) and low fidelity (FLORIS) wake modelling as well as multi-fidelity by combining results from both. The results are based on an impressive amount of LES, and provide interesting results. However, the article also show several shortcomings and several aspects, which lack important details for instance in terms of modeling choices, numerical blockage and uncertainties. Therefore, I recommend major revisions.

We thank the reviewer for the time devoted to reading our manuscript and for providing helpful comments and feedback.

The provided revision has minor adjustments to previous comments, but does not properly address a number of my comments from the first review and some new comments/results appear misleading. This is at least partly due to important details still missing from the manuscript, which still leaves a reader guessing or assuming key details of what the authors have done.

## Specific Comments:

### Lack of details:

#### Actuator Disc modeling and Power estimation:

The description of the actuator disc method is very inadequate, and Bempedelis et al. 2023 does not provide the necessary details. I can only assume that an uniformly loaded actuator disc is employed. If so, it is far from state-of-the-art and it has been shown several times to be insufficient, particular for more complex flows such as yawing turbines, see for instance [1], where a standard (uniform) actuator disc is compared to a BEM based actuator disc with rotation.

[1] also show how power production behind a uniformly loaded actuator disc is significantly over-estimated compared to a slightly more advanced actuator disc model using BEM and rotation.

Therefore, the authors should specify how the power is estimated, as this is particularly important for yawing turbines, and further complicated if yawing turbines are operating in waked conditions, see [2]. I suspect the efficiencies in Figure 12 are a result of simplification, but it is hard to test/decipher as a reader. It would be beneficial to also report the actual power production (in [kW]), not only the efficiency. Furthermore, I have some concerns about the optimization angles shown in Figure 12. First, it is a general concern when optimizations consistently provides results at the admissible limit, as is the case where the first four turbines are yawing  $30^\circ$ . Second, several higher fidelity simulations have shown that there are secondary effects of wake steering. This means the turbines operating in wake will typically yaw less, see [3] for two turbines, where the second turbine yaws positively to increase production, and [4], where deep-row turbines should yaw relatively less. Such secondary effects are actually also visible in Figure 8.

We thank the reviewer for the thorough analysis and comment. The wind turbines are modelled with a standard non-rotational actuator disk model. In section 2.2 of the revised manuscript, we have added a brief description of how power is computed in the actuator disk method, along with four more references [3, 10, 7, 9] besides [2]. Additionally, we have added a link that directs the interested reader to the open-source code repository for direct access to all methodology and implementation details (<https://github.com/xcompact3d>).

As suggested by the reviewer, the ADM with rotation is a more advanced turbine representation model compared to the standard ADM that we employ in the present study. Nevertheless, the optimisation framework we propose is compatible with any type of turbine representation model (e.g., standard actuator disk, actuator disk with rotation, or actuator line). This is now mentioned in section 2.2 of the revised manuscript, together with a comment on the potential for improved turbine wake and power predictions through use of more advanced turbine modelling approaches. The efficiencies shown in figure 12 are calculated as  $\eta_{ind,i} = P_i(\gamma)/P_i(\gamma = 0^\circ)$  (see section 4.2 in the manuscript). Efficiencies are used as a more suitable metric for the comparisons made in this study, as opposed to presenting dimensional power values. The actuator disk method inherently presumes a connection between thrust and power coefficients [10] unless the user specifies otherwise [12]. In our study, where constant thrust (and power) coefficients are assumed, employing efficiency metrics effectively conveys the trends without the need to specify a power coefficient that is representative of a specific turbine only.

Secondary steering effects are accounted for in our simulations. LES-ADM naturally captures the counter-rotating vortices that are present in the wake of a yawed turbine and lead to its curled shape (see, for example, [8]). This is evidenced by figures 12 and 13, where the wakes of downstream turbines deflect by approximately equal amounts, though requiring increasingly smaller yaw angles, and by the wake of the last turbine, which deflects considerably even though the turbine is at almost zero yaw offset. The inclination towards large yaw angles at the first few turbines may be attributed to the actuator disk theory overestimating the power of yawed turbines. Nevertheless, the operational details of the turbines are identical in both flow models (LES-ADM and FLORIS). This suggests that, in the comparison between the two flow models, the preference for larger yaw angles in the LES is likely related to the flow mechanisms (e.g., row interactions) that are missing in FLORIS. The above have now been added as comments in section 4.2 in the revised manuscript.

In the reply to my review and the other reviewer, the implementation of the actuator disc is still lacking details. The authors state that CT is constant (e.g. line 170), but it is unclear how and if CT changes when yawing. In the reply to the other reviewer the authors repeat that CT is constant, but also state that it is function of yaw angle. This is quite critical for the present study. If CT is indeed constant, is the total thrust force also constant? If so, then a yawing turbine would reduce the streamwise induction (as the thrust is directed with an angle laterally), leading to a small increase in the mean streamwise wind speed and therefore increase in power  $P = u^*T$  with constant T. That would mean that yawing turbines are not penalized in terms of power production.

The LES results would essentially be meaningless and should not be published. I do not think this is what is going on, particular as the authors state that the efficiency of the turbines depends on the yaw angle, but as it is, I have to speculate and assume this. The authors need to describe this in proper detail for the readers, for example using an exponent to describe changes in CT and CP as in [2], which as mentioned in my first review is particular important for yawing turbines in wake. I'm not convinced that "efficiencies are ...more suitable metric" as [1] clearly shows that the efficiency is overestimated with this actuator disc implementation. Reporting efficiencies for the simplified actuator disc makes it harder for readers to actually decipher the results. Again, having to guess/speculate on the underlying numbers due to lack of details. However, with proper explanation of how the turbines are modelled in yaw it might be OK.

Finally, the authors do not address my concern about the yaw angles of the first four turbines in LES-BO being optimized to the limit of the allowable range ( $30^\circ$ ). This clearly shows that the optimization limits are not wide enough, which is a basic flaw in optimization. The authors should address this.

### Numerical blockage:

For the second study, the authors assess that the estimated efficiency would reduce from 4% to 3.3% by reducing the numerical blockage from  $(\pi \cdot 80m^2)/(560m \cdot 1024m) = 3.5\%$  to  $(\pi \cdot 80m^2)/(5 \cdot 560m \cdot 1024m) = 0.7\%$  (I assume it is a typo that line 282 states km, not m). This analysis is good and the results plausible. However, in the first study, the numerical blockage is up to  $(16 \cdot \pi \cdot 100m^2)/(3340m \cdot 501m) = 30\%$  and even if the 16 turbines were arranged in a  $4 \times 4$  it would correspond to 7.5%. For such layouts, the numerical blockage is significant and the impact clearly seen in the results with substantial speedups, where certain turbines have efficiencies of 110% (Figure 7). I am sceptical how realistic these speed-ups are, particular given the sensitivity mentioned for the second study. The article states that the impact is less than 1% when tested with a domain three times wider (line 230-231) and refers to Antonini et al. (2018) and King et al. (2017). I can not find the details in Antonini et al., which is also a 2D simulation, but King et al. have blockage ratio of 5.2% for worst case scenario and reference Chen and Liou to give a threshold of less than 10% for wind tunnel studies. I think the details of this analysis is required in an appendix.

In both cases care was taken so that blockage effects are limited (to a degree that would still render performing all simulations feasible). In section 3, the maximum blockage (if all turbines are aligned normally to the flow, and without considering the porosity of the turbines) is  $(16 \cdot \pi R^2)/(LyLz) = (16 \cdot \pi \cdot 502)/(3340 \cdot 501) = 7.5\%$ . In the second study, the blockage is  $(\pi \cdot 402)/(560 \cdot 1024) \leq 1\%$ . (The values provided by the reviewer were computed using the diameter rather than the radius of the turbines.) The domain blockage in all simulations remains below the 10% threshold mentioned by the reviewer. The typo (km instead of m) has been corrected.

I apologize that I made an error in my previous assessment of the blockage. However, I still suspect that the reply and additional text by the authors is misleading. Evaluating blockage in terms of the difference in the *maximum streamwise velocity* within the domain is not representative of the impact of numerical blockage on the operation and power production of the wind farm. I suspect/guess the small increase in maximum velocity occurs high in the domain, where the velocity is already high due to the atmospheric boundary layer. In order to assess the numerical blockage, you should report speed ups of mean wind speed at the turbines and differences in power production with the two different domains, one 3 times wider than the other. Typically, physical wind farm blockage is results in 1-2% changes in power, see e.g. [5, 6], not 5-8% as shown in Figure 7.

## Uncertainties:

- line 126: In principle, turbines should align themselves with the incoming wind direction. The reality is however, that unintended yaw misalignment is a very large uncertainty in normal operating wind farms, and it is a large challenge in order to actually apply wind farm control, where it is notoriously difficult to determine a wind direction and hence provide accurate estimates of how much to yaw, see for instance [7, 8].

The following comment has been added in the revised manuscript to reflect this issue: “The turbines can align themselves with the mean direction of incoming wind. In practice, accurate and robust estimation of the wind direction poses a significant challenge [1].”

I find it a bit hard to understand this sentence and where it is placed in the text. Does “The turbines can align themselves...” refer to the optimisation setup or to reality? If it relates to the simulations and the setup, then it implies that the authors have utilized a numerical wind direction controller. If so, is the reference baseline before optimizing the yaw angles including alignment to local wind directions for all turbines? The footnote clearly relates to reality, but placed in the setup of the optimisation. I suggest to rephrase to correctly represent if the simulations does or does not include a wind direction controller utilized to determine the baseline, and that this is a challenge in reality. I suggest to place such comments in a more appropriate section, for instance while discussing limitations and uncertainties of the present study.

- Section 2.1 and line 206: SLSQP is used to optimize FLORIS while LES is optimized with BO, but does the choice of optimization strategy not potentially affect the optimization results? Please motivate why different methods are used.

SLSQP is used as it is the default and most well-tested optimisation method in FLORIS (this is now stated in section 3.2 and Appendix B of the revised manuscript). [11] studied the performance of different optimisation methods and showed that they perform similarly when appropriately used. One potential issue with gradient-based methods as SLSQP is their poor performance when dealing with multi-modal optimisation problems. To address this in the present study and enable a comprehensive comparison with BO (which is a global optimisation method), we implemented a multi-start approach for layout optimisation, and utilised a set of optimal yaw angles (as obtained in [6]) as initial conditions for the wake steering problem. On the other hand, the optimisation framework we propose relies on LES data. As discussed in sections 1 and 2 in the manuscript, a Bayesian approach is adopted as it is particularly attractive for optimising expensive-to-calculate functions and for use in problems where adjoint methods would be hard to apply.

OK.

- There are significant uncertainties of wake steering related to the impact on structural loads, particular for large yaw angles, which is not addressed.

A relevant comment along with two references has been added in section 4.2 of the revised manuscript: “For large yaw offsets, it is important to also consider the effects of yawing the turbine on the loads it experiences [4, 5].”

OK. The authors have added a few scattered comments on uncertainty, but the uncertainties remain in the presented results, and comments on uncertainties and limitations remains limited. Therefore, I suggest the authors to be less certain and take more reservations to their own results, for instance on wake models performing “outstanding” compared to an simplified turbine representation in CFD. In my opinion, it is good scientific practice to be critical and outline limitations and uncertainties of ones results. It does not take away value, but builds confidence and facilitates scientific discussion.

## Unclear comments:

- Line 21: I think it is misleading to say that wind farms become increasingly less efficient as their size increase. First, many studies have shown how there is an equilibrium between power extracted by turbines and the entrained energy from the atmosphere, see e.g. [9]. Second, wind farm layouts have historically developed from aligned/rectangular layouts to curved and finally more misaligned and "random" layouts. The authors use Horns Rev for the second study, and this development in layouts is very clear in the three different generations of wind farms at Horns Rev, see figure 1. The figure is reproduced from [10], where it can also be seen (Table 2) how the capacity factor has increased historically for these wind farms.

The sentence has now been changed to: "One key issue is that wind farms that consist of many turbines are typically less efficient than smaller wind farms."

The sentence has been rephrased, but the meaning is the same: Larger wind farms are less efficient than small. This is still misleading and I think this statement requires a solid reference, as I have provided reference which shows the opposite and as commented in my previous review this is (to my knowledge) not general knowledge and stands as an unsupported statement.

- line 34: Wake steering is still not generally applied on commercial wind farms, despite growing scientific evidence. The main reason is that the uncertainties related to wake steering remains very high, i.e. it is not given that overall power output will increase, see for instance the review paper [11] or the benchmark paper [12]. I think the article would benefit for including such considerations at least in the motivation and discussion.

The relevant discussion in the introduction has now been changed to: "Demonstrations of performance gains in a number of computational (Fleming et al., 2015; Gebraad et al., 2016), experimental (Adaramola and Krogstad, 2011; Campagnolo et al., 2016; Bastankhah and Porté-Agel, 2019) and field (Fleming et al., 2017; Howland et al., 2019; Fleming et al., 2019; Simley et al., 2021) studies have prompted the consideration of implementing wake steering in commercial wind plants (see, for instance, a press release by WindESCo (2023)). Nevertheless, further research is required in order to reduce the uncertainties surrounding its potential benefits (Kheirabadi and Nagamune, 2019)."

OK.

- line 55: RANS is fully capable of modeling wake-to-wake interaction.

The revised sentence now reads: "However, the unsteady wake dynamics and the atmosphere-to-wake interactions, which play a critical role in large wind farm flows, cannot be appropriately accounted for."

OK.

- Figure 6 and text: I find the text unclear related to the figure. First, in line 215 it says that 70% of the designs found by LES-BO produce more power than the designs found by FLORIS. Looking at Figure 6, LES-BO (black line) is always above FLORIS (red line), i.e. 100%. The 70% seems to be that the combination of LF/LES outperforms LES-BO. Line 219 is also unclear as to which wake model outperforms LES-BO, but it appears to be LF/LES. It is not clear in the figure, but have FLORIS and LF/LES been sorted independently or does design "n" of FLORIS correspond

to design "n" of LF/LES? If not, it would be interesting to see a correlation plot of the two.

The sentence in question has been rephrased to make this point clear to the reader: "First, the proposed LES-BO framework is capable of finding a design that produces more power than  $\approx 70\%$  of the optimal designs suggested by FLORIS (as evaluated with LES)". The layouts have been sorted from best to worst based on the LF/LES data (this is now clarified in the revised manuscript). Figure 6 has also been updated, as we had erroneously used a sorted variant of the matrix containing the FLORIS data in the production of the figure. As noted in the revised manuscript, it may now be observed that though qualitatively similar, there is no one-to-one correspondence between the predictions of the two flow models.

Thanks for updating the sorting. I think it provides very interesting insights as to how well the simple wake model actually performs. .

- Section 4: The two studies are somewhat disjointed, and could perhaps even have been clearer in two independent articles. Why not optimize for yaw angle on the optimal wind farm layout of Section 3?

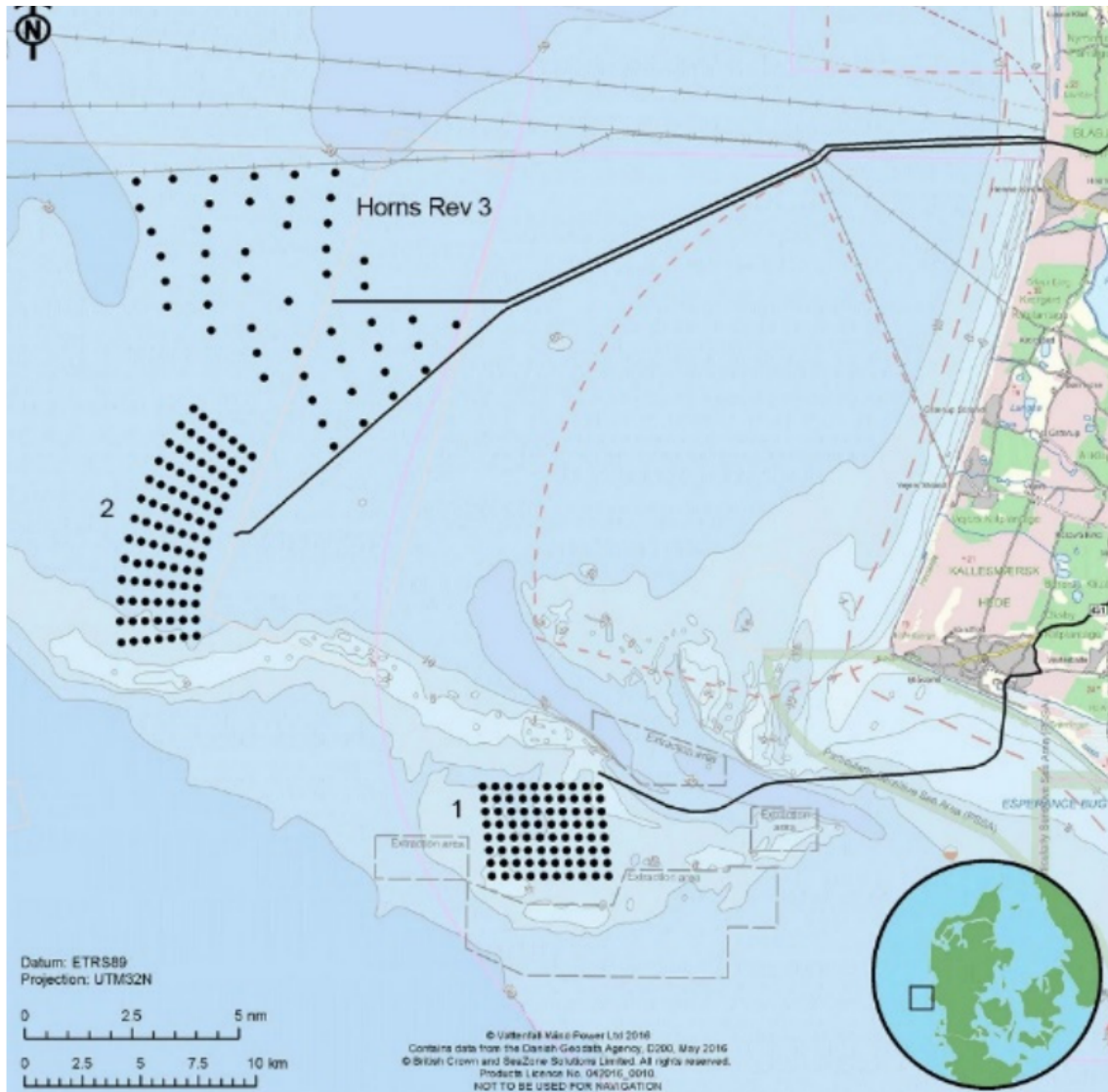


Figure 1: Layout of three generations of wind farms at Horns Rev.

The wake steering optimisation study was selected because the particular scenario showed very large sensitivity when optimised with wake models (see, for example, [13, 6]). Attempts at sequentially or jointly optimising farm layout and wake steering with the framework proposed in this work will be made in future work.

OK.

## Minor Comments and Technical Corrections:

- Figure 5: Please define "f" (y-axis)

f is now defined in the caption of figure 5.

"f" is now defined as "the standardised overall farm power output", but this is not a standard definition nor is it clear. Please provide a definition in terms of an equation how "f" is computed.

- Figure 9: Please add turbine numbers.

Done.

OK.

- Figure 10: What does the hatched bars indicate?

The hatched bars indicate the efficiency of the design suggested by the multifidelity approach (labelled EI-LES-BO, see also the legend in Figure 10).

My point was that only two of the six EI-LES-BO bars are hatched. What is the difference between hatched and non-hatched bars? If no difference, please provide consistent plotting.

Additional comments:

- Line 179: Boundary conditions of the simulation setup is detailed in both Sections 2.2 and 3.1. I suggest to move the LES description to Section 2.2.

- Figure 2: I'm not familiar with how precisely regulators define areas and if it is based on location of towers or full extend of rotor, but it is clearly visible in Figure 2 that the rotor of the top turbine extends outside of the available area. Please comment.

- Figure 7: Please extend the y-axis to cover the full range so readers can assess increase in efficiencies above 100.

## References

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