

Response to Reviewers

Matteo Baricchio, Daan van der Hoek, Tim Dammann, Jenna Iori,
Pieter M.O. Gebraad and Jan-Willem van Wingerden

Before responding, we would like to thank the reviewers for the time spent on reading our work and we highly appreciate all the comments that have helped us to improve the quality of the manuscript.

Reviewers' comments are included in **blue** and our response is shown in black. The main modifications/additions made in the paper are included in this document in **magenta**.

Response to Reviewer 1

This study uses an engineering wake model tuned from large eddy simulations (LES) to assess the benefit of using both wake steering and helix control on a turbine-by-turbine basis, allowing a turbine to actuate one of the methods at any given time. An optimization algorithm was developed to determine the optimal strategy and its parameters, and this was used to make predictions for a turbine pair and a real-world wind farm layout. Results show that a combined optimization strategy is worth pursuing because the benefits outweigh either strategy on their own.

Comments

R1.1. Why is wake steering considered quasi-static? Don't yaw misalignments need to be dynamic as wind conditions change?

The terms used in this manuscript follows the classification introduced by Meyers et al. (2022), for which a quasi-static strategy is described as follows: *"changes turbine set-points at a relatively slow pace, adapting them to background meteorological variations (changes in wind direction and/or wind speed over the day) but does not react to physical details that happen at time scales that are significantly faster than the farm flow through time."*

R1.2. It could be argued that wake steering is still not very mature, as it is rarely applied on real world wind farms.

The term mature has been replaced with "technology readiness level" and retained only when used in a relative context to compare wake steering with active wake mixing technique.

Line 24: **Conversely, dynamic WFFC techniques have not yet achieved a similar technology readiness level.**

Line 54: **This is due not only to the lower technology readiness level of these methods but also to the inherent difficulty of capturing their dynamic effects using steady-state models.**

R1.3. Perhaps the title would benefit from noting that the helix method was used, not the more general active wake mixing concept, which would include the pulse method. "Combining wake steering and helix wake mixing on a large-scale wind farm". The title could also benefit from clarification that this is a modeling study using an engineering wake model, not LES or an experimental study.

Based both on this comment and R2.6 we have changed the title to: **"Multi-strategy wind farm control: alternating wake steering and helix wake mixing on a large-scale wind farm"**. We have decided not to include the suggestion of adding "engineering wake models" to avoid an excessively long title and

to improve accessibility for a broader audience, for whom we do not expect familiarity with these terms.

R1.4. It would be helpful to clarify exactly which version of PyWake was used to perform the calculations.

The version number has been added at line 110: ...PyWake software (version 2.6.11)....

R1.5. Can this model account for nonuniformity in wind speed and direction across the farm on the quasi-steady (~ 10 minute) timescale?

This model is based on PyWake, which can account for both spatial nonuniformity in wind speed and wind direction. However, in this study we consider only wind speed, as shown in Fig. 2. The spatial nonuniformity in wind direction has not been included due to a lack of available data, but it can be readily incorporated within the same framework once such data become available.

R1.6. Please comment on the generality of the results taken at a constant 4% turbulence intensity. How realistic is this? If the gains are nonlinear with respect to TI, perhaps simulating more of the actual distribution is necessary.

The reviewer raises a valid point regarding the generality of results obtained at a fixed turbulence intensity of 4%. According to Türk and Emeis (2010), average offshore turbulence levels in the North Sea typically range between 4% to 6%, placing our simulated TI on the lower end of realistic conditions. The TI in our simulations is not set directly, but results from a precursor simulation of a conventionally neutral boundary layer with a surface roughness typical for offshore conditions of $z_0 = 0.001$ m. This yields approximately 4% TI at hub height. While this value is physically consistent with the chosen boundary layer configuration, we acknowledge that it does not represent the full range of conditions encountered in practice. However, simulating additional turbulence scenarios is not trivial in our current high-fidelity simulation setup. The lower TI is known to be associated with larger wake deficits and slower wake recovery, meaning our results likely represent a scenario where wake losses, and hence the potential gains from wind farm flow control, are somewhat overestimated relative to higher-TI conditions. We have added a sentence to the discussion section reflecting on this limitation.

Line 483: The turbulence intensity is also expected to play a major role in comparing and combining different WFFC techniques. In this study, the LES data used to tune the engineering wake model were run with an ambient TI of 4%, which lies toward the lower bound of the typical range of 4–6% observed in the North Sea (Türk and Emeis, 2010). Lower TI is generally associated with larger wake deficits and slower wake recovery. Consequently, this may lead to a slight overestimation of the AEP gains achieved by WFFC strategies.

R1.7. Please comment on how the wind direction uncertainty used for simulation models either natural variability on timescales shorter than the control system reaction time or sensor error. Does it model either source of uncertainty? If so, why are they equivalent? For example, sensor bias may be much more detrimental than noise or other random error. Can or should deficiencies be separated out by category? What is a typical value for a real-world wind farm?

We assume that the distribution of wind direction variations from the nominal value follows a Gaussian distribution centered on the nominal wind direction. This assumption is based on a widely used approach for yaw optimization under wind direction uncertainty (Quick et al., 2017; Rott et al., 2018). We have now clarified in the text that these wind direction variations may reflect both short-timescale variability and sensor errors, in line with the reviewer’s suggestion.

Line 141: Based on this definition, σ_θ indicates both the variability of wind direction on timescales shorter than the control system reaction time or uncertainty due to sensor errors.

The main assumption is that the distribution is centered on the nominal wind direction value, which prevents us from accounting for sensor bias, for example by using a distribution that is not centered

on the nominal wind direction. A more detailed investigation of sophisticated methods to account for wind direction uncertainty and variability could provide valuable additional insights; however, we consider this to be beyond the scope of the present study.

We have added additional references to provide typical values of σ_θ encountered in wind farms.

Line 227: In this work, three values of σ_θ are used to test the effectiveness of the WFFC strategies under various conditions: 0° , i.e. no uncertainty, 2.5° , and 5° . These choices are based on values reported in previous studies. For instance, Gaumond et al. (2014) obtained a $\sigma_\theta = 2.67^\circ$ from 10-min interval in the Horns Rev wind farm. Mittelmeier et al. (2017) extracted a $\sigma_\theta = 3.6^\circ$ from wind turbine sensor data. Quick et al. (2017) adopted $\sigma_\theta = 5^\circ$ in their work and (Rott et al., 2018) reported values around 5.25° .

R1.8. Tuning of the engineering wake model may be worth including as a major section in the paper rather than an appendix. Perhaps include some information regarding the uncertainties of the values derived from the calibration process as well. LES and OpenFAST are not observations of the absolute truth of course. How might these uncertainties propagate through to the final AEP gain calculations?

Based on this comment and R2.3, we have decided to include a new section: **3.1 Model validation**. We do not reproduce it here in full in order to limit the length of the rebuttal. In this section, we compare the results obtained from the engineering wake model with the LES data in terms of power gains for the different control strategies. We believe this is sufficient to quantify the model accuracy after the calibration process and, consequently, to assess the uncertainty introduced at this step.

Moreover, we have extended the discussion on the reliability of low-fidelity models, highlighting the effect of uncertainty propagation on AEP gains and the need for experimental validation.

Line 445: In conclusion, the application of the engineering wake model in this study extends beyond the conditions for which it has been originally tuned. This may lead to some uncertainty in the reported AEP gains obtained with this method; however, we do not expect this to affect the main trends observed in the present work. To increase the reliability of results from WFFC on large-scale wind farms, the scale of the LES used for validation should be extended from a few turbines to larger wind farms to investigate deep-array effects and avoid extrapolation beyond the validated conditions. Lastly, since LES is a numerical modeling approach, experimental field data are essential to improve the reliability of low-fidelity models in representing wake mixing effects and to validate the effectiveness of active wake mixing strategies on large-scale wind farms.

R1.9. Sharing code and data is much appreciated. Please include in the README the steps that must be taken with the repository to compute the EWM calibration values, compute the AEP at all the discretized points, compute the integrated AEP, and generate the figures and numerical results. The data should be stored in an open format like CSV, HDF5, or Parquet, as Python Pickle files can represent security risks. Lastly, before publication please deposit the compendium in a long term archival service like Figshare or Zenodo and provide the digital object identifier (DOI) in the paper references.

We have followed the reviewer’s suggestions by providing additional information in the online repository and converting the data format. These are available in the folder `case/HKNscaled` of the main repository. Both `.pkl` and `.csv` are kept to facilitate fast data extraction and prevent the security issues mentioned by the reviewer. We have also uploaded our code to Zenodo, and the DOI is now available.

Minor comments

1. Line 29: "the pulse.": Is this a typo? Should it read "the pulse technique" or "the pulse method"? We have added the term "technique".

2. Line 87: "the helix": Similarly, should this read "the helix method" or "helix mixing"? The term "method" has now been added here and in several other parts of the manuscript.
3. Line 34: "static" should be "quasi-static"? We have corrected it to "quasi-static".
4. Line 376: "as a results": Should be "as a result". We have corrected this.
5. Line 384: "performing the helix": Should be "operating in helix actuation mode" or similar. We have added the term "method".
6. Section 2.5 could be named "Cases studied". We prefer to keep "Case studies" as title of Sect. 2.5, as it is a widely used standard for section similar to Sect. 2.5 in this manuscript.
7. Why are the COT values doubly negative in figure 9? The values on the y-axis are negative, and the axis is labeled " $-\text{COT} [\%]$ ", indicating that these values correspond to the opposite of COT, e.g. -50 in the plot indicates $\text{COT} = 50\%$. Since the multi-objective problem solved here is a max-min optimization problem, the choice of inverting the y-axis is made to highlight the trade-off solutions at the "top-right knee" of each curve. This is common practice in the visualization of Pareto fronts, which are typically plotted using either max-max or min-min metrics.
8. Line 453: "The helix" used again with no additional nouns like "method" or "approach". "Helix control" could also be used in lieu of "the helix". This specific line has been removed in the revised manuscript, and the term "helix" has been replaced by "helix method" throughout the text.

Response to Reviewer 2

The manuscript presents a valuable and as far as I am aware unique analysis of the benefits of applying a combination of wake steering and wake mixing on turbines in a large-scale wind farm. The analysis is executed using a low-fidelity engineering model, which is both the strength and the weakness of this work.

The manuscript is a valuable addition to existing research in the field of WFFC. However, I believe the manuscript in its current form has some significant shortcomings that need to be addressed before publication.

I was torn between recommending minor or major revisions, so please do not interpret my final choice to mean that I do not like the manuscript. I actually really liked the manuscript, which is why I wrote such an extensive review. I believe this manuscript is good work that has the potential to be great work if some of the shortcomings are addressed. I do not doubt the authors' ability to address my comments though, and when they do, I will gladly recommend this manuscript to be accepted.

We sincerely appreciate the feedback and the time and effort dedicated to this review. We believe that these suggestions have significantly improved the quality of this work. As detailed in our responses to each comment below, several additions have been incorporated into the revised manuscript. While preparing this revision, we aimed to keep the overall length of the paper within reasonable limits to ensure clarity and readability. Consequently, although most suggestions have been addressed in the manuscript, some are discussed only within this rebuttal.

First and foremost, I believe that the fidelity of the engineering model substantially puts into question the reliability of the final results. I have the following concerns with the engineering model that I feel need to be addressed or at least mentioned:

R2.1. The model is used (far) outside of the space it is tuned for. As far as I can tell, the model is tuned for one specific boundary layer condition, one wind speed, one TI percentage, and only for up to three wind turbines with only one specific spacing. It is specifically NOT tuned for partial wake overlap, deeper arrays, or control actions implemented on downstream (waked) turbines. However, the case study does involve all of these conditions. This daisy chain of extrapolations makes that the uncertainty of the model, which is not quantified in any way in the paper, likely far exceeds the reported AEP gains for the case study wind farm. The fact that the model was tuned without any of these dynamics should explicitly be mentioned throughout the paper.

As noted by the reviewer, the model tuning has its limitations. Specifically, it is based on a specific atmospheric condition, namely a single boundary layer, wind speed, and ambient TI value. However, the other cases mentioned by the reviewer, i.e. partial wake overlap, control action on a downstream turbine, and deep arrays, have been (partially) considered.

We have now highlighted the limitations of this tuning process in Sect. 4.2, as well as its impact on uncertainty propagation with respect to the AEP gain values.

Line 433: However, due to the significant computational resources required for LES, this study considers only a limited set of conditions for the model calibration and validation. These are restricted to a single atmospheric boundary layer condition and to a fixed value of the free-stream wind speed and the turbulence intensity. More importantly, the analysis is limited to configurations with up to three turbines at fixed spacing.

Line 445: In conclusion, the application of the engineering wake model in this study extends beyond the conditions for which it has been originally tuned. This may lead to some uncertainty in the reported AEP gains obtained with this method; however, we do not expect this to affect the main trends observed in the present work.

We address below how the additional cases raised by the reviewer have been considered:

- Partial wake overlap: This is implicitly accounted for by tuning the model across different cross-stream positions. Specifically, the model is calibrated to match the cubic wind speed values of the flow field data obtained from the LES at discretized streamwise and cross-stream locations, with the latter ranging from $dy = -2D$ to $dy = 2D$. In practical terms, the flow field can be rotor-averaged at any of these positions and interpreted as the effective wind speed experienced by a virtual turbine. This aspect has been clarified in the manuscript with the following addition.

Line 594: **Considering different cross-stream positions enables us to calibrate the model for both cases of full alignment and partial wake overlap. While the streamwise discretization of the flow field depends on each case, the cross-stream bounds are set to $[-2D, 2D]$.**

- Control action on downstream (waked) turbine: This aspect is considered for wake steering, as described in Table A2 (simulation no. 11), but is neglected for the helix method. For the latter, this limitation, together with the synchronization issue, is now discussed in Sect. 4.2.
- Deep arrays: The LES used in this study includes up to three turbines, due to the prohibitive computational cost of large-scale LES. This constitutes a limitation, as the model is applied beyond the conditions for which it has been calibrated. However, this is a common assumption in studies involving engineering wake models. As also noted in response to comment R2.4, this limitation is now stated more explicitly in Sect. 4.2 of the revised manuscript.

R2.2. I know neutral boundary layer conditions are common in the North Sea, but it is a significant simplification to consider this representative of the year-round conditions in the case study wind farm. The same is true for the wind speed and turbulence levels. I can see how 10 m/s and 3-6% might be average for the North Sea, but “average” is not the same as “representative”. I recommend adding a Weibull distribution of the wind speed measured at the wind farm, accompanied by the TI range for each wind speed bin. This will likely show that the TI is generally higher at low wind speeds, when WFFC in general, and the helix specifically, is most effective. Therefore, by tuning for a relatively low TI, you might overestimate the benefit of the helix, which should be mentioned. You do say you use the Weibull distribution to calculate the AEP, so regardless of this point, I feel like a figure of the distribution should be added to the paper.

The reviewer raises a valid concern regarding the choice of ambient TI. Please refer to our response to comment R1.6, where we provide additional explanation and describe how this aspect has been incorporated into the revised manuscript.

The Weibull distribution for wind speed is implicitly included in the wind rose plot of Fig. 1, which combines the probability distributions of wind speed and direction. The Weibull distribution at hub height and at the location of the substation is shown in Fig. R1. This distribution differs for each spatial location as a consequence of the heterogeneity of the wind resource, as described in Fig. 2. We believe that the wind rose, together with the map shown in Fig. 2, provides sufficient information about the wind resource of the site studied here. Therefore, given our aim to keep the manuscript at a reasonable length, we prefer not to include this plot in the manuscript to leave more space to the additional plots discussed in the following comments.

Due to the unavailability of the TI data, we were not able to generate the plot suggested by the reviewer. However, a similar analysis is presented by Türk and Emeis (2010), where it is shown that TI is indeed higher at lower wind speeds. Median TI values are observed to lie within the range of 4–6% for wind speeds above 5 m s^{-1} .

R2.3. Currently, the flow model validation is limited to two figures tucked away in the appendix. This should be a major part of the paper, as the reader needs to be able to assess the fidelity of the model to put any trust in the reported findings. The results in Section 3.1, using the 2-turbine layout, are perfectly suited to compare the engineering model with LES data. You should be able to produce LES equivalents of Figure 4 at different control settings (using rotor-averaged wind speeds) to compare with the model. This is the “simplest” case that is basically exactly what you tuned your model for, so this provides a floor on the error that should be expected for the wind farm case. I believe this needs to

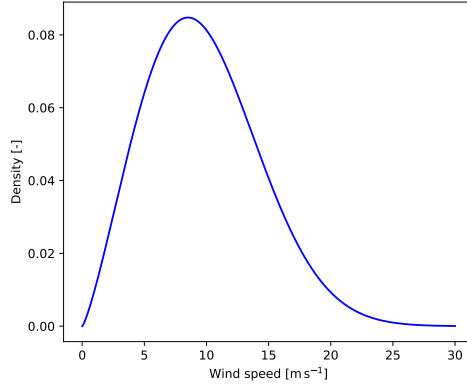


Figure R1: Weibull distribution at the substation location and turbine hub height

be added to the revised manuscript.

We thank the reviewer for this valuable suggestion. In response, we have conducted an additional analysis to address the comment. This analysis is structured into two parts: a validation of the model based on the LES results, and a sensitivity analysis examining the impact of the chosen engineering wake model and the power–yaw loss exponent. This is followed by a general conclusion, along with a summary of the corresponding modifications made to the manuscript.

Model validation

Two different cases are used to compare the engineering wake model with the LES, as suggested by this comment. First, a two-turbine array with varying downstream turbine positions is considered, replicating the approach adopted in Sect. 3.2 (previously Sect. 3.1). In this case, the power gain is computed based on rotor-averaged effective wind speeds extracted from the LES flow field. This setup is therefore used to validate the accuracy of the engineering wake model in predicting wake deficit and deflection, which are the aspects specifically re-tuned in this study. The results are shown in R2 (Fig. 3 the revised manuscript). Second, a three-turbine layout is analyzed, considering both full-alignment and partial-misalignment conditions. In this case, the results from the engineering wake model are directly compared with LES-derived power gains, thereby evaluating the full workflow of the engineering wake model for power estimation. These results are presented in Fig. 4 of the revised manuscript.

Overall, this analysis leads to two main observations. First, the engineering wake model provides a good approximation of the flow field obtained from the LES. Consequently, the power gains computed using the same method (i.e. the same rotor-averaging model and power curve) applied to the effective wind speeds from the LES and from the engineering wake model show good agreement. Figure R4 provides a broader overview of this comparison, supporting this conclusion. Second, the comparison between power gains obtained directly from the LES and from the engineering wake model (i.e. using different rotor-averaging models and power curves) shows the same trend but a slight mismatch.

From these two observations, we conclude that the mismatch between the LES and the engineering wake model is mainly caused by the models used to compute the rotor-averaged wind speed and to estimate the power drop at the yaw-actuated turbines. These models were not re-tuned in this study and are widely used in the literature.

Sensitivity analysis

To support the conclusions drawn in the previous section, we provide two additional analyses. First, two widely used wake deficit models are added to the plots in Fig. 4 of the revised manuscript and shown here in Fig. R5. These are the models developed by Bastankhah and Porté-Agel (2014) and

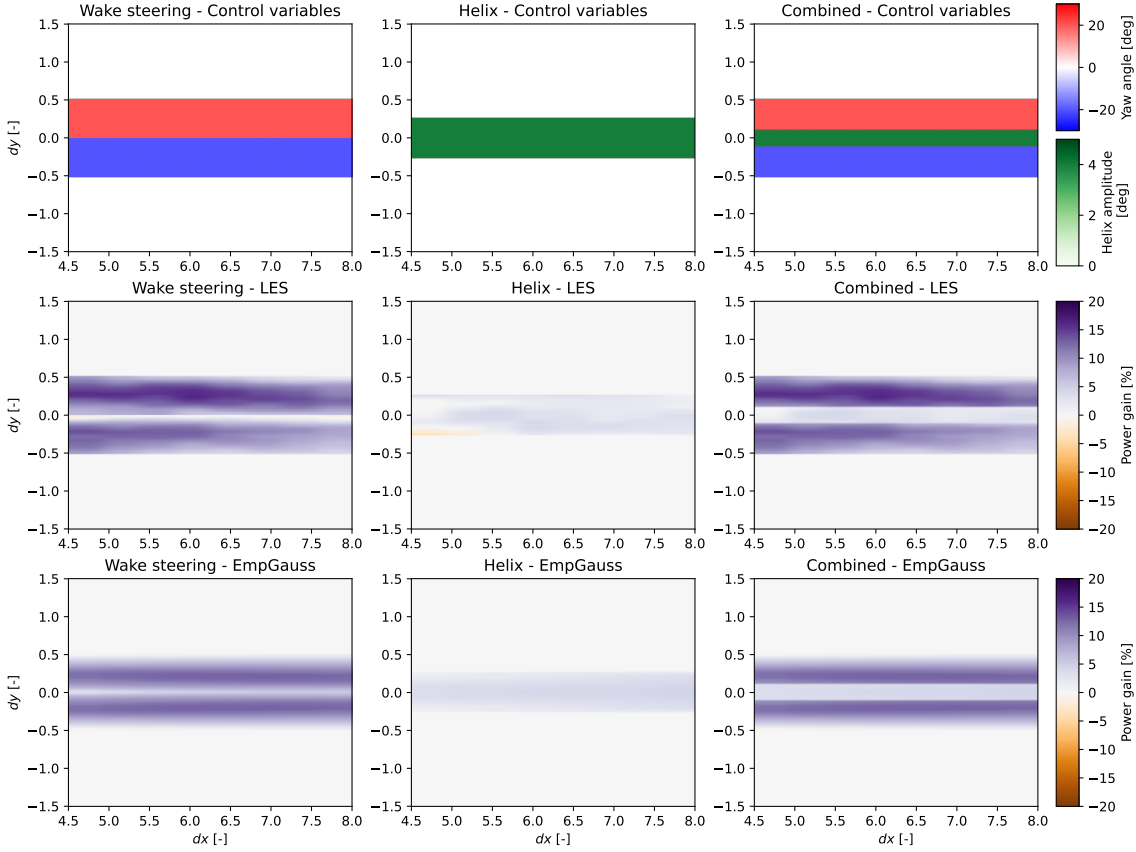
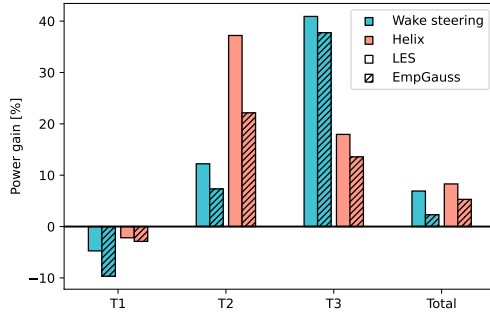


Figure R2: (Fig.3 in the revised manuscript) Comparison between power gains obtained from the LES (second row) and the engineering wake model (third row) for a two-turbine wind farm. The control setpoint of the upstream turbine (shown in the first row) varies with the position of the downstream turbine, expressed in terms of streamwise and cross-stream distances from the upstream turbine, normalized with D . The wind speed and direction are 10 m s^{-1} and 270° , respectively.

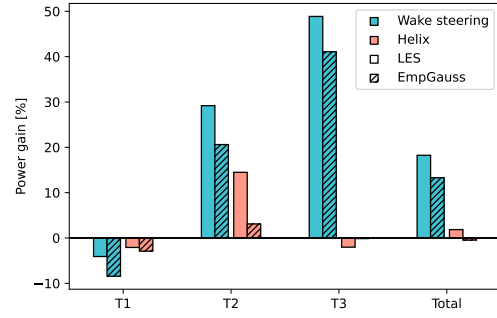
Zong and Porté-Agel (2020), which are available in PyWake. The results obtained with these models refer only to wake steering, since the helix strategy cannot be simulated with these models. It can be observed that the empirical Gaussian model adopted in this study performs similarly to, or even better than, these alternative models. Second, in Fig. R6 we investigate the impact of the model used to calculate the power reduction under yaw misalignment. As mentioned in Sect. 2.1, we have not modified the default PyWake implementation for this purpose, which estimates power by evaluating the power curve at the effective wind speed under yawed conditions. However, as noted in Sect. 4.2, this approach may lead to an overestimation of the power reduction. This is because, depending on wind speed, the implied power–yaw loss exponent ranges from 2.5 to 3.1, whereas other studies report lower values obtained with more sophisticated models. For instance, Liew et al. (2020) obtained values in the range 1.7–2.2, and Gebraad et al. (2016) used a value of 1.88 in their study. This overestimation is evident in Fig. 4 of the revised manuscript, where the power losses predicted by the engineering wake model are consistently larger than those observed in the LES for the yawed turbines (T1 and T2). To further investigate this issue, we recompute the power gains of each turbine using a power–yaw loss exponent of 1.88, and include the results in Fig. R6. It can be observed that this adjustment leads to a better agreement with the LES. Therefore, a significant fraction of the mismatch between the engineering wake model and the LES can be attributed to the choice of the power–yaw loss exponent.

Conclusion and modifications to the manuscript

We acknowledge that engineering wake models have inherent limitations, which are now more clearly highlighted in the revised manuscript. Nevertheless, they remain the only practical option for control



(a) Full alignment case



(b) Partial misalignment case

Figure R3: (Fig.4 of the revised manuscript) Comparison between the power gains obtained from the LES and from the engineering wake model of a three-turbine array at 10 m s^{-1} .

setpoint optimization and AEP calculations in large-scale wind farms. These limitations are consistent with those of widely used approaches in the literature and are therefore not specific to this study. Furthermore, the implementation closely follows the default PyWake configuration, with modifications introduced only when strictly necessary (e.g. to simulate the helix method), thereby relying on the established validation of these models.

A new section titled **3.1 Model validation** has been added to the manuscript, where the results depicted in Fig. R2 and R3 are presented and discussed. We do not reproduce it here in full to limit the length of the rebuttal. Conversely, the sensitivity analysis concerning the different engineering wake models and the power-yaw loss exponent is presented only in this rebuttal, in order to maintain a reasonable length for the revised manuscript.

R2.4. I understand that LES runs are timely and costly, so although I believe more simulations at different conditions are necessary to sufficiently tune the model, I respect that you have to deal with a limited amount of (computational) resources. However, this should be mentioned as a limitation of the study much more explicitly. It should be made (more) clear to the reader that the case study far exceeds the conditions for which the model was validated, and that the results should be interpreted accordingly.

This aspect has now been highlighted both in the Discussion (Sect. 4.2) and in the Conclusion.

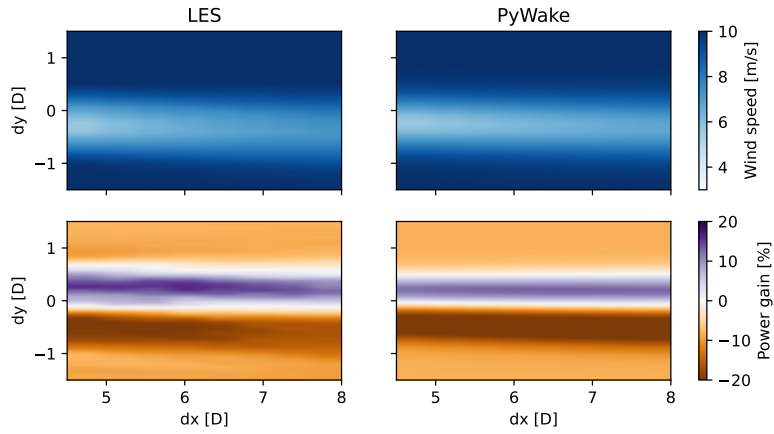
Line 445: In conclusion, the application of the engineering wake model in this study extends beyond the conditions for which it has been originally tuned. This may lead to some uncertainty in the reported AEP gains obtained with this method; however, we do not expect this to affect the main trends observed in the present work.

Line 535: However, these results are based on recent wake models used for wind farm simulation, which are associated with notable uncertainties, as they are applied beyond the range of conditions for which they have been validated.

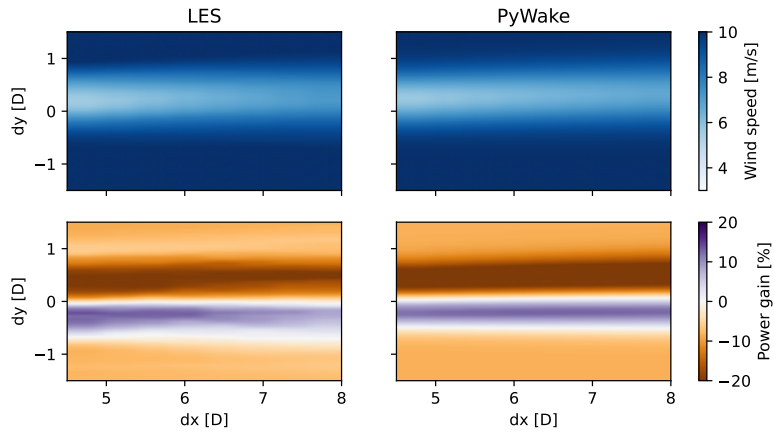
Other major comments:

R2.5. The quality of the written text is fair, not great. Many sentences do not flow naturally, the order of words is sometimes unintuitive, the interpunction is not always logical, and the choice of words is not always fluent (and this is coming from a non-native English speaker). In my opinion, it is good enough, the manuscript is certainly understandable in its current form, but I would personally strive for a higher standard. I therefore recommend having the manuscript reviewed by a professional editor or a native English speaker.

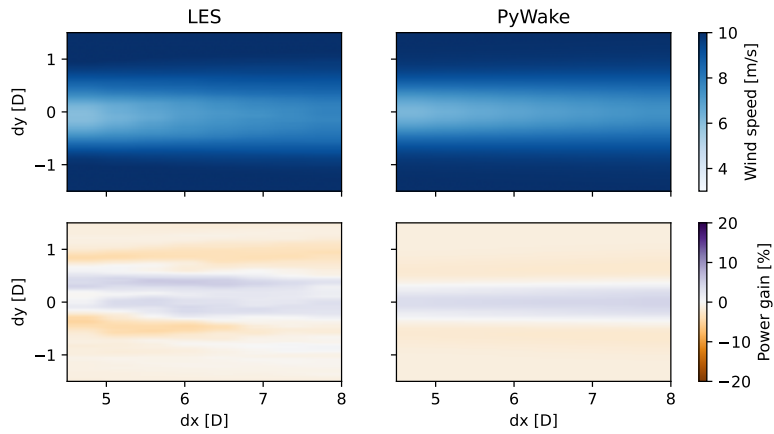
We have carefully reviewed the manuscript to correct typos and improve syntax.



(a) Wake steering with $\gamma = 20^\circ$



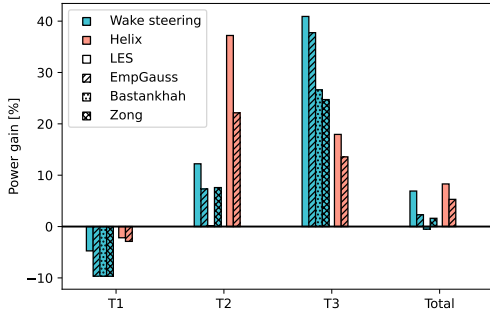
(b) Wake steering with $\gamma = -20^\circ$



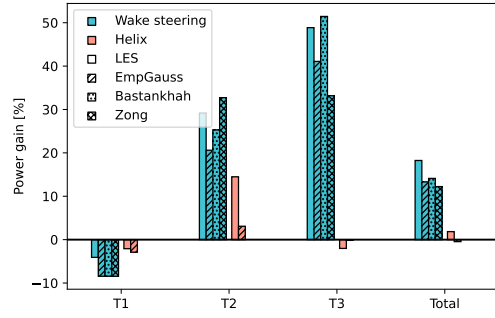
(c) Helix method with $\beta = 4^\circ$

Figure R4: Flow field and resulting power gain comparison between engineering wake model and LES for a two-turbine array with varying position of the downstream turbine, indicated by dx and dy . The control setpoint of the upstream turbine is specified in each sub-caption.

R2.6. I would suggest changing the title of the paper to improve expectations. When I first read the title, I assumed 1) that wake steering and wake mixing were going to be applied on turbines simultaneously (about which I was skeptical), and 2) that different wake mixing strategies would be studied. I quickly learned that neither was the case, but I would suggest changing the title so that it better

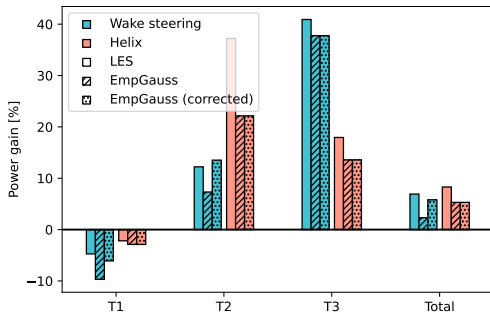


(a) Full alignment case

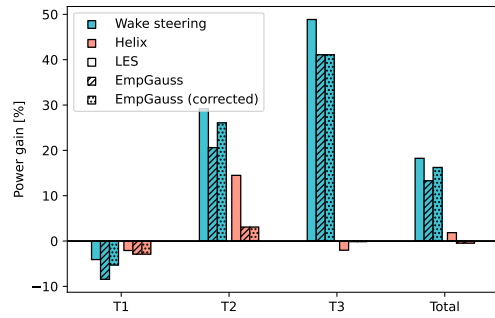


(b) Partial misalignment case

Figure R5: Comparison between the power gains obtained from the LES and from the engineering wake model of a three-turbine array at 10 m s^{-1} . The impact of other widely-used wake deficit model is shown.



(a) Full alignment case



(b) Partial misalignment case

Figure R6: Comparison between the power gains obtained from the LES and from the engineering wake model of a three-turbine array at 10 m s^{-1} . The impact of the power-yaw loss exponent correction is shown.

reflects this and better guides the expectations of the reader.

We acknowledge that the previous title may not have been precise enough. Based on this comment and R1.3, we have changed the title to: “Multi-strategy wind farm control: alternating wake steering and helix wake mixing on a large-scale wind farm”. We believe that replacing “combining” with “alternating” and “active wake mixing” with “helix wake mixing” clarifies the issues raised by the reviewer.

R2.7. Why do you use the IEA 22MW turbine? Unless my Googling skills betray me, the case study wind farm HKN is equipped with 11MW turbines. However, you seem to conveniently leave this out of your manuscript. Wouldn't it make much more sense to use, for example, the DTU 10MW reference turbine (or even the IEA 15MW turbine), as it is much closer in size to the actual wind turbines used? I assume you had a practical reason to use this specific reference turbine, which is understandable, but should be justified. The subsequent scaling of the whole farm adds another layer of uncertainty to the already highly uncertain final conclusions of the paper. Did you also scale the wind speed to correspond to the same point on the turbine wind speed-power curve?

The reviewer raises a valid point. The motivation for our choice is that we adopt the same case study used in several studies within the SUDOCO project. This enables direct comparison with, and connection to, upcoming publications related to this project. SUDOCO aims to develop the “Control Room of the Future”; therefore, the case studies were defined to reflect the current trend of rapidly increasing turbine sizes. Overall, we believe that this choice of case study does not affect the main conclusions of the work, since most models are normalized with respect to the turbine diameter, which is also used to scale turbine positions, thereby preserving normalized distances. This procedure is described in

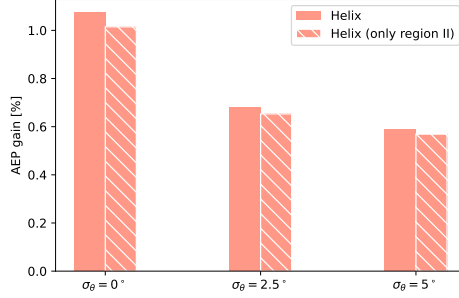


Figure R7: Impact of the assumption of helix operation for above-rated conditions.

Sect. 2.5.

In the initial version of the manuscript, the wind resource distribution was not adjusted to the hub height of the IEA 22 MW turbine. Based on this comment, we have implemented this correction by adapting the distribution to the corresponding hub height, in order to improve the realism of the case study, following the reviewer’s suggestion. This results in updated scale parameters for the Weibull distributions used in this study. For this purpose, we assume a power-law exponent of 0.1, which is typical for offshore conditions. This aspect has been mentioned in the revised manuscript. As a consequence, we obtain slightly lower AEP gains for all wind farm control strategies. This is due to higher wind speeds experienced at the increased hub height, leading to a reduced fraction of operation in the below-rated region, where wind farm control is most beneficial. The AEP gain values have been updated throughout the manuscript. Nevertheless, all trends and conclusions remain unchanged.

Line 217: The wind resources of the site are defined by the wind rose included in Fig. 1 and the heterogeneous wind speed field shown in Fig. 2 (Vortex FDC, 2024), which are scaled to the turbine hub height using a power law exponent equal to 0.1.

R2.8. You do not mention whether controller behaviour changes between region II and III. I therefore assume it doesn’t. However, up until now, as well as in your LES cases, the helix approach is only studied in region II. If your study does not limit helix to region II, you should mention this as an additional degree of uncertainty.

In this work, the operation of both control strategies is not explicitly limited to Region II; however, based on the wind farm model adopted here (described in Sect. 2.1), operation above rated conditions is unfavorable in most cases. Therefore, the optimization of the control setpoints yields $\gamma = 0$ and $\beta = 0$ for most conditions above rated wind speeds. Although cases with $\gamma \neq 0$ or $\beta \neq 0$ may occur when the effective wind speed exceeds the rated value, we expect these instances to have a negligible impact on the overall AEP gains obtained with these WFFC strategies.

We acknowledge that this assumption may not be fully realistic, particularly for helix operation, which remains an unexplored research topic. Therefore, we have investigated its impact by disabling helix operation when the effective wind speed at a turbine exceeds the rated value. The resulting AEP gains are shown in Fig. R7, where it can be observed that this assumption has a minimal impact on the final values. This aspect has been mentioned in the revised manuscript.

Line 441: Another limitation is that the operation of the WFFC strategies is not explicitly constrained in the below-rated region, where they are typically tested or simulated using higher-fidelity models. However, as shown in the LUTs in Fig. 10, this operation occurs only in a limited number of cases. Moreover, for the helix method, it has been verified that this assumption yields negligible differences compared to the presented results.

R2.9. I really like the approach taken to consider the uncertainty in wind direction using probability

distribution. I think this might be the most valuable contribution of this paper. However, I do have a few smaller remarks with respect to this approach:

R2.9.1 I assume this uncertainty does not propagate downstream, is that correct? In other words, you only consider unintentional misalignment for turbine power production, not for how it might affect the wake (unintentional wake steering). I understand that including this would be much more complicated, so I do not expect you to add that, but I think it is worth mentioning as this could further affect the performance of WFFC, specifically wake steering.

In this study, wind direction uncertainty is modeled by varying the wind direction across the entire farm while keeping the absolute turbine orientation fixed. As a consequence, since yaw angles are defined as the difference between turbine orientation and wind direction, they are also affected. This approach is represented through the wind direction deviation $\Delta\theta$ in Eq. 3 of the manuscript. Therefore, the effect does propagate downstream. Overall, this method is based on previous studies in the literature (Rott et al., 2018; Simley et al., 2020).

R2.9.2 Did you consider using a non-zero mean for the probability distribution? I believe that sensor bias is a common issue for wind turbine anemometers, so this could be relevant to study. How might that affect your findings?

We model wind direction uncertainty by assuming that deviations from the nominal wind direction follow a Gaussian distribution centered on the nominal value. Consequently, non-zero mean values that could represent sensor bias are not considered in this study. We acknowledge that this is a relevant aspect that could lead to interesting insights; however, we consider it beyond the scope of the present work.

R2.9.3 Why did you choose the uncertainty levels of 2.5 and 5 degrees, if literature shows that values of up to 5.25 degrees are observed in the field?

In the literature, we found values of σ_θ ranging from 2.67° to 5.25°. Therefore, we believe that our choice provides a reasonable range of realistic conditions. We have now included additional sources in the revised manuscript.

Line 228: These choices are based on values reported in previous studies. For instance, Gaumond et al. (2014) obtained a $\sigma_\theta = 2.67^\circ$ from 10-min interval in the Horns Rev wind farm. Mittelmeier et al. (2017) extracted a $\sigma_\theta = 3.6^\circ$ from wind turbine sensor data. Quick et al. (2017) adopted $\sigma_\theta = 5^\circ$ in their work and (Rott et al., 2018) reported values around 5.25°.

R2.9.4 Finally, and this is my only major objection to the method used, you assume different levels of uncertainty, but at the same time, you also assume perfect knowledge of the level of uncertainty when choosing the control actions. However, in a real wind farm, you do not know the level of uncertainty a priori, and it usually changes over time. How would your results change if you do NOT have perfect knowledge of the uncertainty? I would really like to see a section added to the paper where you use the LUT for 0 degree uncertainty, but then implement it on the case with 5 degree uncertainty, and compare that to the case where you had perfect knowledge (for both the 2T and the HKN case). I feel like this better represents how WFFC is implemented on real wind farms, and requires only a limited additional simulation effort. This would in my opinion better show the effect of wind direction uncertainty on AEP than your current approach.

We greatly appreciate this suggestion from the reviewer. We have added a paragraph in the revised manuscript addressing this topic, with the results reported in Fig. 9 (also included here as Fig. R8). Overall, we observe that the helix strategy demonstrates higher robustness than wake steering even when the uncertainty level is unknown, confirming the main trends of this study.

Line 330: Up to this point, the same values of σ_θ have been adopted for both the optimization of the control variables and the evaluation of the strategy, assuming perfect knowledge of the wind direction

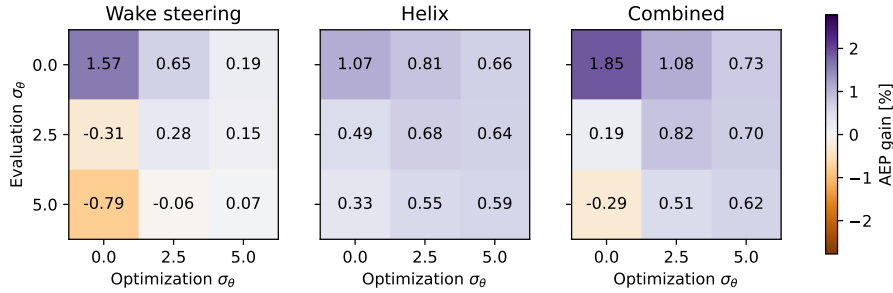


Figure R8: (Fig. 9 in the revised manuscript) Cross-comparison of the AEP gains obtained for different σ_θ used for the optimization of the control variables and the evaluation of the strategy.

uncertainty. However, since in practice the values of σ_θ can be difficult to predict, this assumption is unlikely to be satisfied in reality. Therefore, a cross-comparison of the AEP gains obtained with different σ_θ used during optimization and evaluation enables us to assess the robustness of the different WFFC strategies under more realistic scenarios. These results are shown in Fig. 9. It can be observed that wake steering is highly affected by a mismatch between the σ_θ values. Overpredicting σ_θ significantly decreases the achievable AEP gains, while an underprediction even results in negative values. The helix strategy exhibits higher robustness with respect to an incorrect prediction of σ_θ , registering positive AEP gains for all the considered cases. Lastly, the robustness of the combined strategy lies between those of the two individual strategies.

In the revised manuscript, we address these topics only for the HKN case study due to the substantial additional space that would be required to extend the analysis to the two-turbine example as well. However, we report the results for the latter case study here. Specifically, the power gains shown in Fig. 6 have been recomputed via a cross-comparison between different values of σ_θ used for optimization and evaluation. The results are shown in Fig. R15–R17, included at the end of this rebuttal. It can be observed that, as the value of σ_θ used in the evaluation increases, wake steering cases are strongly penalized when the turbines are (nearly) aligned. In contrast, the helix strategy shows greater robustness.

R2.10. Given all the uncertainty regarding the reliability of the AEP gains as addressed in multiple points above, I would seriously consider not mentioning the specific percentages at all. I think it would be better to stick to the more robust RELATIVE conclusions that wake steering works better with no WD uncertainty, helix with WD uncertainty, and a combination of both works best. That is in my opinion still a valuable finding, that is much less likely to be rebuked by future research using a better / more thorough methodology. At the very least, I would remove the percentage claims from the abstract and conclusions sections, or add lengthy caveats mentioning all the layers of uncertainty surrounding these numbers.

We acknowledge the reviewer’s concern and have followed the suggestion of removing the specific percentages from the Abstract and Conclusion sections, focusing instead on the relative conclusions.

R2.11. I do not fully understand why wake steering performs as badly as it does in your simulations when WD uncertainty is considered. I would expect a more significant drop than for the helix approach, as is observed, but I hadn’t expected the difference to be this big. This is also supported by section 3.1, which is much more in line with my expectations. I think this surprising result, and discrepancy between a simple 2-turbine setup and a complex 69-turbine wind farm, deserves deeper digging into. If these results are to be believed, the simple 2- or 3-turbine setups commonly used in assessing WFFC are actually terribly bad at predicting the benefit of WFFC strategies. That would be a very significant finding. However, as the analysis of this discrepancy is limited in the current manuscript (and my limited belief in the fidelity of the model), I am not sure if I can fully trust these results.

The reviewer raises a fair point, as we also expected a slightly lower sensitivity of wake steering to wind

Table R1: Comparison with other studies on AEP gain drop due to wind direction uncertainty.

Publication	N. turbines	σ_θ	AEP gain ($\sigma_\theta = 0^\circ$)	AEP gain ($\sigma_\theta \neq 0^\circ$)
Quick et al. (2020)	11	5°	2.6%	0.48 – 0.58%
van der Hoek et al. (2020)	60	3°	1.10 – 1.28%	0.34 – 0.60%
van Beek et al. (2021)	48 (very dense)	5°	6.1%	2.5 – 3.4%
Submitted manuscript	69	2.5°	1.57%	0.28%
Submitted manuscript	69	5°	1.57%	0.07%

direction uncertainty. Therefore, we provide additional analysis to support our study. Specifically, we focus on three different aspects: comparison with previous works from the literature, impact of the chosen engineering wake model, and impact of the power–yaw loss exponent. This is followed by a general conclusion.

Comparison with previous works

A comparison between the results obtained in our work and previous studies that investigated a similar problem is provided in Table R1. It can be observed that both Quick et al. (2020) and van der Hoek et al. (2020) report reductions in AEP gains of an order of magnitude similar to those found in our work. In contrast, the results reported by van Beek et al. (2021) show relatively high AEP gains even when uncertainty is considered. We believe this is partly due to their case study choice, where the dense and aligned layout of the wind farm increases the potential for wake steering, but also due to differences in methodology. From van Beek et al. (2021), it is not clear whether turbine orientations are kept fixed when wind direction deviations are applied, or whether they are updated based on the corresponding wind direction. Given the relatively small reduction in performance reported in that study, we suspect that the latter approach is used (i.e. updating turbine orientation for each wind direction), which prevents a direct comparison with our results. In addition, van Beek et al. (2021) also considers uncertainty in yaw angles, explicitly accounting for unintentional misalignment of the turbines. However, a lower standard deviation value (1.75°) is used in that case.

Overall, this review of previous studies indicates that the results presented in our manuscript are consistent in terms of order of magnitude with those obtained in studies adopting a similar methodology, i.e. the works from van der Hoek et al. (2020) and Quick et al. (2020). In particular, when comparing our results with those of van der Hoek et al. (2020), who considered a wind farm with a comparable power density and number of turbines to our case study, the corresponding AEP gains become closer. Nevertheless, a discrepancy remains, as wake steering slightly underperforms in our manuscript relative to their findings. Therefore, we hypothesize that this could be caused by the choice of the engineering wake model and the value of the power-yaw loss exponent. These two aspects are investigated in the next sections.

Impact of the engineering wake model choice

To assess whether the relatively low performance of wake steering is caused by the specific choice of engineering wake model, we compare the Empirical Gaussian model used in the manuscript with other popular models available in PyWake. To enable a fast and fair assessment, we apply the yaw angles obtained using the geometric yaw approach termed Exponential Corrected, introduced in our previous work (Baricchio et al., 2024). Although these yaw angles are an approximation of the optimal solution (and therefore lower AEP gains are expected), this avoids biases that may arise from using yaw angles optimized for a specific wake model. Wind direction uncertainty is neglected in this comparison. The results are shown in Table R2 in terms of AEP gains obtained by applying these yaw angles with different wake models. The table also includes baseline wake losses to serve as reference values. From these results, it can be observed that the model used in the manuscript, with our re-tuned coefficients, yields AEP gains comparable to those obtained with the Bastankhah and Zong models, which are widely used in the community. In contrast, the TurbOPark model yields significantly higher AEP gains, but is also associated with substantially larger wake losses

Table R2: Comparison between different wake deficit models in terms of wake losses at baseline operation and AEP gains when wake steering is applied.

Wake deficit model	Wake losses	AEP gain
TurbOPark (Nygaard et al., 2020)	10.42%	1.56%
Bastankhah (Bastankhah and Porté-Agel, 2014)	5.02%	0.75%
Zong Zong and Porté-Agel (2020)	7.02%	0.77%
EmpGauss (this work)	5.57%	0.70%

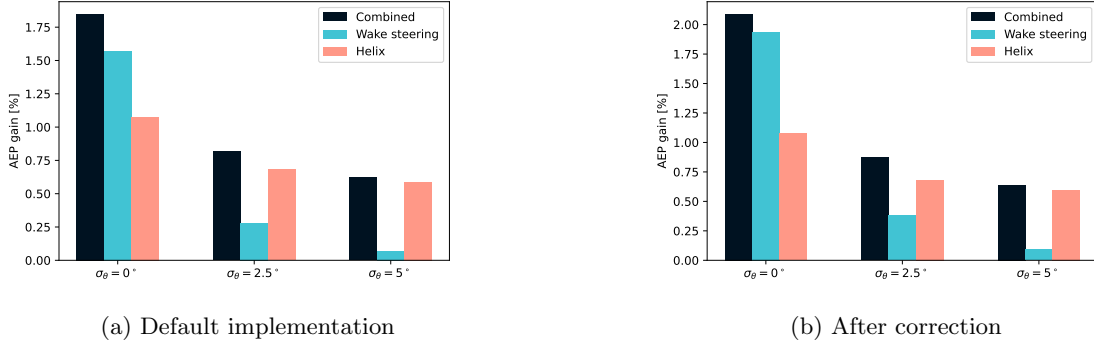


Figure R9: Impact of the power-loss yaw exponent on the evaluation of the WFFC strategies in terms of AEP gains. A constant value of 2.8 is assumed for the default implementation (representative of the below-rated wind speeds).

Impact of the power-yaw loss exponent

Another relevant aspect of this analysis is the impact of the power-yaw loss exponent, as already discussed in our response to comment R2.3. Figure R6 shows that using a lower (i.e. more realistic) value of the exponent yields a better match with the LES compared to the default PyWake settings adopted in this study.

To understand the impact of this correction on a large-scale wind farm, we evaluate the impact of the WFFC strategies using a power-yaw loss exponent of 1.88 during the AEP calculation. The same WFFC setpoints obtained in the manuscript are adopted here. The results are reported in Fig. R9b, which also includes the corresponding plot from the manuscript for comparison. The data show that the gains from wake steering slightly increase after this correction. Nevertheless, the observed trends remain unchanged.

A limitation of the analysis presented in Fig. R9b is that the correction of the power-yaw loss exponent is applied only in the evaluation of the strategies and not in the optimization of the control variables, which would require rerunning most of the simulations in this study. Therefore, higher AEP gains for wake steering (and consequently also for the combined strategy) would be expected if this correction were applied during the optimization phase. To assess whether the main conclusions of this work would change when updating the power-yaw loss exponent during optimization, we performed the optimization for a limited set of flow conditions. The results are shown in Fig. R10, where the power gains for a limited range of wind directions at a wind speed of $8, \text{m}, \text{s}^{-1}$ are reported. It can be observed that the power gains from wake steering are higher, and that wake steering is applied more frequently within the combined strategy. For instance, for $\sigma_\theta = 2.5^\circ$, the combined strategy shows a more balanced use of wake steering and helix across the different turbines, whereas it was previously dominated by helix operation before the correction. However, the main conclusions of this study remain unchanged.

These corrections are based on the assumption that the default implementation of power losses due

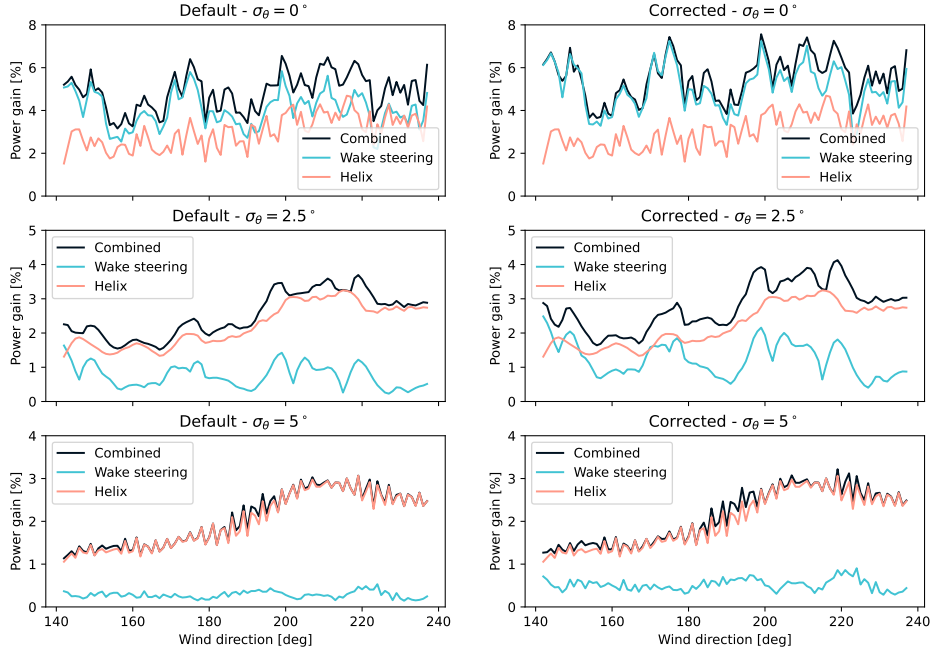


Figure R10: Impact of the power-loss yaw exponent on the evaluation and optimization of the WFFC strategies for a limited number of wind directions and a wind speed of 8 m s^{-1} . A constant value of 2.86 is assumed for the default implementation (representative of a wind speed of 8 m s^{-1}).

to yaw misalignment relies on a fixed value for the power–yaw loss exponent. However, this is not strictly the case, as the power is obtained directly from the power curve using the effective wind speed modified by the turbine yaw angle. As mentioned in Sect. 4.2 of the manuscript, this corresponds in our case to an effective exponent in the range 2.5–3.1, depending on wind speed. Therefore, since this value depends on the effective wind speed at each turbine, the correction cannot be readily applied a posteriori, as done in this example. This would instead require modifications to the PyWake implementation, which we consider beyond the scope of this work.

Conclusions

We agree with the reviewer that the detrimental effect of wind direction uncertainty on wake steering may have been overestimated in this work. Based on this analysis, we believe that one major contributing factor is related to how power losses due to yaw misalignment are modeled in PyWake. However, this implementation is not specific to this study, where only strictly necessary modifications were made to the original models, such as the implementation of the wake-mixing feature. Overall, we believe that further improvements to the models used here would not alter the main messages of the revised manuscript, which focus on the complementarity between WFFC strategies on a large-scale wind farm and their robustness under wind direction uncertainty.

R2.12. I would be very interested to see where the wind farm AEP gains originate from. Would it be possible to add a plot showing how much each turbine within the farm gains/loses power with respect to baseline?

The difference in AEP between the baseline and WFFC operation is shown for each turbine in Fig. R11, included in this document. The results show that the AEP gains mainly originate from turbines located in the central region of the farm and in the top-right corner. Since the dominant wind direction in this case study is SW, the turbines in the bottom-left corner reduce their power production when

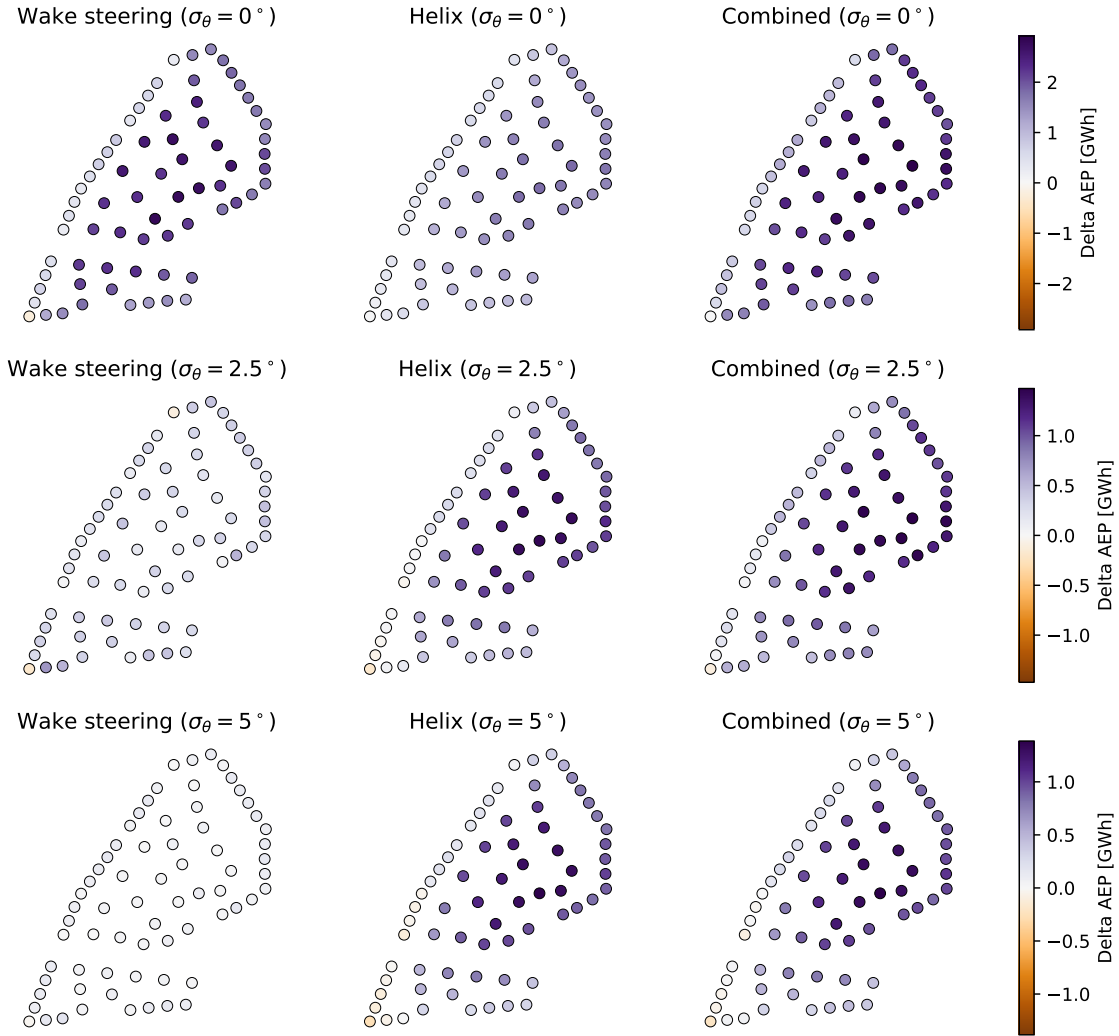
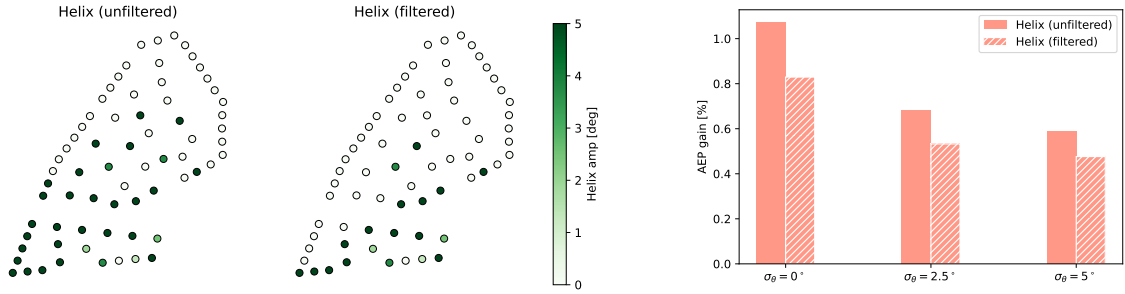


Figure R11: Impact of the power-loss yaw exponent on the evaluation and optimization of the WFFC strategies for a limited number of wind directions and a wind speed of 8 m s^{-1} . A constant value of 2.86 is assumed for the default implementation (representative of a wind speed of 8 m s^{-1}).

WFFC strategies are applied in order to benefit downstream turbines.

R2.13. Related to the above, figure 7b indicates to me that a significant part of the gain of helix is obtained by using it on downstream turbines. As you briefly mention yourself in the discussion, this is still very much uncharted territory and requires synchronization. Your model is also not tuned for waked turbines using the helix approach, not to mention waked turbines where the upstream turbine is also using the helix approach. I therefore feel like I need to know how much of the AEP gain can be explained by waked turbines using the helix. Could you please run a simulation in which only freestream/upstream turbines are allowed to use the helix, and compare the AEP gain with the situation in which all turbines are allowed to use it? I do not think this simulation needs to be added in detail to the manuscript, but I do think it is important to quantify what portion of the AEP gain comes from downstream turbines using the helix approach.

We greatly appreciate this suggestion and have decided to include the results in Appendix C, reported below. Specifically, Appendix C1 refers to the simulation case suggested by the reviewer, while Appendix C2 shows that the majority of the AEP gains are obtained by actuating WFFC strategies on upstream turbines.



(a) Effect of the filter to avoid the application of the helix method on downstream turbines. The example refers to a wind direction of 201° and speed of 8 m s^{-1} .

(b) AEP gains comparison between filtered and unfiltered helix control setpoints.

Figure R12: (Fig. C1 in the revised manuscript) **Impact of the assumption of actuating the helix method on downstream turbines.**

Appendix C1: Impact of actuating the helix strategy on downstream turbines

The WFFC optimization conducted in this study allows any turbine in the wind farm to apply the helix method whenever an increase in the objective function is obtained. However, as mentioned in Sect.4.2, applying the helix method across multiple turbines requires appropriate synchronization between upstream and downstream turbines, a concept that remains unexplored for large-scale wind farms. To understand the impact of this assumption, an additional scenario is evaluated, which prevents downstream turbines from applying the helix method. The new control setpoints β_{fl} are obtained by filtering the results of the previous WFFC optimization, i.e. setting the helix amplitude to zero of all turbines located within the wake of an upstream turbine applying the helix method. An example of this filtering phase is provided in Fig. C1a. Specifically, a turbine is considered waked if it lies within the trapezoidal wake shape of an upstream turbine, obtained with an expansion coefficient of 0.01. The results expressed in terms of AEP gains are shown in Fig. C1b. It can be observed that the difference between the two cases, filtered and unfiltered, is limited, demonstrating that this assumption does not affect the main conclusions of this study.

Appendix C2: Lookup tables of the combined strategy for a balanced objective

This section analyzes how the LUTs obtained from WFFC optimization change when the penalty on the COT is introduced. Figure C2 shows the control setpoints of the same turbines examined in Fig. 10. Comparing these LUTs with those shown in Fig. 10, it can be observed that the actuation of the turbine located in the central region of the farm is significantly reduced when the penalty is considered. On the other hand, the LUTs of the turbine on the perimeter remain almost unchanged.

Minor comments

1. Line 2: rather **THAN** the output. This has been corrected in the revised manuscript.
2. Line 2: an established **WIND FARM FLOW CONTROL** approach This suggestion has been implemented in the revised manuscript.
3. Line 4: you should define/explain the helix here, or at the very least specify that this is the wake mixing strategy that you are studying in this paper. We have now added a brief explanation in the revised manuscript.
4. Line 19: I would say wake steering is **THE** most effective solution, or at the very least the most established solution. We have implemented this modification in the revised manuscript.
5. Line 30: “These concepts have ...”, I think you mean the helix here, as all the citations refer to helix studies. If so, it should be singular (“This concept has ...”) We have corrected the syntax as

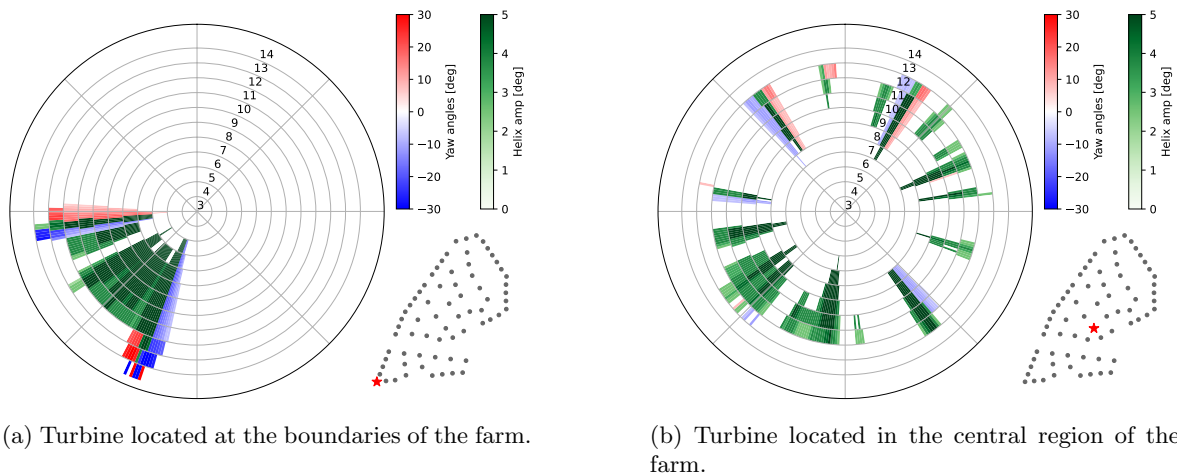


Figure R13: (Fig. C2 in the revised manuscript) Control rose for the combined strategy of two different turbines obtained applying the penalty on COT with a weight $w = 2.5 \cdot 10^5$. Each subplot shows the values of the control variables in the LUT for each wind speed and direction, obtained with a wind direction uncertainty of $\sigma_\theta = 2.5^\circ$.

suggested in the revised manuscript.

6. Introduction: I know this is a personal preference and I wouldn't say it's wrong, but I don't love the use of present perfect tense throughout the introduction. You are at least consistent, but I would personally prefer past tense or even present tense ("Citation A showed that ..." instead of "Citation A has shown that ..."). We prefer to use the present perfect tense to summarize the current state of knowledge.

7. Line 43: It is fair to say that the analysis limits itself to two-turbine arrays, but in your study, you also only do LES for three-turbine arrays. I would therefore argue that your findings have similar limitations. We respectfully disagree with this statement. The primary objective of this study is to extend the insights obtained from small turbine arrays to a large-scale wind farm context. While it is true that the LES data used for model calibration are limited to configurations of up to three turbines, these simulations serve as a basis for developing and tuning an engineering wake model, which is subsequently applied at the wind farm scale.

8. Line 80: "... in reaching the global optimum." Please add a source for this claim. We have modified this sentence, focusing instead on the dimensionality of the problem.

9. In general: Why did you choose to specifically study only the helix approach and not DIC? I have no issue with the choice, but it couldn't hurt to justify it. The main reason for this choice is the availability of LES data for the helix operation required to tune the engineering wake model; the selection is therefore primarily driven by practical considerations. As noted in Sect. 4.1, extension to DIC would only require minor modifications within the present framework, provided that corresponding LES data are available for model re-calibration. In addition, we expect similar conclusions for DIC and the helix method, given their comparable steady-state behaviour.

10. Line 90-94: Make the contributions listed here full sentences please. Also, I wouldn't split them up into two lists, the "additional contributions" are in my opinion just as much contributions as the main ones. These lines have been modified as suggested by the reviewer.

11. Line 97-99: Use "Section" or "Sect.", but be consistent. We have followed the WES guidelines: *The abbreviation "Sect." should be used when it appears in running text and should be followed by a number unless it comes at the beginning of a sentence.*

12. Line 106 “The effect of wake steering ...”: add “on the upstream/waked turbine”. Actually, the explanation here refers to the effect on the actuating turbine. This has been clarified in the revised manuscript: **The effect of wake steering on the actuating turbine...**

13. Line 130 / Appendix A: In my opinion, the process of tuning the model is fine to be moved to an appendix. However, the results of the tuning, i.e., how well your model matches the higher fidelity LES data, is very important in assessing the reliability of the results, and should be moved to the main part of the manuscript. Based on comments R1.8 and R2.3, we have added a new section titles **3.1 Model validation** to the main text. We believe that this also addresses the request of this comment.

14. Table A1: “inflow”, not “infow” We have corrected the typo in the revised version.

15. Section 2.5: I would move this section forward, as I felt like I already needed this information to understand some of what was mentioned in previous sections. We believe that Sects. 2.1-2.4 describe the general methodology, which can be applied to any case study (e.g. different farm sizes, site locations, turbines, flow discretizations, and uncertainty values). Section 2.5 then outlines the specific choices made to produce the results reported in the manuscript. Therefore, for consistency, we prefer to retain the original structure.

16. Figure 3&4: consider trimming the y-axis, at least for the first two rows, to show a more relevant/zoomed representation of the findings. We have reduced the range of the y-axis from $[-2.0 D, 2.0 D]$ to $[-1.5 D, 1.5 D]$, as suggested by the reviewer.

17. Figure 7a: what is the downstream distance between the controlled turbine and the nearest downstream turbine in the 225 degree-sector where helix is implemented? How much does this downstream turbine benefit from the shown turbine using the helix? Does that correspond to the LES results? The distance between these turbines is $4.5 D$, i.e the same used for comparison with the LES results shown in Fig. 4 of the revised manuscript. Therefore, this point has been addressed in Sect. 3.1.2, which has been added to the manuscript in this revised version.

18. Line 381-383: You only mention one of the many dimensions in which you are extrapolating LES simulations here. This section should include a much more extensive summary of ways in which you are extrapolating results. These aspects have now been included in this section of the revised manuscript, as also suggested in comment R.2.1.

19. Line 451 “with a specific focus on the helix technique”: a specific focus implies that you also looked at other wake mixing techniques, which you didn’t. This part of the sentence has been removed, as suggested by the reviewer.

20. Table A2 (1): For tuning the turbine model, did you use LES simulations or standalone OpenFAST simulations? If you did the former, you could use these simulations to compare optimal values found with the pyWake model, at least in the case of a 2-turbine setup, to validate the match. A validation case replicating the two-turbine setup has been added to the revised manuscript in Sect. 3.1.1, as suggested here and in comment R2.3.

21. Table A2 (2): As part of the model verification, please provide a comparison of turbine powers between the 3-turbine LES cases and the exact same setup in pyWake. As mentioned above, this would function as a floor on the modeling error and helps the reader assess the reliability of results in the more complex case. A validation case based on the 3-turbine LES case has been included in the revised manuscript in Sect. 3.1.2, as suggested here and in comment R2.3.

22. Appendix B: It seems to me that the current algorithm uses unnecessary repetitions of the same simulations. Perhaps that doesn’t matter since the simulations are cheap, but if that is the case, then why do you not take more iteration steps than the 3 mentioned in Table 1 to assure convergence? Furthermore, I am surprised to see that smaller values are found to be optimal for the helix method. How does this compare with the results from the LES simulations?

We acknowledge that the reviewer’s observation is correct: repetitions occur when the bounds of the control variables are selected before the final step. This aspect is clearly noticeable in the simplified example shown in Fig. B2, while in practice these conditions occur less often. For this reason, it was not explicitly addressed in the initial version of the algorithm to preserve its structural simplicity. Nevertheless, we agree that this is a relevant point, which we will consider in future developments of the algorithm, with the aim of further reducing computational cost.

The settings chosen for the MSR algorithm ($N_{\text{values}} = 5$ and $N_{\text{step}} = 3$) result in a resolution of $\delta\gamma = 0.9375^\circ$ and $\delta\beta = 0.078125^\circ$, as obtained from Eq. 1. Increasing the number of steps would only improve the resolution of the control variables, not exploring alternative optimal paths. We consider the resolutions used in this study to be sufficient and we suggest that, if additional computational resources are available, increasing N_{values} rather than N_{step} would be preferable. This would enable the exploration of a broader set of solutions and could potentially yield improved results.

$$\delta c = \frac{c_{\text{max}} - c_{\text{min}}}{(N_{\text{values}} - 1)^{N_{\text{step}}}} \quad (1)$$

In most cases, the helix amplitude is either set to 0° or β_{max} . Intermediate values may arise from a trade-off between power loss on the actuating turbine and reduced wake losses downstream. However, these typically correspond to only a marginal increase in power and are therefore filtered out when even a small penalty on control effort is included. This behaviour can be partially observed in Fig. R13. To further illustrate this, we perform the following test: the optimal β values obtained in this study are post-processed such that they are set to 5° if $\beta \geq 2.5^\circ$ and to 0° otherwise. In this case, only the maximum helix amplitude is allowed as a control setpoint. The updated AEP gains are shown in Fig. R14, where they are compared with the original values. It can be observed that the differences are limited, confirming the small impact of intermediate β values. Nevertheless, the remaining gap in Fig. R14 can be attributed to the fact that these updated values are not necessarily optimal under the modified constraint, since they are obtained by filtering previous optimization results rather than rerunning the optimization.

Lastly, to address the question of comparison with LES results, we believe it is more appropriate to focus on the two-turbine case, since helix operation has not yet been explored in large-scale wind farms using LES. The study of van der Hoek et al. (2026) identified an optimal helix amplitude of approximately 4° – 5° for a two-turbine array, which is similar to the β_{max} adopted here. Therefore, the findings of van der Hoek et al. (2026) are consistent with the reviewer’s expectation. We can compare these LES results with those shown in Fig. 5–6 of the revised manuscript. It can be observed that for $\sigma_\theta = 0^\circ$ and 2.5° , the optimal β found using PyWake exhibits a predominantly binary behaviour, i.e. $\beta = \beta_{\text{max}}$ or $\beta = 0^\circ$, for most conditions. In contrast, for $\sigma_\theta = 5^\circ$, intermediate β values occur more frequently, as also noted in the reviewer’s comment. However, Fig. 6 shows that these cases correspond to negligible power gains and are therefore not relevant to the overall analysis.

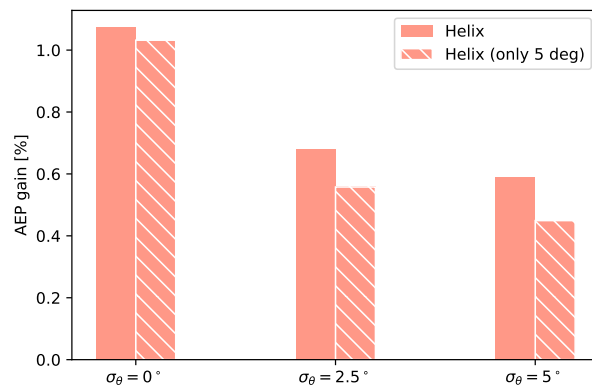


Figure R14: Impact on the AEP gains of considering only β_{\max} as possible helix amplitude value.

Additional plots

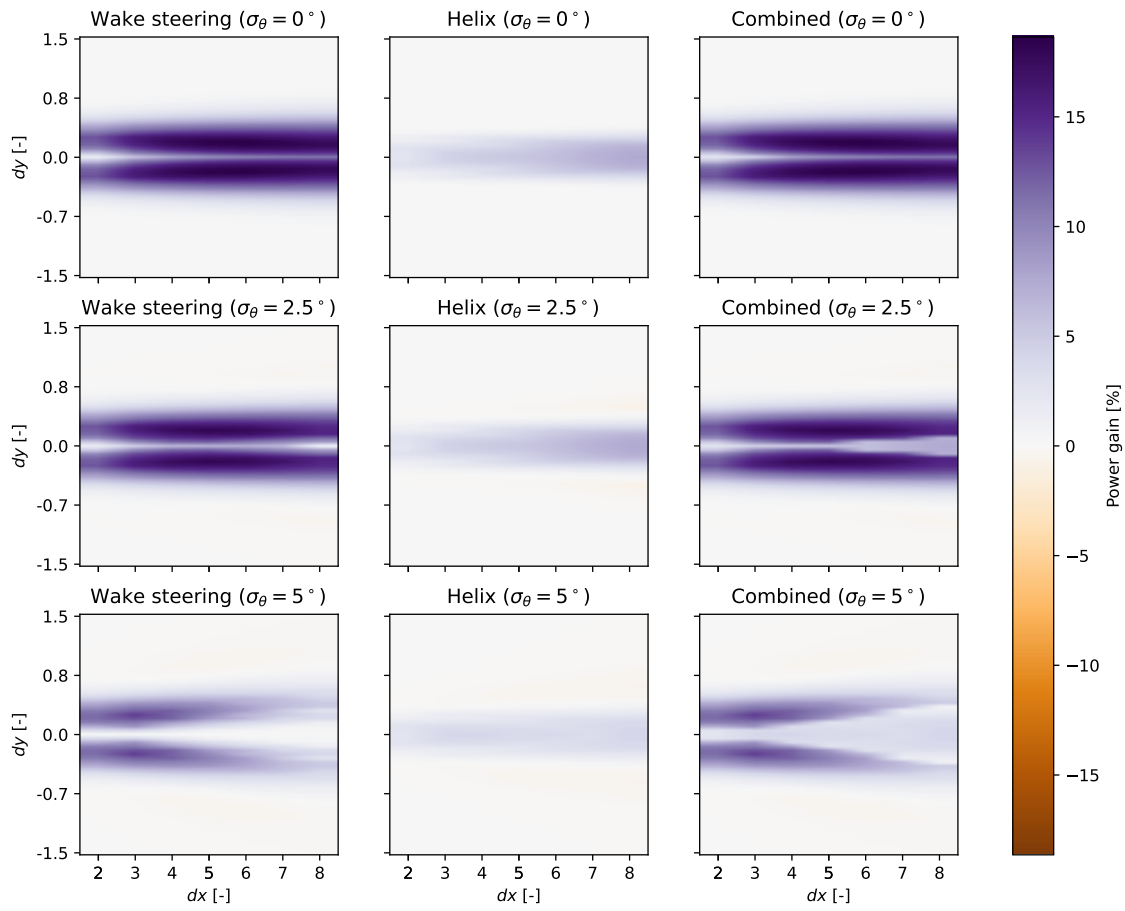


Figure R15: Power gains of the two turbines wind farm for different positions of the downstream turbine, evaluated considering $\sigma_\theta = 0^\circ$. Each subplot specifies the σ_θ value used for the optimization.

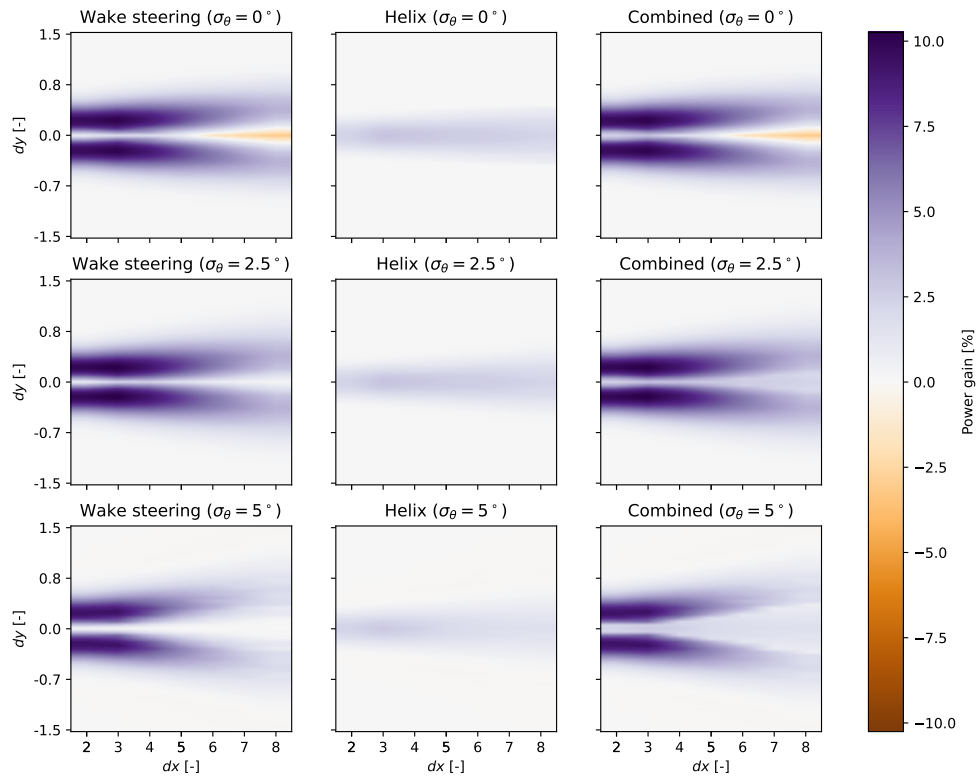


Figure R16: Power gains of the two turbines wind farm for different positions of the downstream turbine, evaluated considering $\sigma_\theta = 2.5^\circ$. Each subplot specifies the σ_θ value used for the optimization.

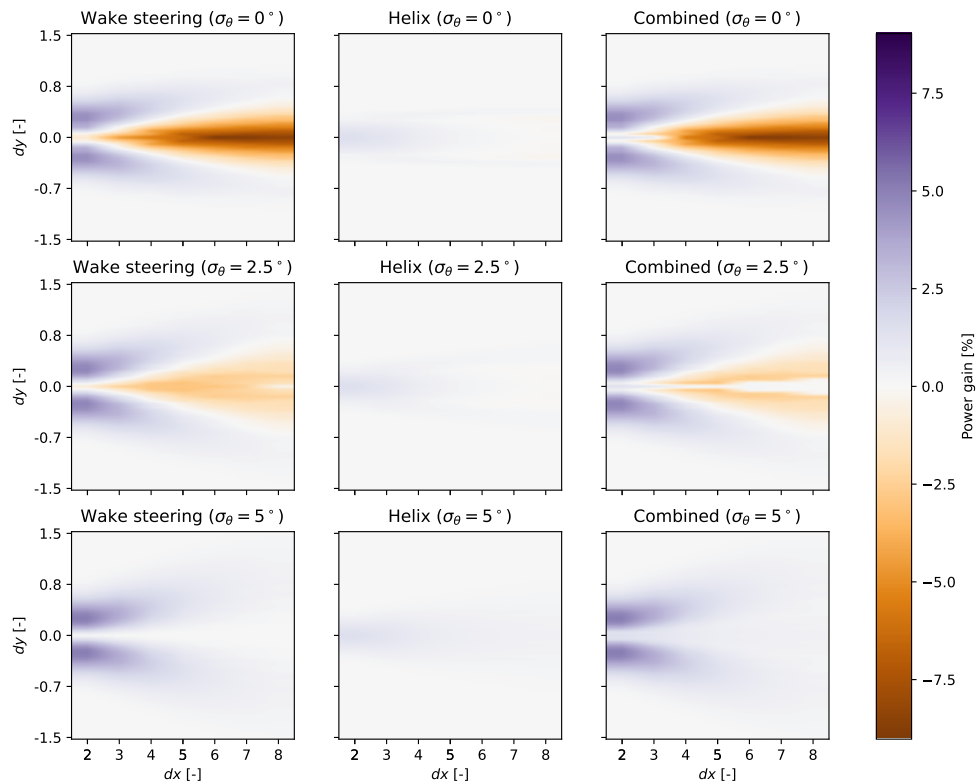


Figure R17: Power gains of the two turbines wind farm for different positions of the downstream turbine, evaluated considering $\sigma_\theta = 5^\circ$. Each subplot specifies the σ_θ value used for the optimization.

References

- Baricchio, M., Gebraad, P. M. O., and van Wingerden, J. W.: Evaluating the potential of a wake steering co-design for wind farm layout optimization through a tailored genetic algorithm, *Wind Energy Science*, 9, 2113–2132, <https://doi.org/10.5194/wes-9-2113-2024>, 2024.
- Bastankhah, M. and Porté-Agel, F.: A new analytical model for wind-turbine wakes, *Renewable Energy*, 70, 116–123, <https://doi.org/https://doi.org/10.1016/j.renene.2014.01.002>, special issue on aerodynamics of offshore wind energy systems and wakes, 2014.
- Gaumond, M., Réthoré, P.-E., Ott, S., Peña, A., Bechmann, A., and Hansen, K. S.: Evaluation of the wind direction uncertainty and its impact on wake modeling at the Horns Rev offshore wind farm, *Wind Energy*, 17, 1169–1178, <https://doi.org/https://doi.org/10.1002/we.1625>, 2014.
- Gebraad, P. M. O., Teeuwisse, F. W., van Wingerden, J. W., Fleming, P. A., Ruben, S. D., Marden, J. R., and Pao, L. Y.: Wind plant power optimization through yaw control using a parametric model for wake effects—a CFD simulation study, *Wind Energy*, 19, 95–114, <https://doi.org/https://doi.org/10.1002/we.1822>, 2016.
- Liew, J., Urbán, A. M., and Andersen, S. J.: Analytical model for the power–yaw sensitivity of wind turbines operating in full wake, *Wind Energy Science*, 5, 427–437, <https://doi.org/10.5194/wes-5-427-2020>, 2020.
- Meyers, J., Bottasso, C., Dykes, K., Fleming, P., Gebraad, P., Giebel, G., Göçmen, T., and van Wingerden, J. W.: Wind farm flow control: prospects and challenges, *Wind Energy Science*, 7, 2271–2306, <https://doi.org/10.5194/wes-7-2271-2022>, 2022.
- Mittelmeier, N., Blodau, T., and Kühn, M.: Monitoring offshore wind farm power performance with SCADA data and an advanced wake model, *Wind Energy Science*, 2, 175–187, <https://doi.org/10.5194/wes-2-175-2017>, 2017.
- Nygaard, N. G., Steen, S. T., Poulsen, L., and Pedersen, J. G.: Modelling cluster wakes and wind farm blockage, *Journal of Physics: Conference Series*, 1618, 062 072, <https://doi.org/10.1088/1742-6596/1618/6/062072>, 2020.
- Quick, J., Annoni, J., King, R., Dykes, K., Fleming, P., and Ning, A.: Optimization Under Uncertainty for Wake Steering Strategies, *Journal of Physics: Conference Series*, 854, 012 036, <https://doi.org/10.1088/1742-6596/854/1/012036>, 2017.
- Quick, J., King, J., King, R. N., Hamlington, P. E., and Dykes, K.: Wake steering optimization under uncertainty, *Wind Energy Science*, 5, 413–426, <https://doi.org/10.5194/wes-5-413-2020>, 2020.
- Rott, A., Doekemeijer, B., Seifert, J. K., van Wingerden, J. W., and Kühn, M.: Robust active wake control in consideration of wind direction variability and uncertainty, *Wind Energy Science*, 3, 869–882, <https://doi.org/10.5194/wes-3-869-2018>, 2018.
- Simley, E., Fleming, P., and King, J.: Design and analysis of a wake steering controller with wind direction variability, *Wind Energy Science*, 5, 451–468, <https://doi.org/10.5194/wes-5-451-2020>, 2020.
- Türk, M. and Emeis, S.: The dependence of offshore turbulence intensity on wind speed, *Journal of Wind Engineering and Industrial Aerodynamics*, 98, 466–471, <https://doi.org/https://doi.org/10.1016/j.jweia.2010.02.005>, 2010.
- van Beek, M. T., Viré, A., and Andersen, S. J.: Sensitivity and Uncertainty of the FLORIS Model Applied on the Lillgrund Wind Farm, *Energies*, 14, <https://doi.org/10.3390/en14051293>, 2021.
- van der Hoek, D., Doekemeijer, B., Andersson, L. E., and van Wingerden, J. W.: Predicting the benefit of wake steering on the annual energy production of a wind farm using large eddy simulations and Gaussian process regression, *Journal of Physics: Conference Series*, 1618, 022 024, <https://doi.org/10.1088/1742-6596/1618/2/022024>, 2020.

van der Hoek, D., Dammann, T., and van Wingerden, J. W.: Gaussian process surrogate modeling for efficient controller tuning and fatigue load prediction of the helix wake-mixing method, *Wind Energy Science Discussions*, 2026, 1–26, <https://doi.org/10.5194/wes-2026-62>, 2026.

Vortex FDC: Vortex Wind Map, Accessed via Youwind, <https://youwindrenewables.com>, accessed: 2024-11-01, 2024.

Zong, H. and Porté-Agel, F.: A momentum-conserving wake superposition method for wind farm power prediction, *Journal of Fluid Mechanics*, 889, A8, <https://doi.org/10.1017/jfm.2020.77>, 2020.