Review: Yaw-based wake steering control under field uncertainties: a line of 4 industrial wind turbines investigated with LES and engineering wake models

The work studies the impact of errors in assumed wind direction and nacelle alignment on the power performance and flow when applying yaw-based wake steering to a 4-turbine row in a neutral boundary layer. The power and wind farm flows are evaluated using both LES and engineering wake models, and the results compared. The main conclusion is that even a small error in assumed mean wind direction, when resulting in a change in sign of the optimal yaw angle, can turn a significant predicted power gain into a small loss. The work is well written, concerns a relevant topic and approaches the problem with sound numerical methodology and simulation design. However, I have major concerns regarding the reproducibility and novelty of the work.

Major comments:

My first concern regards the lack of reproducibility, given the very limited information provided about the setup, and the impact that has on the interpretability of the results. No details on turbine diameter, hub height, or rated wind speed are given. The turbine spacing (both streamwise in the row and lateral from the periodic boundaries) is also not given, despite the fact that it is key to understanding the main result of the work. When assessing the impact of an error in the assumed inflow wind direction, it cannot be known what a 4deg offset means for the expected change in wake impingement downstream without knowing the distance between turbines. Secondly, the analysis only considers a flow time of 1200s, but without knowing the turbine diameter, spacing or hub-height velocity magnitude, it is difficult to assess whether this flow time is sufficient to lead to appropriately converged statistics. The justification for this lack of information is that the setup represents a real commercial wind farm, from which SCADA data is utilised. However, the SCADA data is only used in a single plot (Fig 7) and shows a very large spread of results. Therefore, I would not consider this comparison with SCADA data to constitute a 'validation' (as stated in the Introduction) of the models. Overall, the inclusion of the SCADA data seems to add very little, while making the most important conclusions of the work difficult for a reader to interpret, due to the omission of key details of the setup. My recommendation would be to remove the SCADA data and study a wind farm for which the dimensions and layout can be reported, in order to improve the reproducibility and interpretability of your results.

The evaluation time of 1200s is my second concern. Assuming a diameter of 180m, a spacing of 7D, and a hub height velocity (in the farm, so including wakes) of 6m/s, one flow through of the wind farm would take 840s, meaning that the total evaluation time covers less than 1.5 flow-throughs of the wind farm. Regardless of the precise numbers, it seems unlikely that this is sufficient to provide converged statistics for the whole wind farm, particularly when the impact of small changes in assumed wind direction and nacelle alignment are being investigated. In Section 4.2.2, where the spectra of wake meandering are studied, only half of the total evaluation time is utilised (600s). Substantive conclusions are drawn from these spectra, but they seem highly speculative when only based on 10 minutes of data. My

suggestion would be to rerun the baseline case with a longer evaluation time and analyse the convergence of the statistics and spectra, and then either report these results or rerun all simulations with a longer flow time if convergence cannot be shown.

The last major concern regards novelty. Line 80 states: 'Similarly to a recent initiative (Hodgson and Andersen, 2025)'. Having seen the referenced paper, it seems that there are striking similarities between the two works. These similarities relate to the setup (a 4-turbine row in a neutral/ conventionally neutral boundary layer), the research questions investigated (the impact of static wind direction errors on the effectiveness of yaw-based wake steering), and the conclusions (that a small unknown error in wind direction can turn a large predicted gain into a loss). Therefore, rather than just mentioning this work in passing in the introduction, it would be more appropriate to compare the works in greater detail, both in the introduction and in the results/ discussion, and clearly justify what this paper contributes in terms of new knowledge.

Minor comments:

Figure 1: As the dimensions are not stated, it is hard to assess what the blockage ratio is here, and whether it is likely to have an impact on the results. Seeing as changes in the thrust coefficient due to yawing can affect blockage, the results may be sensitive to domain size. Therefore, it would be nice to justify this choice or provide information on what sensitivity studies have been conducted to ensure that the domain size does not influence results.

Line 124: If the Gaussian distribution approach to simulate wind direction uncertainty was utilised in the optimisation of the yaw setpoints, then why in Fig 2 is there such a steep step change around 0deg? I would expect the Gaussian approach to smooth the yaw setpoints around the fully aligned condition as shown in Rott et al. 2018. Please clarify exactly how and when the Gaussian uncertainty representation is utilised.

Line 125: It would be valuable to state and provide citations for wind direction uncertainty values obtained from field experiments or used in other work, to provide a point of comparison.

Line 136: You address this later, but it might be nice to also state the power-yaw loss exponent used in FLORIS here as well, and then refer to your later discussion.

Line 144: Please give further justification as to why many models were combined in obtaining the yaw setpoints, while only one of those is used for evaluation.

Line 172: Not sure this sentence is needed, is this not just restating what an actuator line is?

Line 175: Please comment on the likely impact of these omissions on the loads and wakes.

Line 197: Please justify why you chose a neutral boundary layer, rather than e.g. a conventionally neutral boundary layer, which includes veer. This is likely to have quite a big impact on your results, given the interaction between veer and yaw-induced wake deflection.

Perhaps comment on the likely impact of this, either here or in a discussion section, particularly regarding the agreement between engineering models and LES.

Line 219: Please provide some citations or further details on the use of a parallelepiped precursor domain geometry to reduce unphysical flow artifacts due to domain size, and justify the choice of a 1D lateral translation – have sensitivity studies been conducted?

Figure 5: Please provide actual profiles of velocity and TI with height, as these plots are very difficult to discern and therefore the inflow is not reproducible.

Figure 6: What happens to the development of turbulence when the cell size changes so rapidly around the vicinity of the turbine and wake? Please include citations which support and validate this approach.

Line 250: Is a 60s sample size meaningful when assessing a row of 4 turbines? It seems small compared to e.g. the wind farm flow-through time. Despite not being overlapping, the 60s samples will not be independent, due to the short overall flow time assessed (1200s), so can much really be interpreted from the fact that the bootstrapped 60s samples and the total average agree well with eachother?

Line 283: 5deg sounds like a reasonable choice, but can you provide a justification? For example related to the wind direction variability in the LES, or similar?

Figure 7: Despite the significant filtering, there is a very large spread in the presented SCADA results after the first turbine location. Therefore, I don't think that the statement in Line 90 that this constitutes a 'validation' is appropriate. As stated in the first major point, my recommendation would be to remove the SCADA data and use a setup for which the dimensions and simulation details can be properly provided.

Line 303: Please explain this statement further.

Line 330-331: I think this is already well known and has been shown many times previously, so perhaps compare to and cite the existing literature here.

Figure 12: What happens to the vertical velocity component at the end of the planes?

Figure 13: Why do the LES results show a negative normalised velocity deficit in the negative y directions? Perhaps you should comment on this in the discussion of the figure.

Lines 461-474: The explanation here appears to be quite reasonable, but it seems difficult to distinguish whether this indicates a non-converged flow solution or whether the described trends arise due to the physical effects that you describe.

Lines 500-505: As stated in the major points, this section seems to be highly speculative when only 10 mins of data are considered in your analysis. How can the contribution and development of large scale turbulent structures be properly assessed over such a short flow time?

Conclusions: This section should be split into a Discussion section and a Conclusions section. In the Discussion section, the limitations, implications, and comparisons with other work can

be expanded on in more detail. That will allow the Conclusions section to be more concise and not introduce topics that have not previously been mentioned.

Line 533 and Line 539/540: ‘partly supported by previous findings’ and ‘highlighting once more’ – if there is previous work on this topic, you should cite it and discuss how much it supports your conclusions in a Discussion section.

Lines 557-559: Similar to the previous comment on Line 250, the implication that 60s sampling may be a viable power change assessment method in wind farms because of the small variability seems quite misleading when the bootstrapped 60s samples are all taken from such a short total flow time. Perhaps remove, rephrase, or provide more details if I am missing something here.

Line 603: Where does the 90% value come from? Is it just based on testing?

Typos/ grammar:

Line 142: This sentence is very confusing, please rephrase.

Line 172: I think that instead of ‘Especially’ you mean ‘Specifically’ – this typo occurs in a few places so please fix all occurrences.

Line 174: ‘conveniently stored’ – too colloquial, please rephrase

Line 328: ‘may not be totally accurate’ – too colloquial, please rephrase

Line 415: ‘downstream the first 3 turbines’ – an ‘of’ or ‘from’ is required after ‘downstream’. This is also repeated a few times so please fix all occurrences.

Line 425: ‘show to differ’ – grammatically incorrect, please rephrase

Line 545: grammatically incorrect, perhaps rephrase e.g. to ‘an outcome which was somewhat expected’

Line 547: Even swapping especially and specifically, I don’t think this sentence makes sense, so please rephrase.

Line 549/550: Sentence starting ‘Nonetheless’ seems to be just repeating the previous sentence? So perhaps remove or rephrase.

Line 573: ‘yet convenient’ – either remove this or rephrase the sentence, perhaps you mean ‘while convenient’ but still some additional rephrasing is required.

Line 575: ‘remain’ not ‘remained’

Line 581: typo ‘asPowe’