Authors’ response to “Increased power gains from wake steering control using preview wind direction information”

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Response to all reviewers

The authors appreciate the feedback from all reviewers. The authors’ response to each of the reviewers’ comments (in black) can be found below (in red), as well as the rephrased sentences or added text (in blue). Besides, some editorial changes were made to the manuscript.

Reviewer 1

General comments

In this study, the author investigates the preview control of wind turbines under varying incoming wind directions. Overall, this reviewer finds that this manuscript is of good quality and can be accepted with some minor revisions.

Minor comments

1. In line 193, the author should cite the original literature of ADMR (Wu and Porté-Agel, 2011), which was also cited in Dörenkämper et al. (2015).
   Agreed
   “In the main simulation, two NREL 5 MW turbines are simulated using an Actuator Disc Model with Rotation (ADMR) similar to Wu and Porté-Agel (2011) as described in Dörenkämper et al. (2015).”
2. In line 195, the author mentioned that “As a result, a transient wind direction change propagates through the domain with a time-varying wind speed and direction”. To me, it seems to indicate that the spatial variation of wind direction in the simulation domain is associated with the incoming wind speed. Considering the fact that the authors adopt a Taylor frozen hypothesis in their control model, this reviewer would like to ask the authors to add a short discussion on the possible influence of different incoming wind speeds on the preview control.
   The authors understand that this sentence might cause some confusion. While a Taylor frozen hypothesis with a constant wind speed is assumed, in LES the wind speed will obviously fluctuate around its mean, which is what is meant with “time-varying wind speed” in line 195.
   “As a result, a transient wind direction change propagates through the domain with a time-varying wind speed (fluctuations around the mean) and direction (fluctuations around the mean on top of the forced signal) rather than collectively in the whole domain as was the case in Stieren et al. (2021).”

   Regarding the discussion on the impact with wind speed, a small notion was already included in the discussion part of the manuscript in lines 432-434. This section has now been rewritten.
   “With similar reasoning the wind speed is also expected to impact the benefit of preview control, as it is demonstrated to affect the effectiveness of wake steering (Simley et al., 2021a).

Furthermore, the preview distance is directly subject to the wind speed, and fluctuations will add another layer of complexity in the forecasting of the incoming wind direction changes.”

This discussion is now also referred to earlier in the manuscript. This is done in Sect. 3.2.1, which is the first time the simulated atmospheric conditions are discussed.
“Although the effect of atmospheric conditions (wind speed, stability) on the effectiveness of preview control is deemed interesting, investigating this was considered out of the scope of the current study. A short discussion is included in Sect. 5.2.”

3. In Section 4.3, the authors present a very interesting comparison of the results from LES and the computational cheap engineering model. This reviewer would appreciate a brief analysis of the possible directions for the improvement of the engineering model, which shows a high correlation but also a systematic bias to the LES results.
   The authors would like to thank the reviewer for this suggestion. This is indeed a valuable addition to this section.
   “The next steps to improve the engineering model could be to include the time-varying wind speed and veer, which are currently assumed to be constant. This would likely result in an increased accuracy at the cost of slowing down the model. Additionally, as the engineering model is subject to the underlying DART wake model, improvements there will likely also result in a more accurate engineering model.”

Reviewer 2

General comments

This paper describes quite clearly the results of some rather thorough investigations into the value of preview information for yaw control and wake steering control. My main criticism is in the balance between the amount of careful and dedicated work which was done compared to the value of the results. The main conclusion is that there is value in using preview information; but it would be rather surprising if that were not the case.

The authors appreciate this critical evaluation. The objective of the manuscript was not to just show that there is a benefit to using preview information, but rather to estimate how large this benefit is for wake steering. A detailed response is given in the reply to major comment 1.

Using a number of more and less detailed simulations, the paper attempts to quantify the benefits in terms of energy production gain. The accompanying changes in the amount of yawing action are only mentioned qualitatively, without trying to quantify this. Turbine loading implications are not covered - that would be well beyond the scope of the paper, but it should perhaps be mentioned as an issue.

The authors would like to thank the reviewer for these suggestions. A discussion has been added to a new Sect. 5.6:

“5.6 Effect of preview control on yawing action and loads

As can be seen in Fig. 2 and 9, the number of yaw maneuvers in wake steering is much higher than in greedy control, which is well described in literature (e.g., Bossanyi, 2019; Kanev, 2020). However, preview control does not seem to affect the number of yaw maneuvers, at least not as long as the same yaw controller is used.

A detailed analysis of how preview control affects turbine loads was considered out of the scope of the current work. The effect of wake steering on loads is complicated and heavily discussed in literature (Houck, 2021, and references therein). It can be hypothesized that due to preview control, the loads on the upstream turbine are reduced since extreme misalignments are avoided (e.g., Fig. 9c). Since the wake will be more successfully steered away from the downstream turbine, the loads of this turbine are also expected to be lower.”

The simulations are certainly useful as illustrations, and as prompts for thinking about some of the issues involved. The authors rightly identify many of the shortcomings in the modelling, but with all the uncertainties, I think it would be difficult to generalise the quantitative results in any meaningful way.

The authors agree that generalization of the results is difficult, which is discussed in Sect. 5.2. However, it is not the aim of this work to present findings that are valid for all situations. Rather, what the reviews calls “prompts for thinking” is more the aim of this work, as will be discussed in more detail in major comment 1.

Major comments

1. The preview is modelled as coming from a notional met mast upstream of the turbines, providing a perfect preview which advects to the turbines as frozen turbulence. The authors acknowledge that this is not practically realistic, and mention some other possibilities, such as using turbine data or multiple scanning LiDARs. Both of these are so different from what is assumed that they should be modelled explicitly. The real question is, what additional information is it practical to obtain, and what performance gains are practically realisable from such information, given its inevitable imperfections, and is the benefit sufficient to make it worthwhile to obtain this information? Otherwise, the conclusion that perfect preview information is useful is self-evident.

The authors would like to point out that the wind direction signal obtained at the virtual upstream met mast is not a perfect forecast. Since advection occurs between the virtual met mast and the turbine, the wind experienced by the turbine will differ from the measured wind at any upstream location. On the one hand one could argue that the quality of the signal from a (virtual) met mast is higher than that from lidars or turbine data due to its higher temporal resolution and relatively small error. On the other hand, lidars can provide a spatial averaged wind direction, which could be more useful for wind farm control where large flow structures are important.

The authors fully agree with the reviewer that it is of great importance to find out what benefit preview control can feasible have when using data from other devices. However, the authors still see a lot of value in the work presented here. When the current study, with all its assumptions and a near-perfect forecast, would illustrate that the benefit of preview control is actually extremely small, it would be pointless to research this in the future. Instead, this study has shown that power gain from preview control can be substantial and that it is a topic worth pursuing in future work. This was already discussed in Sect. 5.1, but has now been emphasized more in the manuscript:

“From these results it is concluded that the benefit of preview wind direction control for wake steering is substantial, making it a topic worth pursuing in future work. ” (Abstract)

“The magnitude of potential power gains is unknown, but they should be substantial to make preview control worth pursuing in future work.” (Introduction)

“As the preview quality of these new methods is likely lower than that of a virtual met mast, power gains from preview control could be lower than illustrated in this work. However, given the substantiality of the gains demonstrated here, preview control using lower-quality wind direction signals still has the potential to provide significant power gains.” (Discussion, Sect. 5.4)

“This study introduces many new research questions, such as how to feasibly obtain preview information, how the quality of these forecasts affects the effectiveness, and how to use this information in more intelligent controllers. However, the results demonstrate that wake steering can benefit considerably from preview wind direction control, making it a topic worth pursuing in future work.” (Conclusions)

2. The paper focuses on a wind farm of two turbines. This makes the quantitative results even less valuable. Repeating all these LES simulations for a large wind farm would be hugely onerous, but it should be feasible with the engineering model.

The authors would like to point out that many studies in wind farm control start with a basic setup of a few (two) turbines. Before moving to the full scale, it is generally good practice to to simplify the problem at first to gain a deeper understanding. For this particular work, scaling up the problem raises the question where to place the upstream measurement point for downstream turbines. Simulating a full wind farm adds a whole new layer of complexity, which is considered out of the scope of the current work. Besides, the engineering model that was used in this study is based on a data-driven wake model which is currently not able to use disturbed (waked) inflow, see Sengers et al. (2021) for discussion. It is therefore currently not possible with this model to simulate a full wind farm.

Sengers, B. A. M., Zech, M., Jacobs, P., Steinfeld, G., and Kühn, M.: A physically interpretable data-driven surrogate model for wake steering, Wind Energ. Sci., 7, 1455–1470, https://doi.org/10.5194/wes-7-1455-2022, 2022.

The following has been added to Sect. 5.2:
"Additionally, simulations containing full wind farms with different layouts should be carried out to see how the benefit of preview control changes with scale. This increases the complexity of the problem, as it is for instance unclear where the preview information for downstream turbines should be obtained. This is further discussed in Sect. 5.4."

3. The conclusion that optimum preview time for wake steering doesn't change much is probably because it depends mainly on wind speed and the control algorithm's timestep and dynamic response, all of which were fixed in this paper, so it's not surprising.
The authors agree with the reviewer that the results depend on the yaw controller used. This is also discussed several times in the manuscript:
- Line 5-6
- Line 249-252
- Line 435-439
- Line 481-483

Wind speed likely affects the benefit of preview control, see also comment 2 from reviewer 1. It definitely affects the preview distance, but it is currently not clear whether it would affect the preview time.

4. The conclusion that preview is most beneficial when wind direction changes a lot is surely also self-evident.
The authors agree that this conclusion is not particularly shocking. However, as explained in the response to comment 1, this work attempts to quantify how big the benefit is to decide whether the topic of wake steering control using preview information is worth pursuing in future work. To not shine too much light on this conclusion, the text "especially when the wind direction change is rapid" has been removed from the abstract.

5. The paper compares against conventional or 'greedy' yaw control, but this is with fixed parameters which have probably never been optimised, and certainly not for different conditions, let alone for the case where preview information is available.
It is correct that the yaw controller used in this study contains fixed parameters that have not been optimized for the current analysis. However, based on the experience of the authors of what they have seen in terms of controllers installed in field, the considered yaw controller is quite standard. Besides, it is similar to the standard yaw controller used at NREL (e.g., Simley et al., 2020) based on the model described by Bossanyi (2018).
As is done in most wake steering experiments, an existing controller is simply adapted to include wake steering (and preview control in this study), while the main structure of the controller remains close to the original. Some implications of the yaw controller design on preview control, as well as the desire for more sophisticated controllers, is discussed in Sect. 5.

Bossanyi, E.: Combining induction control and wake steering for wind farm energy and fatigue loads optimisation, J. Phys.: Conf. Ser., 1037, 032011, https://doi.org/10.1088/1742-6596/1037/3/032011, 2018.

Simley, E., Fleming, P., and King, J.: Design and analysis of a wake steering controller with wind direction variability, Wind Energ. Sci., 5, 451–468, https://doi.org/10.5194/wes-5-451-2020, 2020.

However, in light of the implications of yaw controller design on the results, the following is added to Sect. 5.5:

“Aggressive controllers are better able to follow wind direction changes, resulting in higher power production at the cost of more yaw maneuvers; therefore, the added value of preview control might be small. Likewise, conservative controllers likely benefit more (i.e., higher power gain compared to SC) from the use of preview information.”

6. In a stochastic world the effects of preview can only be determined by running quite long simulations with realistic low-frequency changes in direction. The short simulations with linear direction change ramp don't seem to be worthwhile, even with the addition of high-frequency turbulence, as the conclusions drawn from these are quite self-evident.
   The authors agree that these theoretical linear wind direction changes are not very realistic and that the analysis of these results in Sect. 4.1.1 don’t contribute much to the final conclusions of the manuscript. However, it was chosen to show these results to start with simple examples before analyzing more complex and realistic scenarios. It builds the story and might help the reader to better understand the more complex results later on.

<u>Minor comments</u>

1. Equation 3: different authors use different definitions of thrust coefficient for yawed rotors. Which definition is used here, i.e. which thrust force component in the numerator and which wind speed component in the denominator?
   The methodology discussed here follows the example for the power coefficient in Eq. 5, which is common practice in wake modeling. First, the coefficient is calculated assuming no misalignment, where the wind speed is the horizontal wind speed and the thrust force points directly into the wind. This is in Eq. 3 indicated by $C_{\mathrm{T},\phi 0}$. In a second step, the coefficient is corrected using a cosine function with the yaw misalignment angle an empirically determined exponent. This second step is what is shown in Eq. 3. This way, the C_{T} for the yawed rotor is still assigned to the horizontal wind speed and not any of its components.
   “This is analogous to how the power coefficient is typically corrected for the yaw angle in wake modeling, see also Eq. 5.”

2. Section 3.2.3: the wind vane signal "was chosen ... to mimic a nacelle wind vane not disturbed by the rotor. Surely in reality the wind vane is very much disturbed by the rotor. Offset bias can be compensated for by calibration, but the yaw control dynamics could be strongly affected, which would be relevant here.
   It is correct that the nacelle wind vane signal is heavily disturbed by the rotor in reality. However, the objective of this work is not to quantify the impact of disturbed measurements, but rather the impact of the use of preview information. When placing one vane at a disturbed position (standard control) and one at an undisturbed location (preview control), one would not be able to allocate any effect to preview vs. standard control or disturbed vs. undisturbed data. For simplicity, we have therefore chosen to move the standard control wind vane to an undisturbed position. The following has been added to Sect. 3.2.3:
   “This allows for a fairer comparison of the model results, as the preview control wind vane (see below) is not disturbed by any rotor.”

3. Lines 203-7: is this physically realistic, given that cyclic boundary conditions are a modelling necessity, not reflecting reality? This is acknowledged later, but how much uncertainty does it add to the conclusions?
   The use of cyclic boundary conditions is common practice in LES, adding to the inherently imperfectness of simulations as pointed out by the reviewer below.
   As for the issues discussed in lines 203-207: it is at this stage not possible to quantify the added

uncertainty. As noted, since all experiments experience the same effect, its effect on the results is expected to be small. Additionally, Sect. 5.3 argues that future work should quantify this affect, or that this study should be repeated with a different simulation setup (different code) that does not have this issue.

4. The wording in section 4.3 seems to make an explicit assumption that the LES results are the 'correct' ones. The wording should be changed to be consistent with the fact that neither model is 'correct'. There may be justification for assuming that the LES model is likely to be more nearly correct, at least when the results are averaged over many specific runs, because of the simplicity and empiricism of the engineering model.
   ”Acknowledging that models are inherently imperfect, for this purpose the LES results are considered the truth.”

5. The word 'exemplary' is used many times in the paper, but I think this should be 'example'. The meaning is rather different.
   Agreed, this has been changed.

6. Line 207 typo: 'alters'.
   Fixed