Comments on the review of "Wake redirection at higher axial induction" - Reviewer 2

Carlo Cossu

Laboratoire d'Hydrodynamique Énergetique et Environnement Atmosphèrique (LHEEA)

CNRS - Centrale Nantes, 1 rue de la Noë 44300 Nantes, France

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I thank Reviewer 2 for his/her comments and suggestions and I appreciate the rapidity of the refereeing process. Each raised issue is addressed in detail below.

The reply process has been longer than expected because during the revision, in relation to the performed additional simulations and to a question raised by Referee 1 , I became aware that most turbine simulations input files were bugged leading to the inconsistent use of roughness lengths $z_0 = 0.15$ that were larger that used in the precursor simulation. I therefore had to rerun all the simulations with the correct value $z_0 = 0.001$, postprocess the results and redraw all the figures.

The manuscript has undergone a major revision, the main modifications being the following ones:

- All the presented results have been updated with the new simulations where the bug on the z_0 value was fixed. These corrected results, reported in the revised manuscript, are mostly similar to the previous ones so that the main conclusions of the manuscript do not change. Changes resulting from these new simulations are updated in the revised manuscript and are discussed, when appropriate, below.
- Additional simulation have been performed to analyze the role of the used turbine model, and in particular the effect of including wake rotation effects. The new results are presented and discussed in the new Appendix B and mentioned in the manuscript when appropriate.
- New figures have been added showing the mean streamwise vorticity and velocity fields in the cross-stream planes to highlight the role of the counter-rotating streamwise vortices forced by the tilt or yaw misalignement and discuss the role of wake rotation. A scheme has been added to define the tilt and yaw angles φ and γ.
- The abstract and conclusions have been modified to make them more clear following reviewers' suggestions.

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1. The abstract mainly provides general introduction and the past research of the author. Please discuss the findings of the current research. You can quantify and further discuss the power gains due to tilted rotor and yaw control.

I agree. The abstract has been largely rewritten accordingly.

2. It is not clear why you have considered just two rows of turbines. I do not see any technical challenge in simulating for wind farms with more rows.

I agree that the two-rows configuration is highly idealized and, as such, it is not representative of a typical wind farm and that technically I could have considered more turbine rows. However, the problem of considering many rows is that the results depend on the specific tilt/yaw angles enforced in each row. Thus, if I had considered more than two rows, the effect of increasing the induction in tilted/yawed turbines (which is the main message of this paper) would have been blurred by considerations/analyses of the optimal combinations of tilt/yaw angles to be enforced in each row. I have therefore chosen to consider only two rows for which the results in term of β and (a single value of) γ or φ remain relatively easy to interpret. The chosen configuration is indeed similar to the two-turbines case considered by Fleming et al. (2015) except for the fact that spanwise periodic distributions of turbines (the two rows) are considered instead of only two turbines. This was explained in the original manuscript (lines 61 to 64).

But in case you think two-row wind farm simulation is sufficient, please discuss how you can link your findings to larger wind farms.

Actually, I do not think that two-rows simulations are sufficient but that they are necessary. Indeed, in addition to the considerations discussed in point n.2, one should also consider that the computation of the optimal overinductive tilt or yaw control for realistic turbine arrays, where the optimal combination of tilt, yaw and pitch angles of all turbines has to be computed for a large number of wind directions and intensities, would be too computationally demanding if performed by means of large eddy simulations. This type of analysis is customarily based on less computationally demanding simplified sets of equations where the accurate modeling of the controlled wakes is of primary importance. In this context, the results presented in the present study should be used to improve/validate the existing simplified wake models in moderate to high-tilt/yaw and pitch angles regimes, particularly in the case of significant overinduction. Indeed, simplified models which are unable to reproduce the power gains results for the set of φ, γ, β combinations and the idealized two-row array considered here would probably be unfit to predict annual power gains in more realistic settings.

This said, I agree, however, that the link between the studied idealized array and realistic configurations was not clear enough in the idealized manuscript. I have therefore summarized these points in the conclusions of the revised manuscript (lines 228-237).

3. Line 81 to 83: Please add more explanation to clarify the relation between β and C'_T. You can use blade element momentum (BEM) theory in order to describe the relations between β, lift and drag coefficients and the thrust coefficient.

Additional explanations have been added to Appendix A (lines 265-270).

4. Line 84 to 91: Add LES and other relevant equations.

As in this study I have used the standard SOWFA code without changing its formulation, I prefer to refer the reader to the original papers to keep the focus on the main scopes of the study. The same is done in the many related studies based on SOWFA such as the cited ones of Fleming et al. (Renew. En. 2014) and Fleming et al. (Wind En. 2015) who also refrain from reproducing all the details of the formulation and refer to Churchfield et al. for the full details on the used formulation, including the LES equations. However, I agree, that section 2 lacked even some of the most basic information. This is fixed in the revised manuscript where more details on the used model (filtered Navier-Stokes equations with Boussinesq approximation) are now mentioned in section 2, lines 88-92 (they were briefly mentioned only in Appendix A in the original manuscript).

Adding a schematic for the computation domain will be helpful too.

The velocity fields reported in Figs. 1 and 4 of the original manuscript (Figs. 2 and 6 of the revised manuscript) show the full computational domain. This is now mentioned in the revised manuscript (line 118 and near the end of the caption of Fig.2).

5. This may be beyond the scope of this manuscript, but how practical do you think it is to tilt blade by $\varphi = -30^o$? Higher tilt angle will significantly increase the flapwise bending and reduce the blade lifetime. You have mentioned about gravity load in line 137, but that is not very clear.

I agree that the mention of loads, and gravity loads in particular was unclear in the original manuscript. I have removed it from the revised manuscript to avoid a potentially misleading only partial discussion of the structural turbine loading.

I also completely agree that the issue of the practicality of tilting turbines is important and deserves further investigations that are beyond the scope of this paper. However, let me note that given that positive tilt is not immediately implementable in most of the installed horizontal axis wind turbines with upwind-directed rotor, possible drawbacks of the tilt on blade loads could be addressed in the design phase of a new generation of turbines with downwind-oriented rotors and highly flexible blades such as those discussed by Loth et al. (Downwind pre-aligned rotors for extreme-scale wind turbines, Wind Energy, 20, 12411259, https://doi.org/10.1002/we.2092, 2017).

That additional studies should consider the influence of overinduction combined to tilt and yaw on loads and the full aeroelastic response of the blades is now mentioned in the revised manuscript (lines 222-227).

6. Line 116 to 122 and Figure 3(a): I do not understand why increasing β (making it more negative) increases the power from the first turbine row. Wind turbines are usually optimized for the pitch angle around 0^o. If that is the case with your turbine too, power output should be lower for $\beta < 0^o$. Increased thrust coefficients -for negative blade pitch angles- are simply caused by increased drag coefficients, and they will not necessarily translate into the higher power output.

This is an interesting point [you are probably referring to Figure 2(b)]. I agree that it seems strange that more power can be produced by first-row turbines for the suboptimal values $\beta < 0^o$. There are however two reasons that can explain this apparently counterintuitive result:

(a) For the NREL5 turbine $\beta = 0^o$ corresponds, by design, to the maximum C_P (at the optimal wind-tip speed ratio) but only for the reference case $\gamma = 0^o$, $\varphi = -5^o$ for which the optimization was performed. However, the data in Figure 2(b) do not pertain to reference values but to $\varphi = 30^o$ for which there is no guarantee that the maximum C_P is obtained for $\beta = 0^o$.

(b) For the case of a (single) row of closely spaced (non-tilted/non-yawed turbines) Strickland & Stevens (Effect of thrust coefficient on the flow blockage effects in closely-spaced spanwise-infinite turbine arrays, J. Phys. Conf. Ser. 1618, 2020, doi:10.1088/1742-6596/1618/6/062069) show that "the power production of turbines in the row increases approximately linearly with C_T' when compared to the production of a free-standing turbine". It is therefore possible that also in the present case the slight blockage effect of the first row increases when C_T' is increased leading to an increase in the power production.

A note mentioning this has been added to the revised manuscript (bottom of page 7).

7. You have not discussed how the tilt control and the yaw control influence the flow fields inside the wind farm. How do turbulence fields and shear stresses change as a result of those controls should be presented.

I do not completely agree that I have not discussed how the tilt control and the yaw control influence the flow fields inside the wind farm because this is precisely what was done in Figs. 1 and 4 and the related discussion. Additional flow fields and discussion have, however, been added to the revised manuscript where the mean streamwise vorticity and velocity fields are now shown in crossflow planes in Figs. 3, 7 and B1 in order to better discuss the role of wake rotation.

I have shown the mean streamwise velocity fields because they are the ones which influence the mean power output which is the main subject of this study. I do not show the turbulence fields because they are mainly relevant for the analysis of the power and load *fluctuations*, an analysis which goes beyond the scope of the present study. However, that additional studies of load fluctuations for the presented overinductive tilt and yaw control is now mentioned in the revised manuscript (lines 222-227).

Minor comments and corrections: 1. Line 10: an high $\rightarrow$ a high.

Right. This is fixed in the revised manuscript.

2. Line 8: of the produced $\rightarrow$ of that produced

Right. This is corrected in the revised manuscript.

3. Line 110: Is it $\varphi = -30^o$? You can add a schematic describing positive and negative directions for yaw, tilt and pitch angles.

Actually, it is a positive tilt angle $\varphi = +30^o$ (see e.g. the discussion of Fleming et al. 2015 who write "With a positive tilt angle, the rotor would face downward, and for conventional upwind turbine designs, this would cause the blades to hit the tower").

A schematic describing positive and negative directions for yaw and tilt has been added in an additional figure (Fig. 1 of the revised manuscript). The schematic for the rotor collective pitch angle has not been added because it would have required a long discussion to avoid misunderstandings (indeed in a plot one should also discuss the local twist angle, the aerodynamic angle of attack and its definition, etc.). These angles are now mentioned in Appendix A (lines 267-270).

I hope to have clarified the main issues raised in the report. I thank again Reviewer 2 for his/her many remarks and suggestions which have helped to improve the manuscript.