Dear Editor, dear Reviewers,

thank you very much once more for your comments and for the time dedicated to this work.

In the following we go through your *comments* and provide, for each one, both our *answers* and any *actions* we took to comply with your suggestions.

We welcome any further comment and suggestion from your side.

Best regards,

The Authors

Reply to Reviewer #2

Suggestions for revision or reasons for rejection
None

Answers
We thank again the Reviewer #2 for the time dedicated to this work.

Actions
None.

Reply to Reviewer #3 - Vasilis A. Riziotis

Suggestions for revision or reasons for rejection
The paper has significantly improved since the first submission.
The authors convincingly responded to all my comments/questions.
Moreover, the language has been greatly improved. However, I still find small grammar/syntax errors here and there.
A final careful reading is recommended. I indicated some of the errors in the attached pdf.

Answers
Thank you for this comment. We went through the document and correct the typos/errors highlighted by the reviewer.

Actions
We revised the whole text also from the point of view of language, grammar and typos.

Reply to Reviewer #4

Suggestions for revision or reasons for rejection

The topic is interesting and relevant to the community, but the approach - in its present form - is too simplistic to give meaningful/useful results. It's difficult to justify that a paper dealing with wind farm control is build around a 'showcase' consisting of only one wind turbine. Thus, the paper will benefit from taking a more focused approach - e.g. to be condensed to the first part of the paper- and then work this through in more details, however, still using a simple showcase (consisting of minimum two wind turbines)..

Answers

We thank you again the Reviewer for her/his comment. We fully understand the Reviewer's vision, but the goal of this paper is to look at the wind farm control problem from a wind turbine design point of view. As it will be clear from the reply to the next comments, this implies, according to the International Regulations, that one has to consider the worst possible scenario. From a practical point of view, this leads to a formulation of the problem which does not eventually require (at least for the purpose of our investigation) a simulation of the whole farm flow. At the same time, in line with the reviewer's comment, some of the authors assessed the problem from the point of view of wind farms in another work presented at TORQUE2020 and soon to be published in a journal paper. What is described in this paper remains, in our opinion, of fundamental importance to show the impact of the wind farm control techniques on the design of the wind turbine.

Actions

No action at this point.

Reviewer comment:

This paper falls in two parts. The first part presents considerations of the impact of farm control on an individual wind turbine quantified in term of fatigue and ultimate loads as well dynamic blade response under extreme conditions using a simplistic showcase. Two examples of active wind farm wake control are in focus - wake re-direction and dynamic induction control. The second part uses the results obtained in the first part to perform a wind turbine blade re-design with the purpose of ensure structural integrity under increased loading of the solitary wind turbine investigated in the first part.

The topic is interesting and relevant to the community, but the approach - in its present form - is too simplistic to give meaningful/useful results. Roughly speaking the paper is composed of two 'half papers' (cf. more detailed comments below), and would benefit from selecting a more focused topic (e.g. first part) and then work this trough in more details, however, still using a simple showcase. In its present form, the paper is not ready for publication in the Wind Energy Science journal. Detailed comments/suggestions are given below.

Answer:

The Reviewer accurately described the main content of the paper, but we share only partially her/his opinion that the manuscript is composed by two "half papers". On the one hand the paper is certainly composed of two parts, which, taken individually, could be extended, but on the other hand we believe that we can have a correct overview on the impact of wind farm control in terms of wind turbine design only by putting these two parts together as we have done. In fact, from a design point of view, one is interested in evaluating the machine loading in different scenarios according to Standards and then in designing the machine considering the worst possible conditions. The mainly focus of this research is exactly this one, i.e. to evaluate the possible impact of wind farm control strategy(ies) on the rotor design, an important industrial and research topic which, according to our best knowledge, has been never addressed within the current literature. In this design framework, it is possible to understand our choice of considering, in this first paper on this topic, the single upwind wind turbine for the investigation, without detailing possible (to be carefully evaluated) reduction in downstream machine loading. This point, which, as far as we understand, represents the major concern of the Reviewer, will be detailed in the rest of this point-by-point reply.

Action:
No action at this stage.

Reviewer comment:
Heading: Due to the simplicity of the selected showcase - where only loading of a single wind turbine in a non-waked inflow condition is considered - the wording 'wind farm' should be toned down, and the heading modified to e.g.: Aspects of the impact of active wake control on fatigue and ultimate loads for a 10 MW wind turbine.

Answer
This comment is similar to one of Reviewer #3 during the first round of review. As in that occasion, we are open to modifying the title not to create a misleading expectation in the reader

Action:
We propose to the editor to change the title in "Evaluation of the impact of active wake control techniques on fatigue, ultimate loads and rotor design for a 10 MW wind turbine"

Reviewer comment (Scope - 1):
Following the comments in the introduction, the paper could benefit from narrowing down the scope to e.g. consider only the first part (i.e. impact of active wake control on wind turbine loading and dynamics), and then treat this topic more elaborate. This can e.g. be done simplistically by considering the smallest possible wind farm - a two-turbine setup - allowing for analyses of important wind farm load and production characteristics such as wind turbine spacing and wind turbine offset relative to the mean wind direction (i.e. full wake, partial wake cases).

Answer:
Here again, we strongly believe that the paper should have these two lungs, impact on turbine loads and on rotor design, and a single hearth, evaluating wind farm control effects at turbine level from the perspective of the design compliant with current Standards. We believe the new proposed title could emphasize exactly this.
Dealing with the fact that the analysis should consider also downstream (waked) turbines, this comment was similar to one of Reviewer #2 in the first round of review. Of course, we agree with this consideration, but the focus of the paper is slightly different. We certainly know that wake redirection may have a positive impact on downstream machine loading, but from the design standpoint, one is interested in the worst possible case, which happens, as showed in this work, when the turbine is upstream. Similarly, an analysis about turbine spacing, although extremely interesting, is not necessary in this paper. In fact, any turbine is designed once and then is supposed to operate in different geographical locations, different farms, and different positions within the same farm.

Action

We stress even more this concept by adding the following sentence in the introduction immediately after the declaration of the scope of the paper.

"As the design of a new rotor has to be carried out according to the Standards and has to consider the worst possible scenario, the analyses in this paper will focus on the isolated upstream machine, under different farm-related operating conditions. In fact, as it will be pointed out in detail in Sec. 2, in the simple case of wake redirection control, upstream turbines are more prone to the negative impacts of the farm control, e.g. those entailed by operations at large yaw angles, while the downstream ones will possibly experience all the advantages, e.g. lower turbulence and lower wake impingement with respect to the case without wind farm controllers.

Clearly, in a single farm, there is a subset of machines which most often see a clean flow, i.e. the outer ones exposed according to the most probable wind direction, and another subset of turbines, the inner ones, which sometimes see a waked flow. In this scenario, it is certainly interesting to evaluate a possible usage of partially customized or totally different turbines in a single farm, depending on the specific machine location. In such a case, the turbines proposed for the innermost farm locations may be characterized by more competitive designs thanks to the farm control. Although extremely interesting, this idea falls out of the scope of the present paper."

Reviewer comment (Scope - 2):

The premise for the wake re-direction case in the first part of the paper is that wake effects of downstream wind turbines can be completely mitigated. This is usually not the case. The premise is established based on a stationary flow model (cf. Fig. 1). Stationary modeling of the inflow field makes sense for production prediction. However, for load simulations wake dynamics is important, and un-steady modeling of wind farm flow fields is needed. This stochastic dynamics comes on top of the static flow illustrated in Fig. 1, and the consequence is that, even in the case of considerable reduced wake production losses, non-neglectable wake loading of downstream turbines may occur. The 'efficiency' of wake re-direction is usually of the order of 0.5D at approximately 10D downstream - somewhat less for densely spaced wind farms. Therefore analyses of different wind farm spacings should be performed.

Answer:

Figure 1 is not to be viewed as a premise for wake redirection case, but rather as a simple investigation to derive some reasonable pieces of information related to the region of activation of farm control in terms of speed and TI, and compare such conditions with those prescribed by Standards.

We agree with the Reviewer that loading of impinged rotor is to be evaluated considering a dynamic wake. In fact, we have already started a dedicated analysis and some preliminary results are already available (see https://doi.org/10.1088/1742-6596/1618/6/062033).

Action:

To stress this, we added the following sentence in the conclusions.

"In terms of extensions of the proposed work, the evaluation of the impact of farm control on ultimate and fatigue loads of downstream turbines is certainly an interesting topic, which deserves dedicated analyses with more sophisticated tools than those used in the present work to simulate the wind farm flow, e.g. CFD or dynamic wake models."

Reviewer comment (Scope - 3)

The second part of the paper (Section 5), which is which is anyway limited to the PCM control strategy with parameters apparently somewhat arbitrary - or at least not motivated in terms of cost efficiency - could be the topic for another paper based on more detailed findings from a focused first part of this paper.

Answer

We believe that the paper, without the design part, would be incomplete according the very scope of the work, as we detailed in the answer to a previous comment.

PCM control parameters have been selected according to the preliminary sensitivity study. Moreover, the range of variation of frequency and amplitude, has been selected from a set of experimental activity in wind tunnel presented in a previous paper (see https://doi.org/10.5194/wes-5-245-2020).

Action:

A sentence at the beginning of Sec. 4.2.2 is modified and now reads:

“Different combinations of amplitude A_PCM and Strouhal number St were considered: the range in amplitude was set between 1 and 4 deg, whereas the range of Strouhal between 0.2 and 0.5., according to the findings of an experimental campaign in wind tunnel (see Ref. Frederick et al., 2020)”

Reviewer comment:

Wind speed limitation: In the present study, the wind farm controller (i.e. the wake steering) is only active for wind speeds lower than 15m/s. This makes perfect sense for production optimization, but for load mitigation of e.g. a system consisting of two wind turbines, where one is operating in the wake of the other, this is less obvious and should be motivated. It is a weakness of the paper that only possible increased loading of the wake generating wind turbine is considered without considering the possible load reduction of a downstream wind turbine.

Answer

As written in one of the previous replies, our choice is to look at the wind farm control problem from the design standpoint, and, accordingly, we consider the worst possible scenario. We do not believe that this is a weak approach, and this is particularly evident for ultimate loads. Consider the case of wake redirection and, as usual, all turbines of the same type in the farm. We may have an increase of a specific ultimate load in the upstream turbine, and a reduction in downstream one(s). Since all turbines are of the same kind and we have to select the highest load, the design process results to be blind respect the positive impact in the downstream machine.

Action

No action.

Reviewer comment:

Dynamic induction control: In dynamic induction control, the magnitude of the thrust force of an upstream turbine is varied, which leads to increased power and thrust variations. This in turn negatively impacts power quality and fatigue loading of the wind turbine subjected to this type of active wake control. Therefore, maybe a comparable approach - the helix approach - could be considered. Investigations (https://onlinelibrary.wiley.com/doi/full/10.1002/we.2513) show that this approach leads to enhanced wake mixing (like dynamic induction control) with minimal power and thrust variations.

Answer

We knew and had already read the suggested paper. Interestingly, the helix approach paper and ours were submitted to two different journals exactly the same day. Hence, we could not consider the helix approach in the development of this work. All in all, we do not expect that helix approach could significantly ameliorate the blade loads. In fact, with IPC the single blade loads may undergo significant changes, while the thrust (being the sum of the thrust of the three blades) may be subjected to a limited modification. On the other side, the impact of the helix control on the tower, directly connected to the thrust, can be actually smaller than that of collective dynamic induction control. In any case, we would like to thank the Reviewer once again for this further suggestion, which we will certainly look into in more detail in future work.

Action
We inserted a reference to the helix approach paper in the 'Conclusion and outlook' section.

Reviewer comment:
Ultimate loading: The paper claims to deal with wind farm ultimate loading but in reality only deals with ultimate loading of a solitary wind turbine under various (wind farm related) operational conditions. Addressing wind farm ultimate loading requires in addition extreme loading of a wind turbine operating in a waked flow field to be considered.

Answer
See our answer to a previous comment.

Action:
No action required.

Reviewer comment:
Choose wind farm or WF throughout. Choose wind farm control or WF control or WFC throughout.

Answer
We agree, and since in some statements we think it is more useful to use the full expression rather than the acronym, we have removed the latter from the text.

Action
We removed the acronyms WF and WFC.

Minor Reviewer comments
We skip in the reply of these minor comments, the request for correction of typos. These corrections have been applied directly to the final uploaded version. We would like to thank the Reviewer once again for these suggestions.

Editorial, semantics and minor comments
- P.1: "…production, possibly weighted with the wind Weibull": Inclusion of wind direction and wind speed pdf's are a minimum for a trustworthy estimate wind farm production.
We changed "possibly" with "properly"

- P.2: A number of studies dealing with the impact of WF control on WT fatigue loading (Cardaun et al., 2019; Ennis et al., 2018; White et al., 2018; Boorsma, 2012; Damiani et al., 2018; Zalkind and Pao, 2016) is mentioned, but the results of these studies are not shared with the reader.
We included a brief description of those findings where needed.

- P.2: … the possible increase in machine loading induced by wind farm control: I would actually expect that wind farm control aiming at increased production in most cases will lead also to load mitigation due to less severe wake effects.

See reply to a similar previous comment.

- P.3: … wind farm controller (WFC) has on the single wind turbine (WT). Why not introduce these acronyms in the introductory section, where 'wind farm control' and 'wind turbine' are also mentioned.

See answer to a previous reply.

- P.3: WFC is highly site specific. YES, INDEED!!

Indeed, but according to the vision from a design standpoint we are looking at the worst case, information of general validity and not site-/farm-specific.

- P.5: … in order to provide an analysis of general validity: I'm not convinced this is possible for a case study consisting of very few WTs. This statement needs at justification/motivation.

Again, this sentence can be fully understood, as detailed in the reply to general comments, looking at the problem from the design standpoint. Aspect, which is already written multiple times in the manuscript, including the part which immediately follows the sentence "in order to provide an analysis of general validity", at the end of section 2.

- P.5: if only because a machine → because a machine

Changed as suggested even if the previous sentence was correct.

- P.8: … simplify the analysis and make it of general validity, the farm control is considered active only in a range of wind speeds: This intuitively a good idea for power optimization - less intuitive for load mitigation.

See a reply to a similar previous comment

- P.9: only for wind speed lower than 15m/s the wind farm controller (i.e. the wake steering) is active: Justify that this is a sensible choice for WFC when also WT loading of downstream WTs is taken into control - see e.g. IOP Conf. IOP Conf. Series: Journal of Physics: Series: Journal of Physics: Conf. Series Conf. Series 1102 (2018) 012019 1102 (2018) 012019 doi :10.1088/1742doi :10.1088/1742--6596/1102/1/0120196596/1102/1/012019 in which optimal yaw strategies are defined for above rated wind regimes, where no power loss occurs.

A sentence has been added to clarify this point, together with the reference of the proposed article.

- P.9: lacks in generality → lacks in generality and will be highly dependent on the site wind rose

We corrected the text in "lacks of generality". The problem is not related to the wind rose, but to the fact that the tower has a cylindrical shape and hence evaluating the modification in a direction does not tell the truth about the actual loading of the tower, as explained in the manuscript.

- P.9: … rotated of an angle → … rotated an angle

We changed it in "rotated by an angle"

- P. 15: PCM: … impact of such a pulsating flow with downstream machines can be significant in terms of turbine loads and aero-servo-elasticity. This particular study, out of the scope of the present paper. BUT IT SHOULDN'T - same comment as for the wake re-direction!

See a reply to a previous comment.

- P.18: How does the increased PCM induced loading balance with the potential increase in power production in a COE context? This is the crucial question every wind farm owner will address - why should they otherwise accept the increased loading shown in Table 2? It makes no sense to include the detailed analyses presented in Section 5 as long as this basic question isn't addressed. As an illustration, it is stated that "on the other hand, only the PCM with amplitude of 2 deg., the impact of PCM and WR becomes comparable” - yes correct, but what is then the sensibly choice in a WF control context … and why?

The question has been posed in the introduction. In the paper, we showed that non negligible increase in blade mass is to be considered if one includes wind farm control in the design process. This is not a go/no go assessment. It is simply an indication to be considered in the farm control optimization and hence in the final cost of energy of the complete wind farm. As explained in the introduction, the problem of wind farm control is the one of minimizing the cost of the energy, which is a more proper merit figure than the maximization of AEP or maximization of AEP with some constraints about fatigue. Clearly, in this paper, we have not solved the entire problem, but have rather provided an indication.

- P.18: skip Sec. 5 (which is anyway limited to the PCM control strategy with parameters apparently highly arbitrary - or at least not motivated in terms of cost efficiency) and
elaborate more on Sec.4 to approach more complete picture of WF loads associated with active wake control.

See reply to a previous comments

- P.20: Baseline → baseline
- P.21: Baseline → baseline
- P.21: Redesign PCM → re-designed PCM
- P.22: Baseline → baseline
- P.22: Redesign PCM → re-designed PCM

"Baseline" and "Redesign PCN" are used in this context as proper names identifying the specific rotor. We believe these names should be maintained as they are.