Dear Editor, dear Reviewers,

thank you very much 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 the *actions* we took to comply with your suggestions.

We will welcome any further comment and suggestion from your side.

Best regards,

The Authors

Reply to Reviewer #1

Comment
This is an interesting topic, and the conducted research and associated conclusions are worthy of publications. But the clarity of the writing is quite poor. There were many sentences that i fundamentally could not understand, and most of the paper has rather awkward English. It really needs a review and edit by a native speaker. Once that is complete, i would be willing to re-review, and i think that will go smoothly since the actual research is quite interesting.

Answers
We thank the Reviewer #1 as he or she has acknowledged that content and findings of our research is interesting and worthy of publication. With the aim of replying to the other Reviewers, we modified the manuscript, and, at the same time, as suggested by Reviewer #1, we reviewed the text so as to improve its clarity and grammar.

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

Reply to Reviewer #2

Comment

This paper is in a very relevant and active research area of wind farm control. Many studies have been published in the last decade focused on the benefits of different wind farm control strategies, such as wake redirection and induction control. However, there has been only limited attention on the impact of wind farm control on the loads of individual wind turbines.

These papers have not been cited well enought in the introcution section, and the actual contribution of the paper is not clearly motivated by building up on what is already known.

Answers

Thank you for this comment. Regarding the bibliography, we reckon we may have missed some important references, especially the most recent contributions. According to your feedback, we have updated the survey of the state-of-art by including additional recent sources. We also tried to further clarify the scope of this paper in terms of its objectives and innovative contents.

Actions

We have improved the state-of-the-art analysis related to this work and hence added the following references to the introductory discussion:

- Boorsma, K., 2012
- Damiani et al., WES 2018
- Zalkind and Pao, ACC 2016
- Mendez Reyes et al., WES 2019
- Kanev et al., Wind Energy 2018

We also edited the Introduction and the Methodology sections to accommodate the new references and to better clarify the scope of this work.

Comment

My most important comment, however, is the scope of the study. Fatigue loads in a wind farm cannot be evaluated on a single wind turbine level. A single turbine operating with yaw misalignment in free stream as first in a row may increase its loads for some yaw misalignments. Behind the turbine, however, the downstream turbines might benefit reduced fatigue loading as the wake is moved away from them. Considering one single turbine in free stream does not provides completely no basis for drawing conclusions on the overall fatigue loading. Instead, all wind directions must be considered in a detailed analysis, including the proper wind speed and direction distributions.

Answer

We completely agree on your consideration that a proper fatigue assessment should go far beyond the limited framework considered here. In particular, we are fully aware that fatigue might be reduced on downwind turbines and that an overall evaluation of the impact of fatigue on the wind farm requires much more detailed analyses.

However, what we did in this paper was to treat fatigue according to the current standards. This means that, for example, in our analyses the actual TI depends on the wind speed rather than being a fixed value. We also assume that the chosen WFC is constantly activated so that the worst-case scenario is considered. In our opinion, this is a sound approach to evaluate the impact of fatigue on the individual wind turbine as part of our parametric analyses. Besides that, our results clearly show that a non-neglectable contribution to the fatigue loads *with* WFC is related to the increase in the rotor mass so that fatigue is actually an *indirect* consequence of the WFC.

Actions

We have tried to better explain our point of view on fatigue in the "Introduction" and "Methodology" sections.

Comment:

Therefore, I cannot approve the paper unless one of the following modifications are made:

• fatigue loads are completely removed and the discussion focusses on ultimate loads only (which I would recommend), or

• it is clearly stated that the fatigue load analysis performed is too limiting to draw conclusions about the overall impact on the lifetime fatigue loading on any turbine in the wind farm, and that further research is needed to study these effects. In doing so, the presented results are to be compared to earlier results by other researches. Notice that the fatigue results, as they stand now, will still not provide much added value in terms of what is already published

Answers

As we discussed in the previous comment, we are aware that our description of fatigue does not allow to draw general conclusions about the whole wind farm. However, once again we believe that this procedure is fully appropriated to conduct our parametric and design analyses. As mentioned, our description of fatigue is compliant with current standard and allows to characterize fatigue loads on the individual wind turbine adequately. In this paper, in fact, we did not focus on other turbines, as this will be the topic of a future continuation of this work. Since our ultimate goal is to quantify the redesign effort, we think fatigue is an important part of this work, as the final structural design is heavily dependent on both ultimate and fatigue loads. On this basis, we have tried to follow your second suggestion, that is, we tried to provide a better framing of our fatigue analyses by discussing its limitations and possible alternative approaches.

Actions

As written in the previous two points, we have included a survey of other research in the field of fatigue analysis of wind farm and provided a qualitative comparison of the main assumptions and limitations. We also clarified why our current approach is, in our opinion, fully compatible with our scope. Moreover, we added a sentence to the scope of the paper to emphasize that the investigation in terms of ultimate loads and maximum tip deflection represents the major source of novelty of our paper.

Comment

The part of the paper related on ultimate loads is relevant and novel. In my opinion, this should be the focus of the paper. Also, here, the story needs to be put in the right perspective. You can't just claim that because of the sensitivity type of analysis, the results and conclusions can be generalized to any wind turbine type. This is obviously not true, as extreme loads depend on many aspects, such as turbine aerodynamics properties, wind conditions, and (supervisory) control system. This needs to be mentioned.

Answers

We fully agree on this. In fact, we specified multiple times that the findings of this study only apply to the turbine under consideration, and that the results hold true for a rotor design constrained by tip deflection. As we reckon that some doubts can still persist, we tried to further clarify this in the Conclusions and we also have updated the title of the paper to further stress that the analyses have been done on a 10MW wind turbine.

Actions

We have rewritten wide parts of the conclusion stressing out that the findings are limited to the case under investigation. We also changed the title to better reflect the scope.

Comment

The part on blade redesign is obscuring the focus of the paper, and given its current length, I propose to completely remove Section 5. Same holds for Sections 3.2 - 3.3 (also related to redesign), although Table 1 should remain detailing the load cases.

Answers

As we discussed in the paper we believe that, given the scope of this study, the part on redesign is fundamental. It gives a quantitative evaluation of the redesign effort required by the adoption of a WF controller. Although these results can be preliminary or limited by all the considerations above, to our knowledge it is the first time a realistic mass increment has been computed with a state-of-the-art design tool. We acknowledge, however, that the section describing the design procedure probably gives too much information and could be shortened.

Actions

After carefully discussing your suggestion among us, we decided eventually not to remove the design. However, we have shortened significantly the description of the design procedure. Specifically, we have merged sections 3.1, 3.2, 3.3 into a single section to condensate those parts.

Comment

Furthermore, there are lots of typos and other errors, please revise the language toroughly before resubmitting.

Answers

Thank you for pointing this out. We will have the manuscript proofreaded before the new submission.

Actions

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

Reply to Reviewer #3 (V. A. Riziotis)

Comment

The paper assesses the effects of two particular wind farm control methods i.e. WR and DIC on the ultimate and fatigue loads of the turbines that the wind farm controller commands to take control action (usually the ones of the front rows of a wind farm). Then they re-design the rotor blades for the DIC one which proves to be the most critical, both in terms of strength and fatigue.

In the reviewer opinion the paper addresses a very interesting and important topic and deserves publication after some revision is made to the original text, including the re-polishing of the language (among others some suggestions for language corrections are given in the supplement pdf).

Answers

We thank the Reviewer because he provided a concise but precise description of the research. We have fixed and improved the language throughout the paper.

Actions

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

Comment

Please see below my main concerns and points to be further elaborated in the revised text:

1) By reading the title, very large expectations are created to the reader, that the actual impact of the wind farm control on the design loads will be assessed. However, as explained in section 1 and 2, the work turns out to be a parametric study of the effect of i) yaw misalignment and ii) periodic collective pitch angle variation on design loads. Finally the re-design of the blade is only needed and performed for the latter. In order to support originality of the proposed work I would recommend the authors to try to link the conditions scanned in 4.1 and 4.2 with the actual expected conditions in the occasion of wind farm control.

Answers

We acknowledge that the title could be misleading and, after an internal discussion, we will propose to the Editor to modify it to give a better representation of the contents, the final one should be “Evaluation of the impact of wind farm control techniques on fatigue and ultimate loads for a 10MW wind turbine”. As you point out, the scope is to provide an evaluation of the impact of (some) WFC on the driving loads. In this view, we believe our methodology could go in the above direction. In fact, the preliminary parametric analysis is fundamental to study the behavior of the chosen WFC and to identify which settings are the most demanding. Clearly this activity could be extended to other WFC techniques, but we decided to focus only on PCM and WR because they are by now common and widespread techniques. As for the redesign, we only present the PCM because PCM has the strongest impact on the sizing loads (see Table 2). Regarding your last suggestion, our goal was to provide a general analysis by considering all the possible conditions and without referring to a specific case (wind farm layout, wind rose etc…). Clearly, we are probably working under an over-conservative assumption but, in our opinion, this is helpful in assessing the general impact of certain WFC before dealing into case-specific scenarios. Additionally, the map between the conditions scanned for different yaw angles and PCM frequencies can be performed following the work of Zalkind and Pao, ACC 2016. This reference, not present in the original version of the paper, is now mentioned in the manuscript to further stress this point.

Actions

To clarify the scope of this study, we better clarified our assumptions and tried to better describe our general approach. We also made it clear that the conclusions have some generality but the actual impact of any WFC should be evaluated on a case-by-case dedicated activity.
To avoid any mismatch between the title and the contents of the paper, we also modified the title.

Comment
2) There are several independent studies which indicate that overall, yaw misalignment, positive and negative increase the DELs ofthe flapwise bending moment (especially as it increases towards -/+20-30deg). In the reviewer opinion, some more convincing explanation of why this is not predicted by the present work must be given (e.g. some time series plot explaining this reduction etc.).
A reference on the yaw correction model used in Cp-lambda is also missing. This is very important in order to accurately predict loads variations in yaw. Moreover, it is stated that the DELs decrease because of the reduction in the mean value of the load. It would be nice to provide the formula of the DEL calculation used by the authors, as the standard one, that the reviewer considers, does not involve the mean load value but only the ranges of load variations. Furthermore, it would be nice to provide the DEL reference frequency and exponents used in the different components DELs calculation (i.e. blades and tower).

Answers
We modified section 4.1 to clarify and/or correct the text as requested. In particular:

1. There are many factors that significantly affect the fatigue loads, and the final result depends on how all these factors mix together. For instance, the yaw misalignment reduces the effective wind blowing on the rotor (i.e. the wind perpendicular to the rotor disc). The turbine controller, in the below-rated region, reacts to this, by trimming the machine at lower rotor speeds, pushing at a lower frequency all the deterministic loads (i.e. those due to wind shear, blade weight, etc.). Therefore, this leads to a reduction of the loading cycles, considered in the fatigue computation. But, at the same time, the yaw misalignment, by itself, generates a crossflow which, in turn, creates an advancing/retreating blade effect. This increases the load oscillations on the blades and hence the DELs. Finally, this advancing/retreating blade effect is made not-symmetric with respect to the yaw angle due to the wind shear. In fact, when the advancing blade is on the top or on the bottom, it experiences a different wind speed value due to the wind shear. For these reasons, it is extremely difficult to envision a priori the effect of yaw misalignment on fatigue loads. It is even more difficult to compare our results with those already published, as seldom the conditions used for computing fatigue are so detailed. But the results we obtained are compatible with what has been already seen in the bibliography. For instance, a reduction of fatigue loads for positive misalignment was already obtained in other works (Ennis, TORQUE 2018; Boorsma, ECN technical report 2012). These aspects are better explained in the revised text, along with the citation of some results present in literature.
2. Some sentences have been added to clarify the computations of loads of our simulator in the case of yaw misalignment.
3. We used the standard formula for DEL calculation. The reviewer is right, as the sentence “in a misaligned configuration the flow velocity perpendicular to the rotor disk is lower, and entails lower loads on the machine” (section 4.1) is imprecise and has been reformulated.
4. DEL frequency and exponents have been reported in the text as suggested.

Actions
This section of the paper has been modified according to the above answers. We have added a comment to figure 3-left and a comment to the yaw model as well as numbers for the DELs calculations.

Comment
3) With respect to DIC the results are as expected. One point that perhaps needs some more attention is to give an indication of how far these perturbations in the wake flow generated by the upstream turbines, travel. Of course they facilitate mixing in the wake but do you also expect a fast decay of the low frequency coherent

fluctuations introduced by collective pitching? These may cause additional low frequency wind inflow variation to the downstream turbines which if it exceeds the levels of the ambient turbulence might increase their fatigue. Is there an indication on the above? Could that be important to take into account?

Answers

We totally agree with the reviewer. Clearly, the analysis suggested by the Reviewer is extremely interesting but requires a CFD study, which is out of the scope of the present paper. However, we have already started a dedicated investigation on the impact of DIC on downstream turbine aeroservoelasticity based on SOWFA+Fast simulations. A conference paper is under review, while a journal paper is in preparation. Preliminary, we can say that such an impact may be non-negligible in terms of turbine and trimmer performance, as actually inferred by the reviewer. We added some sentences in section 4.2 to stress this fact.

Actions

We added modified section 4.2 according to our previous answer.

Comment

Please also note the supplement to this comment:

Answers

Thank you. We have addressed the comments provided in the supplement document

Actions

Modified the paper according to the comments provided in the supplement document.