Dear Editor,

We thank you for acknowledging the improvements that we have done in the last manuscript update, and for suggesting other issues that should be considered to further ameliorate the paper itself.

In the following, you can find a point-by-point reply to all your comments and for each of them the "Actions" that we have implemented to amend the manuscript.

[Editor] Page 4 "Unfortunately, regulations, in their current status, do not consider yet the fact that a turbine may operate out of the design conditions according to a farm control.". This is not entirely accurate, and you might want to consider this position paper: https://www.windfarmcontrol.info/-/media/Sites/WindfarmControl/Publications/FarmConners-WP2-D2-1-Position-paper-on-WFC-certification_revised.ashx?la=da&hash=E5801009B447D914DD7A566C9FCA9426B4483C9D. In addition to considering wind farm control, the paper also explains well how in-farm inflow conditions are currently addressed by the standards. Material from this paper can help improve the introduction and methodology sections.

[Reply] We are aware of that Position Paper. Clearly, we could not use it in the development of our work as the FarmConners deliverable was made public in July 2020, while the first submission of our manuscript was done in December 2019. By the way, we agree that it is now opportune to refer also to that position paper (along with a similar Deliverable D4.7 of CL-Windcon project), and update Introduction, Methodology and Conclusion sections.

[Actions] We have strongly modified the Methodology sections and updated the Introduction so as to:

- Clarify that Standards can be used for certifying wind farm controls, even if current Standards "in practice do not cover the wind farm control case explicitly" (cf. Section 2.2 of the Position Paper of FarmConners).
- Stress the fact that Standards suggest different tools to model the in-wake flow, with the possibility to handle load assessment for downstream turbines, especially if fatigue is considered. This, on the other side, offers us the opportunity to emphasize that much is still to be done to quantify the impact on ultimate loads (focus of our paper).
- Clarify, even more, that the focus of the paper is on ultimate loads of front-row turbines. This, although limited for certain aspects, represents a further step towards a comprehensive knowledge of pros and cons of wind farm control technique.

We have also updated the sentences related to the Standards in the Conclusions.

[Editor] Page 5, "1D geometrically exact …": I understand what you mean, but the sentence might be misunderstood. The model is 3D, and I believe that the term "beam" already implies what you are referring to.

[Reply] We agree with the editor.

[Actions] The text has been rephrased as "This tool allows one to model the flexibility of blades, tower and shafts through a geometrically exact beam model (Bauchau, 2011), whose sectional structural properties are rendered with fully populated 6 x 6 stiffness matrices."

[Editor] Page 7: is there any possible failure mode of the wind farm control laws that should be considered? If not, please explain why.

[Reply] The comment is extremely pertinent, as any possible technology (hard- or soft-ware) may be subject to failures, and wind farm control is not an exception. However, a proper analysis falls out of the scope of the paper. The problem of failures in farm control laws was briefly analyzed in Deliverable 4.7 of CL-Windcon project (cf. http://www.clwindcon.eu/wp-content/uploads/2020/03/CL-Windcon-D4.7-Review-on-standards-and-guidelines.pdf). After reviewing the different techniques and the associated risks, the Authors of that deliverable stated that in case of conflict between individual wind turbine regulator and farm controller, the former should have the priority. This, at least for the goal of our paper, offers a suitable reason not to include farm control failure modes in the analyzed DLCs. Moreover, to some extent, it suggests that farm controllers can be implemented so as to limit the risks connected to their failure. As a final comment, one may also envision that farm controllers may be linked to failure detection systems which would disengage the control itself when needed, restoring the nominal operations based on greedy control.

Clearly, we acknowledge that the issue raised by the Editor deserves a mention within our manuscript.

[Actions] We have included a comment about the wind farm failure modes in Section 3 after Table 1, briefly explaining the problem and the circumstances under which it is reasonable not to include the farm control failure in the analysis.

Moreover, a new sentence related to this point have been included in section Conclusion.

[Editor] Page 9 (but also elsewhere, wherever you refer to AEP): AEP is not a meaningful KPI in this context, as a turbine will never work with the same misalignment angle for a full year. Power losses for a steering turbine are typically quantified through the cosine law exponent, not AEP (unless you are looking at a specific site, wind rose, farm layout, etc.). The power loss exponent however has a large scatter, and very different values have been reported in the literature. It is doubtful that your (and most other) BEM models would be able to accurately predict this effect, unless high yaw misalignment corrections are implemented and calibrated. I suggest that you simply refer to the literature on this point, which is also not central to the present discussion.

[Reply] We had inserted the indication of AEP in the previous versions of the manuscript to give an idea on the impact of the control techniques on the production of the single front-row turbine, showing that, for example active wake mixing has lower impact than wake redirection. Hence, for active wake mixing, even a small increase in the downstream turbine production may compensate the loss in the upstream machine and lead to an increase of the overall farm power output.

However, we acknowledge that such indication may be prone to misinterpretations. Hence, we accommodate Editor’s suggestion.

[Actions] We have removed the information related to AEP on page 9, Table 2, Table 3. Figure 10 (power curve and power coefficient) have been eliminated and related text have been accordingly modified.

[Editor] Page 9 (but also elsewhere, wherever you refer to ADC): the previous comment applies also to ADC: even if you use Weibull weighting, it is not reasonable to assume operation for a full year at a given misalignment angle. Additionally, you refer to pitch ADC, but one should first have a discussion on yaw ADC (which is the main concern in practice, since it grows significantly, while pitch ADC will tend to decrease).

[Reply] Clearly, the ACD for yaw actuator is essential for wake redirection, but it cannot be evaluated by simply looking at the 600-second simulations, considered in our DLC list, where the turbines are not expected to yaw following wind direction changes. Hence, we agree with the Editor that the treatment about ADC may lead to a misleading piece of information. On the other hand, it would be a pity removing the evaluation of pitch-ADC for active wake mixing, as this is the first time in literature that this analysis has been done. Moreover, a more "realistic" indication of ADC increase may be extracted from "full year" ones multiplying the indication that we provided by the percentage time spent with non-null farm control input.

[Actions] We have left the information on pitch ADC in the context of the active wake mixing, clarifying the limitation connected to the fact that we are looking at full year operation with the farm control (page. 14). We have removed the indication of ADC in the rest of the paper. In particular, we have eliminated the left plot of figure 11 (Weibull-averaged ADC) and modified the related comments in the text.

[Editor] Page 11, end of sect. 4.1: this is also the reason for one-sided wake steering (in addition to the much reduced effects on yaw ADC), which has been consistently used in most of the field tests conducted so far.

[Reply] We agree with the Editor. We had also mentioned such opportunity in the Methodology section in a slightly broader sense, stating that the authority of the farm control can be limited in all those conditions which can be critical in terms of loading. Clearly, avoiding operations in critical conditions may simply lead to one-sided yaw misalignment or to more complex combinations of misalignment angles, speed, and TI.

[Actions] We have stressed this concept and have referred to previous experiments involving wake steering (e.g., Fleming et Al. Wind Energy Science 4, 273-285, 2019.) both in the methodology section and at the end of section 4.1.

[Editor] Page 14: please note the strange apparent inconsistency: here you look at the ADC of the driving input (pitch), while for wake steering you look at the ADC of pitch, although the driving input in this case is yaw. As noted above, pitch ADC will decrease, but yaw ADC will very much increase.

In addition, here again the WF controller will not be operating all the time, so this KPI does not seem to be very useful. As the work focuses on ultimate loads, I suggest that you eliminate references to AEP and ADC: as for DELs, any analysis of these KPIs requires creating first a reasonable scenario, because WFC is not used all the times. In my opinion, as for DELs in the previous version of the manuscript, references to these aspects unnecessarily complicate the discussion and distract from the main point.

[Reply] See the reply to previous comments related to AEP and ADC.

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[Editor] Page 16: you decided to focus only on 2 deg amplitude. But is it enough to achieve a useful increase in wake mixing? Please justify this choice. What happens if we really need 4 deg to achieve some power boost?

[Reply] This choice is justified by previous experiments in wind tunnel, published in Frederik et al., Wind Energy Science, 5, 245–257, 2020. In that experimentation, the highest power increase was achieved with an amplitude of about 1.7 deg for two different atmospheric boundary layers (cf. Tab 2, Fig 10, and Fig 13 of WES 2020 paper).

Clearly, the active mixing with 4 deg would have a more significant impact on loads, and, eventually, on design. The choice on whether to use it or not, if one really needed 4 deg, would depend on the balance between power boost potentially achieved and detrimental impact on loads. Of course, there is a similar issue for wake redirection-based controllers (see for example, the possibility of employing one-sided wake steering, that the Editor mentioned in a previous comment). All in all, the choice of 2 deg appeared, at least to the Authors, more convenient and supported by previous experience in wind tunnel, as stated before. We do not believe that repeating the design also for 4 deg will give value to the work.

[Actions] We have added a proper comment and the reference to Frederik et al., WES 2020, after Table 2, to better explain the choice of focusing on 2 deg amplitude.

[Editor] Page 17: "… have an impact on ultimate loads, especially on the maximum tip deflection, …". I understand what you mean, but tip deflection is not a load. Please rephrase.

[Reply] We agree.

[Actions] We have rephrased the text.

[Editor] Page 17, lines 389-392: I am not sure I understand the motivation not to consider wake steering in the redesign exercise, since previously you said that it causes a significant increase in tip deflection. When you say "The design process of the baseline generated an optimal solution compliant to all optimization constraints, with a structure mildly different …" are you talking of the redesign accounting for wake steering? Please rephrase, this is unclear.

[Reply] That sentence refers to a preliminary step in the design process consisting in the update of the INNWIND.EU blade, with the aim of providing a compliant baseline, i.e. with a blade designed using the very same constraints and DLCs that we will use in the redesign process that considers the wind farm control. The redesign of the blade including wake redirection is not part of this paper, but is included in a conference one, i.e., Sartori et al. TORQUE 2020. Such article was already included in the list of reference. We may however recall one result (i.e. the increase of blade mass entailed by WR), which may be of interest for readers.

[Actions] We have moved the misleading sentence of page 17, line 389-392, some lines above where the baseline updating is explained. We will also include a sentence about the mass increase after the blade redesign under wake redirection control, taken from ref. Sartori et al., TORQUE 2020.

[Editor] Many sentences are long and a bit convoluted. Please do not rely only on the language editor to improve the language during production, and try to shorten and simplify the text wherever possible.

[Reply] -

[Actions] We have shortened, simplified, corrected, and improved the text of the manuscript.