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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Evaluation of the impact of wind farm control techniques on fatigue and ultimate loads" *by* Alessandro Croce et al.

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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). 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. 2) There are several independent studies which indicate that overall, yaw misalignment, positive and negative increase the DELs of the 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). 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?

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2019-103/wes-2019-103-RC3-

supplement.pdf

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