Review of “Adaptive robust observer-based control for structural load mitigation of wind turbines” by Edwin Kipchirchir et al., manuscript number: wes-2021-143

General comments

The manuscript presents a robust disturbance accommodation (RDAC) pitch control method for the above-rated operating region. The method is based on a previous publication by the authors with the addition of a decoupled 1P cyclic IPC loop. The theoretical basis of the controller design is explained. The, mainly qualitative, results include a wind-step simulation and one turbulent simulation of 200s comparing RDAC with RDAC+IPC in time and frequency domain as well as a covariance analysis of power with blade flapwise and tower fore-aft structural loads.

The scope of the study is not clear to me, as there is not a consistent link between motivation/hypothesis-methodology-conclusions. The RDAC is already presented in a previous publication from the authors while the decoupled IPC loop is rather standard with the addition of a Kalman filter to account for noise in state estimation. I think the scope and novelty have to be clarified and explained further.

The results are compared between the two new implementations only, without comparing with a conventional PI controller. This makes it hard to realize what the possible benefit would be for the wind energy community compared to the current status. Moreover, the simulation results are very limited including one step-wind simulation and one turbulent simulation of 200s, which in my opinion are not enough to evaluate the potential benefits.

The literature review is also limited, mostly using self-citations, and seems to be missing a large part of literature working on adaptive control design, DAC and IPC. Furthermore, the choice of the wind turbine model is not justified. Such a control scheme would probably be more relevant in larger turbines with larger and more flexible blades. The size of the machine and the related aeroelastic properties are not relevant for modern commercial systems (onshore or offshore). If the scope should be limited to onshore turbines (the choice has to be justified though) the IEA 3.4 MW or the NREL 5 MW r.w.t. could be used. Otherwise, the DTU 10MW or IEA 10MW/15MW machines can be also considered.

In my opinion, this work needs a thorough revision/rewriting to be accepted for publication. A more in-depth literature review, clarification on the scope, clear differentiation with previous work from the authors, methodology (more detailed explanations of the different implementations, reporting of values used, etc), and possibly a different WT model are some of the topics that need to be addressed. Nevertheless, my major concern is about section 5 which is not convincing. More simulations are required covering more operating conditions along with more relevant quantitative metrics for comparison. Moreover, the results should be compared with a tuned conventional PI pitch controller, as was also stated in the authors' previous work on the same topic. Finally, there are some minor issues with the terminology and phrasing used throughout the manuscript which I believe should be addressed in a later stage and are not discussed here.

Specific comments

Methodology

- Explain why the chosen WT model is relevant. My recommendation would be to switch to one of the most relevant in terms of turbine size and capacity (see previous comments).
- Be more specific in the description of the models and simulations: which FAST version is used, which DOFs are enabled and why etc.
- L 49-51 The sentence is not clear, seems like the wind turbine model and the aeroelastic software are mixed. Also, the meaning of "domicile" in this context is not clear.
- The pitch actuator dynamics are modeled as a first-order low pass filter, what is the time constant used? This choice is important to be stated and explained. In the current version, no value is discussed. In l150 the actuator modeling is referred again as a transfer function included in the plant. Are these the same, can you clarify? In general, provide specific values for constants and derived variables throughout section 3.
- How are the controllers and the switching implemented for both RDAC and IPC? Traditionally this is done based on the collective pitch angle. More explanations are needed to understand the method and ensure reproducibility.
- How is the switching between regions 2.5 and 3 implemented with the proposed RDAC?
- More explanation on the implementation of the method (switching, parameter choice, obtained values etc) are needed and the specific values applied should be provided along with the justification/derivation. In the current state, mainly symbolic derivations are included in the manuscript.
- In l160-162 the authors mention that the RDAC approach suggested is valid for a very narrow operational envelope. How is the smooth transition between the controllers impoemented? Can it be implemented in practice? How does it compare with the common gain scheduled PI CPC controller?
- Tower base fore-aft bending moment is not a standard measurement existing on every turbine. I understand that you used this to improve the model performance, but I think it should be at least mentioned. Did you try to use some of the already existing measurements or an observer instead?
- L 110-113 Possible methods to derive the gains are mentioned but it is not clear to me what methods were used in this work. Please be specific on what is used in this study and why. The text in section 3 reads more like a controls textbook rather than a specific application.
- Maybe I am missing something, but sections 3.1 and 3.2 seem to be the derivation of the RDAC similar to the previous publication from the authors (Do and Söffker, 2021) using also the exact same figure. Does this generic theory need to be repeated to its whole? It is not clear to me if the scope is the RDAC or the cyclic IPC in section 4. Please clarify the differences between the previous publication and state the novelty of the present work.
- Also in section 4, it is not clear to me how the distinct IPC controllers are combined. The sentence in l206-207 is not clear on this. Additionally, how is the incoming wind speed defined and measured? It could make sense to look into using the CPC value as an indicator to switch as it is common practice.
- How are stability and robustness guaranteed when the two methods (RDAC and IPC) are combined?

Results

- The manuscript refers in the introduction and abstract to load mitigation but in the results, no DELs are shown, and not enough arguments are made for the performance of the controller quantitatively. I would suggest focusing on DEL analysis following the IEC recommendations (in terms of wind speeds, TI, shear, duration, etc.) to quantify the possible benefits compared to the baseline.
- The analysis with the step wind is not serving the intended purpose. I don't see the purpose of comparing the PSDs or the time series of speed and power with the steps. Why would the power/speed be changing due to the IPC? How is robustness verified with the step simulations?
- The purpose of the power-load covariance analysis is not clear to me. The relevant figures (8 and 13) are difficult to read and hardly discussed in the manuscript. My suggestion is to remove this part or explain clearly its purpose.
- One turbulent simulation of 200s (including the initial transients) is not enough to show the effectiveness of the controller. More wind speeds and seeds have to be evaluated (see 1st comment of results)
- Specific information on the simulations like windfield generation method (Mann, Veers, etc.), dimensions and duration, DOFs and models activated in FAST, etc. have to be reported.
- The purpose of figures 9-11 is not clear to me. The IPC actuation can be evaluated with other metrics like actuator duty cycle, pitch angle standard deviation, pitch rate, total pitch travel, etc. The possible load reduction can not be identified by visually examining the time series.
- As the authors state the mean values are the same and the standard deviation is reduced by 12%. This is not enough to support the load reduction claims. DELs should be calculated taking into account the load cycles using a rainflow algorithm in longer simulations. I suggest using more wind conditions including more seeds per operating point.
- The load reduction should be discussed compared to a conventional pitch controller and not only between RDAC and RDAC+IPC.
- More load channels have to be evaluated in blades, tower bottom, and tower top. More concrete metrics about rotor speed, power, and pitch activity have to be used to evaluate quantitatively the effectiveness of the suggested methods with more simulations.
- Figure 11 shows overshoots of the power up to 25% and in general high fluctuations. Can this be considered good power/set point tracking? Again a comparison with the conventional controller could tell more about the quality of the proposed methods.
- The single turbulent simulation is only 200s long including the initial transients. I believe it is not enough and longer simulations are required to have meaningful PSD analyses and to derive metrics like DELs, standard deviations, actuator duty cycle, etc.
- Figure 13 is discussed in one sentence in L 286. Can you clarify what is its purpose and why it proves that the proposed controller improves structural load mitigation?