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Interactive comment on "Re-design of an upwind rotor for a downwind configuration: design changes and cost evaluation" *by* Gesine Wanke et al.

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general comments:

- This paper investigates redesigning a specific turbine configuration from upwind to downwind. The writing is clear and the scope is appropriate. The paper is interesting, there are some contributions though the novelty is not high. The motivation and results are well explained. My main concerns are all with methodology. It is publication worthy, but need some additional work.

specific comments:

- The literature review is well done. Thorough summary/discussion of previous work. What is lacking is more motivation on why this paper. What is the gap you are addressing? What are the novel and impactful contributions? Not saying those don't exist, they just aren't discussed. All that is said is basically we are going to look at redesigning a specific turbine for downwind operation.

- The physics methods are all reasonable and explained. My main concerns are with the optimization methodology. Some parts were less clear so some may be my own misunderstanding, but these are the concerns:

1) the main process is some combination of sequential and/or nested optimization (not entirely clear the interaction between the different pieces). Both have well known convergence issues and can lead to non-optimal solutions - these are not discussed.

2) Related - convergence quality in general is not discussed. What tolerances were used for each of the optimizations (including sub problems)? With so many levels and sequential processes it does not seem likely that well-converged optimal points are found. That's fine for improvement - especially for an industrial application no one really cares if you've found "the optimum", but it is a bit more problematic if you are making claims on which design is better when the cost differences are small and likely within the margin of error. This can partially be addressed with just caveats on how to interpret the results.

3) The paper explained that the top-level objective was mass. That's fine, but then conclusions based on cost are likely misleading. It's ok to compare those, but you can't really draw conclusions on relative costs. In other words, if cost was thee top-level objective you would likely find a different design than one optimized for mass.

4) The design variables and the relationships between thee sequential/nested optimizations are not clear. How do you optimize with steady loads but also incorporate dynamic load cases? What are the various optimizations connected (inputs/outputs/feedback)? A formal diagram would really help.

5) What is the target axial distribution? Why is there a target and how was this decided?

6) How well does this loads scaling procedure work as the geometry changes? There should be some verification recomputing the moments directly with HAWC2 to show that the scaling approach is reasonable. Derivatives are mentioned here, what type of optimization is used? I don't think that is stated for Eq 3.

- The cost model is rather vague. There is a big list of variables in Table 2 but the reader has no idea how these are used. Reproducibility is not possible. Perhaps this is method is private, but at least some more detail would be helpful.

- Good discussion at end of paper.

technical corrections:

- Fig 5 would really benefit from color. hard to read.

- There are quite a few places with awkward phrasing or incorrect grammar. Overall it reads really well, but some additional proofing would help clean up some of these sentences that are hard to parse.

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