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Comment on wes-2022-12

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Referee comment on "Adjoint-Based High Fidelity Concurrent Aerodynamic Design Optimization of Wind Turbine" by Sagidolla Batay et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2022-12-RC2>, 2022

Overall remarks:

This paper embarks on the very challenging topic of high-fidelity wind turbine blade shape optimization using an existing shape optimization framework, MACHAero using a discrete adjoint solver for OpenFOAM called DAfoam developed at University of Michigan. The paper provides a brief and very selective literature review ranging across both low, medium and high-fidelity general aerodynamic shape optimization and wind turbine blade optimization research, which in my view leaves out a large amount of needed references.

The paper uses the NREL Phase VI as the basis for the design study. While this rotor is a good and one of the only choices for validating numerical results against experimental data, it is quite a poor choice as a starting point for optimizations, as I go into more detail with in the detailed comments. The paper describes a series of optimizations that were carried out that include different combinations of design variable sets. Overall, I don't find the choices made for this "test matrix" very good, since they don't really enable the authors to conclude much about their individual importance in blade design, and in fact the authors make no or very brief attempts at describing their results aerodynamically in the context of wind turbine rotor design.

For neither of the studies is the cross-sectional shape illustrated, and judging from the pressure distributions, the shapes must be quite "unconventional", and I therefore don't really trust that these results represent a true aerodynamic optimum. One glaring discussion point is why the rotor power coefficient of the optimized rotors are so far from the C_p values we would expect from a rotor, which in this work at maximum reach 0.37. It appears that the authors simply haven't considered this metric in their work. In the optimization considering chord, twist and dihedral no change is observed in the chord and twist, which is highly suspicious. What is also common to all the results is that the number of optimization iterations are very limited, suggesting that the optimization problem is nowhere near converged, making it difficult to trust the results. The results suggest that there has been challenges in getting the optimizations to run successfully, which could be due to poor gradient quality resulting from non-convergence (limit cycle oscillations) of the primal solver, or other challenges in executing the optimization framework not discussed in any way by the authors.

While I fully appreciate the large effort it must have been to become proficient with the MachAero+DAFoam framework, and getting it to run for a wind turbine rotor application, I simply don't think the results are of sufficient quality due to a number of short-comings mentioned above, and discussed in more detail in the comments below. My recommendation is therefore to reject this paper for publication.

Detailed comments:

The use of "concurrent" in the title and text is a bit unclear to me, what exactly is concurrent in the context of this work?

L17: This sentence does not make sense: "To use CFD in the design optimization process, where lower- fidelity approaches like blade element momentum (BEM) are more popular, but new tools to increase the accuracy must be developed as the latest wind turbines are larger and aerodynamics and structural dynamics become more complex"

L22: "A multidisciplinary design optimization tool called DAFoam": to the best of my knowledge DAFoam is a discrete adjoint solver for the SIMPLE solver in OpenFoam, please be more specific as to what you mean by multidisciplinary?

L28: "excellent results are achieved": Please leave it to the reader to judge the excellence of the results, and instead be more specific on what results you have achieved.

L34-39: This may be a matter of taste, but I don't find it particularly relevant to provide a historical view on the use of wind energy or it's relevance to overcome the climate crisis. These are well-known facts and not relevant to your specific work.

L45: "optimizing the blade design is the most difficult aspect of wind turbine optimization": while I am also very biased towards and enthusiastic about aerodynamic optimization, I don't think you should state that the blade design is the most difficult aspect of turbine design. Use instead "highly challenging".

L47: Matlab is not a wind turbine optimization tool.

L48: "while in certain research, Xfoil, NACA airfoils, and AirfoilPrep are used to simulate the aerodynamics of the blades in conjunction with Matlab ": You're mixing tools and specific airfoil geometry series. Xfoil is the only tool mentioned that can compute aerodynamics of airfoils. Matlab is a programming platform.

L52-53: "One of the most effective methods...": I'm not entirely sure what is meant by this sentence. Are you talking about airfoil design?

L73: first sentence: this is stating the obvious.

L74: most critical -> highly critical

L75: "...determining the rotor shape and forecasting the rotor's aerodynamic performance": vague sentence. Determining the rotor shape wrt which criteria?

L92: "...it is accurate enough for many flow conditions..." -> it is NOT accurate enough I guess you intend to write.

Section 2.1 overall: The literature review on low-fidelity aerodynamic design optimization is very selective and in my view missing a significant amount of important references (some of which also deal with full aerostructural design of a rotor). This section should either be improved significantly or removed.

L104:

L109 full paragraph: I don't understand why this paragraph is placed in this section. It describes fundamental analytical properties of rotating helicoidal wakes, but you don't state how this relates to the blade design? And I would not categorize this as "medium fidelity".

L114-117: Why discuss BEM and CFD in this section? I would also think that a lot has happened since 1952 wrt applicability of CFD in design, so couldn't you find a more recent reference?! I would simply delete the sentences starting from "And BEM is still well-established...".

L125 onwards: The main advantage of adjoint based techniques is that they are largely independent of the number of design variables, but scale with the number of constraints and objectives, that is often less than the number of design variables in aerodynamic design. The main challenge with gradient-free methods is that they scale very poorly with the number of design variables.

L138: Ok, you mention the advantage of adjoints here. I'd move that up to where you mention it first, L125.

L171: "flap-wise bending momentum" -> "flap-wise bending moment"

L198: "double-bladed" -> "two-bladed"

L220: structural meshes -> structured meshes, non-structural meshes->unstructured meshes

L230: Here you mention using a wall function-based turbulence model, and in L236 state that you use the SA model. Is that indeed with a wall function or modelled all the way to the surface? If the latter, a y^+ of 10 is not sufficient.

L249: Figure 4 does not show torque values, only C_p

Table 1: Consider also doing a Richardson extrapolation to estimate the numerical error of the three meshes compared to a theoretical infinitely fine mesh. You should see second order convergence of the solution, but this can however, be difficult to achieve for complex flows.

L343: You refer to "lelist and telist" points, but this naming isn't used anywhere else in the paper, and you need to describe what they represent in more detail.

L347: You refer to a coordinate direction but haven't introduced the overall coordinate system used.

Section 3.1.2: You don't discuss the issue of flow solver numerical convergence, which you highlight as a challenge in the work of Vorspel et al. Can you converge the RANS solver deeply or do you see limit cycle oscillations?

Section 3.3: Please be more specific with the geometric constraints. It is quite unclear what e.g. the thickness constraints entail and how they limit the design space.

Section 4:

Overall choice of the NREL Phase VI as basis for optimization: While I understand and appreciate that you want to validate your CFD results against experimental data, the choice of rotor is quite poor for the following reason: The rotor operates at very low thrust coefficient and therefore also low power coefficient. The reason for this is that the rotor in fact was design as a three-bladed rotor, but used as a two bladed rotor in the experiment. It is therefore very far from aerodynamically optimal, and you'd need a significantly higher solidity to reach reasonably high C_p values. I will therefore expect your optimizations to primarily attempt to increase the loading through either high C_l , increased chord and twist/pitch. Reaching optimal loading of $CT=8/9$ could simply be limited by the bounds on your design variables, leaving you with an always sub-optimal design, or even worse, a design where you see design features that arise for the wrong reasons.

In Table 2 you list the various optimizations you've carried out, and I am a bit puzzled as to why you chose this set of optimizations. Why split the optimization of shape with chord and twist into two optimizations? As mentioned above, I think particularly for opt S2 you will obtain design features heavily influenced by the limitation in the design space from not considering chord. Also, for the S4 optimization, you have no way to discern the effect from optimizing including dihedral, since you don't have an optimization with shape only in your test matrix.

Instead of quantifying the objective function in terms of torque it would have been much more obvious to use rotor power coefficient. And as discussed below, I am surprised how relatively low

Section 4.1, L372: "Despite the fact that the improvement in torque is 5.34 %, the optimality is less than 0.0002, while the optimality tolerance is $1E-7$." Indeed, with such few optimization iterations, it seems like this optimization is not converged. In fact, the design variable change between iteration 3 and 4 is very large. You don't describe why you stop the optimization prematurely, not reaching the optimality criterium, or at least a visually converged objective function.

Section 4.2:

In this optimization you run 10 iterations, and appear to reach closer to a flattening of the

objective function, at least visually. As requested above, could you please state why you don't run the optimization for longer?

If you compute the rotor power coefficient, the optimized rotor has a C_p of approx. 0.361. You need to discuss why you can't increase it further, keeping in mind that a regular rotor would have a C_p in the range 0.45-0.50. This could indeed be because shape and twist alone cannot compensate for the very low rotor thrust coefficient you start from.

You only show a comparison of surface pressure of the original and optimized blade, but you don't show the resulting shape, which is highly relevant! Why? Judging from the pressure distributions, the shapes must be quite strange, seeing you have a pressure spike at 40% chord. I therefore find it very difficult to trust that these results indeed are of sufficient quality to publish.

Section 4.3:

Yes, your objective function flattens out visually, and you reach a higher C_p than for opt S2, but still rotor C_p is only 0.372. I would expect the chord to increase significantly but you observe a reduction in chord. Please provide your interpretation of this result. As in the previous S2 optimization, you don't show the cross-sectional shape, and judging by the pressure distribution the shape is highly "unconventional", and again leads me to conclude that the results are not of sufficient quality to publish.

Please comment on whether running 10 major iterations is a consequence of a limit set by you, or a consequence of a crash of the CFD code or the optimizer exiting), bounds on design variables limiting the design freedom or something else?

Section 4.4: It is certainly interesting to optimize a blade wrt dihedral, in particular because basic BEM does not take into account the effect from out-of-plane geometry. But again, I am not so sure your choice of design variables makes it possible to interpret what improvement you get from including dihedral, since you don't have e.g. a shape-only optimization. So the 8.7% increase could in fact be solely due to the shape design

variables, there is no way to know.

Section 4.5: Your results show almost no change in chord and twist, which is highly surprising to me. As stated earlier, I would expect large changes in chord to increase loading. Your objective function seems no where close to converged either, please explain this.

As remarked for the S4 opt, including dihedral has very little value since you cannot compare it to an optimization where you optimize only chord and twist.