

## ***Interactive comment on “Multipoint high-fidelity CFD-based aerodynamic shape optimization of a 10 MW wind turbine” by Mads H. Aa. Madsen et al.***

**Anonymous Referee #1**

Received and published: 22 November 2018

The presented paper gives an extensive overview on the current state of the art in optimization based on CFD. Afterwards, own research on this topic is presented and an interesting overview on the optimization of rotor blades using single-point and multi-point optimization is presented. The results and used tool chain are very interesting and worth publishing.

The main concerns when reading this manuscript are regarding the chosen structure, some presented topics that are out of the scope of this paper and there are some unclear conclusions. Therefore, I suggest a major revision before publication.

I highly recommend omitting the part comparing two CFD codes, as this is not within the scope of the paper. Instead, some further information on the implementation and usage of the adjoints are highly recommended.

C1

The review is structured as follows: First, some general remarks on the chosen structure and order of chapters and sections are given. Second, the content is discussed and finally typos, wrong placing and further minor errors are listed.

Note: Sometimes, there were inconsistent line numbers (starting with 20 at the page top for example). In these cases, I still used the line numbering from the manuscript for my comments.

Structure:

1.1 – 1.2.3 cover related literature, but only 1.1 is titled related literature. The structure should be corrected. 1.2.1 is titled airfoil optimization and 1.2.2 blade optimization. But 1.2.3 also shows airfoil and blade optimization. Another structure can help the reader to properly differ between the topics.

Page 6 Line 4-15: This paragraph is confusing. I suggest to rather sort it by topic then by papers above and below. Lines 10-13 therefore are unclear: What exactly is effective, which computational effort can be reduced under which circumstance – based on which publication.

1.3 and 1.4 could be combined. First give an overview of the present work, then add details on the chosen case.

For the sake of clarity, a consistent notation of the convergence limit ( $10 \cdot 10^{-5}$  vs  $1.0e-4$ ) should be used within the text and plots throughout the paper.

Section 5.2: You introduce the comparison to BEM, then present the results of high-fidelity optimization and later the results of the comparison to BEM. This should be reordered.

Page 33 L 7-14: Should be resorted. Jumps between problem, solution and findings are made.

Content:

C2

Page 1 Line 7-8: The authors claim that especially the tip and root region can gain from CFD based optimization, as BEM is not accurate enough in these regions. This is again stated in Line 14. Nevertheless, the usage of gradient based methods, which is the only used method in this work, also requires very good convergence of the flow field. It is known that these regions are difficult for RANS solvers, as instationary effects, separation and vortices are present. So, it is at least to mention, that also this set-up comes with difficulties.

Page 8 Line 11: The statement that your cases converge typically well below  $1.0e-4$  is not true. Figures 19 and 22 show other behavior. Please differentiate more on that.

Page 10 Overview: The discussion of the optimizations and comparison misses the information about the flow. The used velocities / Reynolds numbers are highly important for convergence of gradients. This should be mentioned when comparing mesh sizes and design cycles.

Page 11 Line 26-28: It should get cleared at this point, if an initial movement of the mesh is done based on the chosen parametrization or not.

Page 13 Line 21-22: The favorable comparison you cite between EllipSys3D and OpenFOAM deals with atmospheric flow over complex terrain. It is not possible to judge based on this paper how the two solvers behave when looking at aerodynamics – which is the topic in this paper. The final report of IEA Wind Task 29 Phase 3 shows that both solvers lead to comparable results in aerodynamics. Therefore this statement should be adjusted.

Page 14 Section 2.3: This section lacks information. You don't cite any other work with ADjoint – it seems to be the first published work with adjoint gradients. So, some detail on the implementation and verification of gradients should be added.

Chapter 3: An extensive revision of this chapter is suggested. It should focus on the numerical Set-Up and chosen WEA. A comparison of two CFD codes (plus one BEM

C3

code) is out of the scope of this paper and should be taken out. The validation could be topic of another publication, which would also allow to have a more detailed look on blade parts and flow phenomena, where incompressible and compressible solver work differently and lead to different results.

In section 3.1 “Computational Mesh” you talk about the WEA model you use, while the mesh is merely topic in this section. You have to justify, based on the results from the GCI, why you keep using mesh L2 (especially when looking at figure 5). The use of hotstarts in your work should be explained here (it was mentioned on page 5 that you will use them, but here it should be explained how). Otherwise it is hard to understand why you should keep using L2.

Page 23 L15-16: It is not clear, which criteria you used to choose the box resolution. Only the two at the blade root are justified, the following testing and choice is not explained at all. Please elaborate more on the choices.

Page 24 L 7-8: It is not clear which parameter sweeps where conducted. Which parameters where swept in which ranges?

Page 26 Table: Another line should be added containing the total CPU time and iterations for optimizations using a hotstart.

Page 31 Figure caption: You mention hotstarts for L0 and L1. Up to here, only hotstarts for L0 where used. This should be handled consistently throughout the manuscript.

Page 30 L 17-19: The statement of “more or less doubled” is exaggerated as factors between 1.42 and 1.96 are shown in table 8. And the largest improvement is found for the coarse mesh, which should not be the reference for the assessment. This again comes up in the conclusion on page 36, Line 10.

Page 33 5.3.1: The finding should be discussed. What movements are suppressed by this constraint? What makes the results more realistic?

Page 34 Figure 22: All optimizations seem to stop with a higher value for the optimality

C4

than some steps before. What is the stopping criterium for cases where convergence of the optimization is not reached? The same can be seen in Fig. 19 (left) for L1. Please explain this behavior.

Further:

Generally: Along the whole document the usage of “x%” is not consistent, also units are used in various ways. Examples are p.9 L 5, p. 15 table caption, p. 26 m/s and m are used in various notations.

Page 1 Line 1-2: Sentence is unclear

Page 2 Line 26-28: Citation is bad placed.

Page 2 Line 28: Start of sentence wrong

Page 5 Line 24: “. . . make use of a surrogate. . .”

Page 10 Caption: “. . . variety of algorithms against we refrain. . .”

Page 12 Caption: “. . . iterative operations that take up. . .”

Page 13 L 8: “. . . (all stored at cell centers). . .”

Page 17 L 15: Empty line

Page 18 L 27: “. . . mesh level. It is also. . .”

Page 19 L 2: “. . . results can be seen save perhaps for the. . .”

Page 25 L 10 and caption Fig. 12: Wrong reference

Page 26 L 22-24: Somewhere the name BEM2 should be introduced. Maybe “The second BEM optimization BEM2 covers the wind speeds . . .”

Page 27 L 8: “Fig. 16 shows the convergence history. . .”

Page 27 Figure 16: Third line should be named

C5

Page 27 Figure 17 caption: Last sentence should be reordered

Page 28 Figure 18 caption: Number and unit are in separated lines

Page 29 Line 2 – 5: Sentence not clear.

Page 29 Line 7: “. . . in final designs obtained. . .”

Page 29 Empty line

Page 29 Equation 10:  $P = T \hat{A} \hat{\omega}$  should be given, otherwise “Using values for torque from Tab. 7” is not connected to the equation.

Page 30 Empty line

Page 35 Figure 24: Might be an optical illusion, but the plots rather look like a comparison between Singlepoint and Multipoint with LE Constraint instead of Multipoint with and without LE constraint. Please check again.

---

Interactive comment on Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2018-66>, 2018.

C6