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Dear Reviewer,

We appreciate very much for your comments. We were asked to response to all comments, while a revised manuscript should not be prepared at this stage. In the following, we will therefore engage with all the comments and propose improvements for the final manuscript.

1) The introduction is lacking a more thorough overview of structural optimisation of wind turbine towers/support structures, which is the main aspect of the paper. It seems that mainly optimization using GA is listed, and these references appear in a later section (Section 3). Also, most references in the paper are to blade design rather than tower design.

Our response:
A more thorough overview of structural optimisation of wind turbine towers/support structures will be added in the revised paper. More papers related to the tower design will be discussed and cited.

2) Line 64-65 The categorization of optimization algorithms is crude. E.g. gradient based algorithms are not mentioned at all throughout the paper although it has been widely applied in wind energy research.

Our response:
The categorization of optimization algorithms will be revised, covering gradient based algorithms.

3) Line 78-80 The authors mention that the genetic algorithm (GA) is capable of "avoiding being trapped in local optima" and that it is used to find "the optimal solution". This sounds like global optimum is guaranteed, which is not the case for many problems. This should be made clearer.

Our response:
The statement "avoiding being trapped in local optima" will be removed, and a more precise description of capability of GA will be added in the revised paper.

4) Line 144: The authors mention that the first 6 frequencies have been investigated, but only the first 4 are shown in the table. Additionally, the mesh is also used for stress analysis. Thus, mesh convergence should be performed on stresses too, as it is a much more local phenomenon that often requires much more fine mesh resolution than natural frequencies. Natural frequencies can often be

obtained accurately with a coarse mesh.

Our response:
The "first 6 frequencies" was a typo. It should be "first 4 frequencies". The typo will be corrected in the revised paper.

Mesh convergence on stresses will be performed in the revised paper.

5) Line 179: Why do the authors alter the height to 80m, when the mesh convergence study was made on a tower of 87.6m?

Our response:
The optimisation framework developed in this work is generic in nature and can be applied to the structural optimisation of wind turbine towers with an arbitrary height. In this paper, the NREL 5MW wind turbine 87.6m-height tower is used only for validation purpose. For the optimisation case study, a typical value of 80m is chosen as the height of the tower.

6) Line 190: Perhaps figure 3 and 4 can be combined, as figure 4 also contains the geometry of the turbine tower.

Our response:
Figs. 3 and 4 will be combined in the revised paper.

7) Line 195-228: It is very unclear which of the formula in Section 2.3.3.1 that is applied, as it seems loads are taken directly from Lanier (2005). Eq. (1) & (2): The 50-year wind velocity, the thrust coefficient, and the rotor radius are defined, but no values seems to be given.

Our response:
The aerodynamic loads on the rotor, as listed in Tables 5 and 6, are taken directly from Lanier (2005). Eq. (1) in Section 2.3.3.1 was added to present the formula which can be used to calculate the aerodynamic thrust force on the parked rotor. Eq. (1) will be removed in the revised paper, and statements will be added to clearly indicate the aerodynamic loads on the rotor are taken directly from Lanier (2005).

Eqs. (2) and (3) in Section 2.3.3.1 were used in this paper to calculate the wind loads on the tower itself.

8) Line 240-242: The authors mention that the thrust force F and bending moment My are the most significant components. The should be clarified why. Also, no coordinate system has been defined, thus My is actually not defined.

Our response:
The thrust force F and bending moment My are generally considered as the most significant

components in aerodynamic loads. References will be added to support this statement. Coordinate system will be defined in the revised paper to facilitate the definition of force F and bending moment My.

9) Line 245: Damage Equivalent Loads are used for fatigue damage estimation. The authors should comment on the assumptions made in the DEL method.

Our response:
A discussion on the DEL method will be added in the revised paper.

10) Line 246: The authors write that the loads from Lanier (2005) are unfactored. However, in Table J-6 in Lanier (2005) both the factored and unfactored values appear. Consequently, Table 5 can be reduced.

Our response:
Table 5 will be reduced to only present the factored values.

11) Line 251+253: The authors refer to Lanier (2005) for both the ultimate limit loads and fatigue loads. These loads are for a hub height of 100m, and seems to be applied directly (without any comments on this) to a tower of 80m. This should be explained.

Our response:
Detailed rotor aerodynamic load calculations, which are generally based on BEM or CFD, is out of the scope of the paper. Therefore, the loads from Lanier (2005) are used in this study as representative rotor aerodynamic loads for 5MW wind turbines, which may be placed at different tower heights (e.g. 80m, 90m, 100m etc.). This will be clearly indicated in the revised paper.

12) Line 276->: Sudden changes in geometries (thicknesses) from segment to segment will give rise to large stress concentrations. The authors should indicate (and comment on) if the stress concentrations are taken into account or not.

Our response:
Discussion on stress concentrations will be added in the revised paper.

13) Line 415->: The authors should indicate the type of buckling analysis (linear/nonlinear)

Our response:
The buckling analysis performed in this paper is linear. This will be indicated clearly in the revised paper.

14) Technical comments:
Line 60 fedility -> fidelity
Line 151: force-aft -> fore-aft
Line 160: force-aft -> fore-aft

Line 240: extreme 50-year extreme wind condition -> 50-year extreme wind condition
Line 283: There seems to be a mistake in the reference listing, “Lin et. Al (Wang et al., 2016)…”

Our response:
These typos will be corrected in the revised paper.

Best regards,
Lin