

Response to reviewers

Manuscript Number: WES-2025-274

Title: “Design optimisation of an open-source reference rotor library for multi-rotor development and innovation”

The authors thank the reviewers and the editor for their time and efforts in reading our manuscript. Their contributions towards the dissemination of our work are greatly appreciated. We have addressed all the reviewers’ comments and suggestions, and we hope that our manuscript is now acceptable for publication. We have listed our responses to the reviewers’ comments below, providing the locations of any changes made with reference to the relevant line number.

Comments from Reviewer #1 and their respective responses are listed below. For additional clarity, reviewer comments are written in italics while author responses are indented and in red.

1. *The global blade parameters play key role in designing the wind turbine rotor configuration. Hence the global blade parameters should be highlighted across both single-rotor vs multi-rotor configurations.*
 - Assuming “global blade parameters” refers to the rotor configuration (blade length, hub radius, etc), these are given in detail in the supplementary repository, since the detailed blade design is the main focus of the paper. The repository includes the calculation methods for these parameters, as mentioned on line 80. The trend in blade length/rotor radius is also highlighted on the right-aligned y-axis of Figure 7(a). In terms of a comparison between the concepts, each rotor is treated in isolation and so the blade parameters are not any different to a single-rotor of the same power rating.
2. *While optimizing the multi rotor system, the blade stiffnesses play a critical role. How is this parameter considered in the proposed optimization of multi-rotor concept models?*
 - The blade stiffnesses affect a number of performance parameters, such as the blade deflections, coupling behaviour, material strains, and aeroelastic stability. Stiffness constraints are implemented in the optimiser and specified in Table 2 under “maximum deflection”. The effect of blade stiffness on the support structure, and the investigation of their coupled dynamics, will be discussed in future work. This has been added to the revised manuscript on line 368.
3. *Blade prebend influences mainly in defining the tower clearance and accordingly designing the blade stiffness to avoid tower strike scenarios. Can you provide justification for not considering the blade prebend parameter?*
 - The reviewer is correct to point out that prebend is an important feature for increasing tower clearance in large modern wind turbines, and the authors agree that it should, in future, be considered for multi-rotors as well.

However, there are multiple reasons for not considering pre-bend in this preliminary design study. First, the support structures meant to host the rotors have not been designed, hence it is not possible to evaluate the equivalent of a “tower clearance”. Second, given the cantilever nature of blades, it is much easier to design stiff smaller blades than large ones, and smaller blades are typically less driven by excessive deflections. Moreover, maximum deflection constraints were imposed during the optimisation process to avoid overly flexible designs. Third, designing blades without pre-bend and sweep could simplify and improve manufacturing, transport, and installation. The above-mentioned justifications were not stated explicitly in the original manuscript, which has been revised accordingly (line 171).

4. *How the optimization model behaves considering the blade prebend parameter?*
 - As the rotors in this study are designed in isolation outside of the context of a connected support structure, it would be inaccurate to attempt to specify a prebend distribution without exact knowledge of the rotor overhang/distance from the support structure. Hence the behaviour of the optimiser considering prebend is not known. This has been addressed in the revised manuscript on line 171.
5. *It would be worth to add the material models considering in the multi rotor vs single rotor study configurations*
 - Material data (properties, layup information, thickness distributions etc) are included in the associated repository. Exploration of the “non-standard” material models that can be used for the multi-rotor context is recommended for future work in the conclusion (line 368).

Comments from Reviewer #2 and their respective responses are listed below. The typeface distinction between reviewer comments and author responses are the same as above.

1. *I recommend to call it Multi Rotor Wind Turbine System (MRS), since it is a system of multiple rotors. But definitely, MRWT is also a proper term for it. (line 1)*
 - The term MRWT was chosen to be consistent with the authors’ previous work (doi.org/10.1016/j.rser.2025.116252). MRS as alternative phrasing has been included in line 1 and 39.
2. *today, wind energy is moving to lower values, often below 400 W/m². It maybe ok to use 420 W/m², but would be good to mention trends as well. (line 100)*
 - The authors agree with the necessity to mention current trends, and have included a reference to modern turbine power densities, citing 2024 data in line 101.
3. *would be nice to explain external and operating conditions of these DLCs for the reader (line 147)*
 - The authors agree and additional details describing the DLCs have been added to the revised manuscript in line 147.

4. 10 % (line 166)
 - This has been corrected for all uses of the % symbol in the revised manuscript, as per the WES composition guide.

5. *If possible, elaborate more on how you implemented the different failure models. i.e. has fatigue been calculated considering rainflow-count and SN-curves. If yes, what was the average wind speed and distribution? Which SN-curve slopes did you consider? Max deflection 10% would mean, that larger blades have larger absolute deflection, i.e. larger overhang is needed, thereby increasing drivetrain costs. Maybe it was written somewhere and I overlooked it: I guess you have no cone angle, no tilt angle, no sweep, no pre-bend. By the way: did you specify the optimal tip speed ratio? (table 2)*
 - SN curves and rainflow counting are indeed used, but this is not mentioned explicitly. A more detailed description has been added to the revised manuscript on line 181.
 - The overhang and therefore drivetrain cost increasing with the blade size is a potential concern for integrated rotor-support structure studies. As such, constraining the overhang to be constant and designing stiffer blades as their size increases may be of interest for future work.
 - Optimal tip speed ratio is not explicitly mentioned, but is implicitly defined through the setting of the rated wind speed at 11.5m/s, and the maximum tip speed constraint of 80m/s (TSR = 6.96, added in Table 2).
 - A mention of the blade design parameters which were omitted from the study (pre-bend, sweep, etc) has been added to the revised manuscript on line 171.

6. *This is a substantial difference in weight, even considering that WindPACT seems to be designed for higher peak thrust. Do you know, where the difference stems from? Please describe, whether all of your blade models include the root connection. Do you mean bolt insert studs by that? Please describe. (line 234)*
 - The potential reasons behind the weight discrepancies between the custom design and the WindPACT model are highlighted in the discussion section (line 339).
 - The remaining design masses do not include the root connection, but for the purposes of comparison, the additional mass of the root was added to the custom design, since the WindPACT design mass included the root. This was initially done by adding a percentage on top of the original mass of the custom design, but in the revised manuscript we instead add on the root mass from the WindPACT blade. This was determined to be a more robust approach, and alters the mass by less than 0.15% Further detail has been added to the revised manuscript regarding this on line 235.

7. *Jamieson thought, that the exponent should be close to 3, when we exclude the evolution from small to large blades. Therefore it would be important to understand, which failure modes were driving the design and check whether this failure mode follows the exponent 2.43. (line 241)*
 - See response to comment 14.

8. *If possible, use kW and kN as units in graphs (Figure 6)*
 - Figures 6 and 9 have now been altered to use kW/MW and kN as units.

9. *indeed, very sharp. It looks like a vertical drop, which seems odd. (Line removed, previously 264)*
- This sharpness has been corrected by re-running with altered wind sampling for the control scheme. The line referencing this has been removed as a result (Line 264).
10. *In the beginning you mention the disadvantage in lift and drag for small blades and low Re-numbers. Here, we cannot see any difference between the 100 kW and the 1 MW rotor in cp? Please explain. Please explain, whether this is aerodynamic cp or whether any drivetrain and electrical losses were included. (Line 266)*
- The same set of aerofoils were used across the entire library, which was validated to not significantly reduce performance between the largest and smallest blades (line 130). Low Reynolds number performance is still a concern, but mostly for blades smaller than those discussed in this study.
 - The Cp used is aerodynamic Cp, so no losses were considered. This is shown in the y-label of Figure 10, and has also been included in the caption in the revised manuscript.
11. *please specify your wind bins (Line 269)*
- Assuming this refers to the average windspeed at the hub/reference height for each turbulent wind file, this detail has been added to the revised manuscript on line 270.
12. *Is the steepness above vrated an artefact? (Figure 9)*
- See response to comment 9.
13. *to be (Line 275)*
- The correction of “this is assumed to sufficient...” to “this is assumed to be sufficient...” has been made and included in the revised manuscript in line 275
14. *Nice figures and what I was looking for to understand the design driving mechanisms. If the 100kW would have been designed closer to the limits, the exponent for scaling would be larger than 2.4, wouldn't it? (Figure 12)*
- This is, most likely, correct. The match between the expected range from historical data (2.1 – 2.9) and the rotor library (2.43), despite the preliminary nature of the latter, is the main fact being highlighted. Upon further iteration, the resulting scaling exponent will likely be closer to 3.
15. *still, I think Jamieson wanted to point out, that the exponent would be close to three, when technological evolution is not mixed in. In order to answer that, one would need to stipulate the scaling laws for the failure modes of the blade. I had assumed that they are all with exponent 3 or even larger (e.g. fatigue). If this overloads your paper too much, I recommend to give an outlook in which you address the necessity to find out, why your results deviate substantially from the 2-3-law. (Line 331)*
- It has been acknowledged in the discussion section that, while the derived scaling exponent is in line with historical data, a set of blades constructed using the same material systems should in theory have a scaling exponent closer to 3, and that the reason why this isn't the case in this study should be scrutinised in future work. This addition can be found on Line 332 of the revised manuscript.