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Interactive comment on "Optimal relationship between power and design driving loads for wind turbine rotors using 1D models" *by* Kenneth Loenbaek et al.

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Thank you for taking the time to read our paper. Your comments are appreciated and we believe that they have made the manuscript better.

The following is the author's answer to the referee's questions/comments. The *italic* text is the referee question/comment the following text is the author's answer/comment. The **(bold)** is the page - (p. #) and line number (l. #) in the document : DIFF_Optimal_power_capture_for_wind_turbines_with_design_driving_loads.pdf attached to this comment where the change has been highlighted.

General Comment

The innovative content of the paper was not clearly stated

After rereading the introduction with this in mind the authors agree with the referee - the innovative content is not clearly stated. To make this clear we added a paragraph about where these results can be applied **(p. 2-3, l. 54-65)**. The sentence which the referee mention ("... it should be understood that the result presented here is not intended to be used directly for rotor design ..") is also taken out, as the authors meant detailed design like blade plan-form but this was not clear from the text.

Limitations in introduction

From the comment: *... section 4.5 ... which in my opinion should be previously introduced in the introduction. This could help readers understand the real innovative content of this paper.*

The authors agree with this point and it has been accommodated by the added discussion in the introduction **(p. 3, l. 66-87)**

Additional MDAO reference

As mentioned by the referee the work by "Bottasso, Campagnolo, Croce, Multi-disciplinary constrained optimization of wind turbines, Multibody System Dynamics" is a seminal work for the use of MDAO to wind turbines and it should be part of the list of references and it is therefore added to the list of references. **(p. 2, l. 30)**

Aero-elastic extreme loads

As mentioned earlier, we have added further discussion of the limitation of the study. Here we also discuss the limitation of aero-elastic extreme loads. **(p. 3, l. 66-73)**

Minor comments

Calling C_P as "efficiency" is not correct from a theoretical stand point
The authors did not consider this fact before it was pointed out by the referee. The suggested change has been adapted **(p. 4, l. 117)**.

Equation 11: $Lexp$ appears here for the first time but lacks of definition.
Indeed, both L_{exp} as well as $\tilde{L}$ has not been defined at this point. It is written here for later reference, which has been written in the subsequent text **(p. 7, l. 155)**.

Line 150: It should be appropriate to notices that a blade stiffness linearly proportional to the chord could be a strong approximation as the internal structure of a modern blade can be complex and could be even characterized by discontinuities.
The assumption of $EI \propto c$ is a crude approximation considering the complex structure that is a wind turbine blade. With that said, the model with $EI \propto 1/r$ is found to match fairly well with modern wind turbine blades capturing the behavior that EI becomes smaller for larger r. It is thought that the fidelity of this approximation is at least on the same order as the other models used in this study.

Line 167: I was wondering whether this assumption be really necessary. In fact, one should be interesting only in having the same (or similar) tip displacement rather than the same deformation shape of the entire blade.
It is an interesting point, and the authors did not think of this case. As the referee points out, assuming that δ_{shape} is the same when increasing R is a sufficient assumption - but it is not necessary. The more general assumption is now added to the paper **(p. 9, l. 202)**.

Figure 7 and 8: It should be mentioned that the dashed lines refers to the baseline rotor and the solid ones to the LIR rotor.

A comment about the dashed lines is now added to all the power-curves containing a baseline curve. (figure 7, 8, 10 and 11)

Figure 9: The symbol appearing in cells associated to $R_{exp} = 2$ and ΔR, ΔM_{flap} and $\Delta\delta_{tip}$ is not clear.
The authors agree that the ∞ symbol in figure 6,9 and 14 were not clear with a smaller font than the numbers. The figures have been updated together with tables in table 1, to make the content of the tables clearer.

Figure 9: In the caption: Please, consider to add also the constraint of the design for R_{exp} equal to 3 and 6, so as to provide a self-explaining figure.
We agree with the referee that this would make the figure easier to understand, and it was added to the caption.

Line 323: "But for ΔAEP it will go towards a finite value", this is not clear looking at the plot. Please, explain.
&
It is not straightforward to understand why for many conditions the "Δ"-quantities go to infinity. I may suggest to add an explanation.
There is indeed no explanation for the limiting cases when $R_{exp} \to 2$. The authors agree that it was confusing not to mention why this is the case. We have added a comment that the result is found by investigating the limit $R_{exp} \to 2$. **(p. 14, l. 281)(p. 17, l. 346)(p. 22, l. 387)(p. 27, l. 405)**. The explanation for this case was thought to be "complicated" and that it would overshadow the results. Especially considering that this limit is not of much practical value. We have attached a separate pdf appendix to this comment ("Limit_Rexp->2.pdf") where the limit values are found for all the three optimization case. We do not plan to add this appendix to the article since it is thought to complicate the understanding of the paper.

Caption of Fig. 13: "It is a similar plot to figure 5 but here it is for the AEP-optimized rotor and it is the change in the max load.". The sentence is not clear.Please, rephrase.
After rereading the caption the authors agree and the sentence is hard to interpret. It was rephrased. **(p. 25, fig. 13)**

Section 4.4 contains only the table and just a sentence. Consider the possibility to insert that content in a previous or subsequent section, or to extend the text with some comments.
The authors added some comments to the section comparing the three optimization methods. **(p. 27, l. 401-407)**

Line 388: "In spite of relatively ... thrust clipping": the concept express in this sentence may be anticipated in the introduction within the context of the innovative content of work.
A paragraph mentioning the concept of thrust-clipping and the study by Buck and Garvey (2015a) was added to the introduction. **(p. 2, l. 47-53)**

Line 394: I agree with the possible inclusion of the radial variation of rotor loading, but what about the use of a more realistic relationship between C_P and C_T? In fact a wind turbine may operate close to $CT = 8/9$ but far from the Betz optimal C_P
It is true that in practice a wind turbine will not reach close to Betz-limit. This is often a consequence of a non-constantly loaded rotor - which is an assumption in this study. For a non-constantly loaded rotor, the loads are not directly related thought C_T and R and the method in this study can not directly be applied. The authors are currently working on generalizing the framework for the radially varying case.

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2019-28/wes-2019-28-AC2-supplement.zip

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