Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-28-RC2, 2019

WIND ENERGY SCIENCE DISCUSSIONS

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Interactive comment

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

Anonymous Referee #2

Received and published: 10 October 2019

General comment:

The paper is the fruit of a work based on an intense analytical derivation of the relationships, written in terms of some relevant turbine quantities (e.g thrust, flapwise moment, tip displacement), between a baseline rotor and one designed with the Low-Induction (LIR) concept .

In general, I believe to have inferred the idea underling the work, and I may imagine that the approach may have a potential utility in the context of LIR redesign, but honestly I have to say that the innovative content of the paper was not clearly stated in the

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Discussion paper

introduction nor in any other part of the paper. In fact:

- Line 44: "... it should be understood that the result presented here is not intended to be used directly for rotor design but to show a possible way to include structural/load constraints into the design process.". This concept is to be better explained, as it seems to refer to the scope of the paper and to hence the core of the research. Structural or load constraints are typically included into blade/rotor/turbine design processes, as witnessed by an extensive literature (see (Fleming et al., 2016; Perez-Moreno et al., 2016; Zahle et al., 2015) that the Authors already cited. To that list, I would personally add also the seminal work "Bottasso, Campagnolo, Croce, Multi-disciplinary constrained optimization of wind turbines, Multibody System Dynamics 27(1):21-53, 2012.".). From this point of view, given the current status of the introduction, the innovative content appears weak. Moreover, section 4.5 (I did appreciate that part with a thorough analysis on the limitations of the approach) lists a number of relevant issues of the developed methodology, which in my opinion should be previously introduced in the introduction. This could help readers understand the real innovative content of this paper.
- Line 50: "The constraints will not include the effect from aero-elastic extreme loads as it is thought to be out of scope for an analysis at this level. But it is expected that if the extreme loads happens in normal operation there should be a relationship between the steady and extreme loads.". The concept could be accepted, but often the ultimate loads come from extreme events such as gusts or during emergency shutdowns or even in parked conditions (where the treatment of this work is no more valid). This should be commented.
- Additionally, even assuming that the maximum loads for a rotor part comes from an operating condition, it is certainly possible that another subcomponent has an ultimate loads coming from a different condition (e.g. Extreme Wind in parked

condition). How can the proposed method handle this situation, given also the fact that different optimized rotors are associated to different loads (performance) through R_{exp}? This may appear as a strong limitiation of the applicability of the proposed method.

- If my comprehension of the work is correct, I would suggest to stress the fact that the methodology can be employed in a very preliminary stage of the design (or redesign) process, when simplified methodologies are always needed to select or define some global parameters (e.g. rotor radius). Additionally, the analysis is useful to comprehend the trend of thrust/loads/displacement as function of rotor radius, as already mentioned by Authors in the Conclusion. All in all, the innovative content is to be better clarify.

Finally, although they do not represent a "show-stop", there are many typos or sentences with grammatical errors, which should be corrected in a revised version of the paper, which is worth publishing. For example:

- Line 148: "...stiffness of the blade AT location ..."
- Line 149: "...the stiffness decreseS towards the tip ..."
- Line 278: "...Will not give the a design ..."

Therefore, I recommend to publish the paper only if Authors will accomodate the previous comments in a revised version of the paper. I also suggest some minor modifications, which may improve the manuscript.

Minor comments:

- Equation 4: This is just a detail: calling C_P as "efficiency" is not correct from a theoretical standpoint as in the power equation one uses the indisturbed wind velocity in front of the rotor and the rotor area, which can be viewed as the area of the streamtube at turbine location. What I would like to say is that $1/2\rho V^3 \pi R^2$ does not represent the kinetic power ideally present in the wind because velocity V and area πR^2 refer to different locations within the streamtube, hence C_P is cannot be defined as "efficiency". In equation 4, this can be accepted as it is used in a generic way, but I suggest to modify line 83 at page 4 from "Efficiency is how much of the potential power the rotor can extract from the kinetic power of the wind." to "Efficiency is the part of the equation related to the power coefficient, representing the capability of the rotor to extract power from the wind." or something similar.
- Equation 11: L_{exp} appears here for the first time but lacks of definition.
- 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 carachterized by discontinuities.
- 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.
- 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.
- Figure 9: The symbol appearing in cells associated to $R_{exp} = 2$ and ΔR, ΔM_{flap} and $\Delta\delta_{tip}$ is not clear.
- 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-explaning figure.

- Line 323: “But for ΔAEP it will go towards a finite value”, this is not clear looking at the plot. Please, explain.

- 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.

- Table 1: It is not straightforward to understand why for many conditions the “Δ”-quantities go to infinity. I may suggest to add an explanation. Moreover, 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.

- 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.

- Line 394: I agree with the possible inclusion of the radial variation of rotor loading, but what about a the use of a more realistic relationship between C_P and C_T? In fact a wind turbine may operate close to $C_T = 8/9$ but far from the Betz optimal C_P.

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