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Interactive comment on "Articulated blade tip devices for load alleviation on wind turbines" *by* C. L. Bottasso et al.

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Thank you to the Associate Editor for the reviews of our paper "Articulated blade tip devices for load alleviation on wind turbines". We have revised and expanded the manuscript with respect to the original submission to accommodate as much as possible the reviewers' suggestions. We have also taken this opportunity to make several other small improvements to the text, and additions to the bibliography.

The revised version of our manuscript is attached to this reply.

Specific detailed replies to the reviewers' comments are listed in the following.

Reviewer #1:

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Discussion paper

General comment:

[Reviewer] The submitted article is of good technical quality. Although the presented concept has been explored since the 80s, there's lack of publications documenting proper evaluation studies. A document with a detailed review is attached and minor revisions to the document are suggested, which will potential improve its quality. [Authors] Thank you for the useful and constructive comments.

Detailed comments:

[Reviewer] Page 1. In brief: âĂć Good technical quality âĂć Not a new concept âĂć Revisions are requested [Authors] We agree.

[Reviewer] Page 2. The impact to pitch actuator usage and pitch bearing fatigue is also a major constraint to the application of IPC [Authors] We agree, and the text has been modified to include the reviewer's comment.

[Reviewer] Page 2. This is not always the case, and many similar concepts evaluated in the 90s have failed to become commercial due to this fact. [Authors] The reviewer's comment refers to the following sentences: "Therefore, for wind energy applications a passive solution might be more appealing than an active one, if the former implies greater simplicity, robustness and ease of repair than the latter." We completely agree with the reviewer that there is no guarantee that passive solutions will finally become commercial products. However, we still think that our sentence is fair and sound: IF one solution is simpler, more robust and easier to repair, then it MIGHT be more appealing.

[Reviewer] Page 3. Some explorations of the concept took place in the 80s-90s. Check and add more reference (e.g. Flexhat project etc). [Authors] We were not aware of this project, and we thank the reviewer for this useful addition. We have now added the relevant references to the bibliography, and we have shortly commented on the ideas explored in the Flexhat project.

[Reviewer] Page 5. Many design ultimate loads occur at rated conditions, and other

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Discussion paper

fatigue loads occur at high wind speeds above rated. What is the main target of the concept? [Authors] As clearly expressed in the paper, we consider the ability of the devices in reducing both fatigue and ultimate loads. In fact, both can be turned into design advantages. We believe that this is the best approach to follow in a preliminary study as the present one. It is also clear that, if we were to develop a similar device for a specific wind turbine, then we might focus on one category of loads or the other, depending on the design drivers of that particular machine.

[Reviewer] Page 6. The centrifugal force does not take into account blade bending which is significant. [Authors] We agree, and the text has been modified accordingly. However, please notice that this simplified model is only used for tuning, while the simulation model is accurate and includes this and all relevant kinematic and dynamic effects.

[Reviewer] Page 7. The 'tip' effects at the pitching tip start are not considered in normal BEM codes. This has been shown to be significant for accurate load predictions, due to the strong influence in local induction rising from the bound circulation jump. The authors should comment on this, since it's the biggest source of model uncertainty. [Authors] The reviewer is right. The mutual induction is an important effect in the evaluation of the performance of the passive blade tip device. Similar considerations were reported also by NASA researchers during the experimental campaign on helicopter's rotors. Several scientific reports are available about this topic and two of them are now cited in the paper to highlight this issue (Stroub et al. (1986) and Martin and Fortin (1988)). Since the standard BEM theory is applied in our simulation environment, the mutual induction effect is neglected. This approximation can be removed by adopting a different aerodynamic model that evaluates the mutual induction among the blade sections, for example a free-wake model. The text has been modified to explicitly note this limit of the present model, and the list of bibliographical references has been expanded accordingly.

[Reviewer] Page 8. Why is the device active below rated? The impact on power will

be evident in this region. [Authors] The following sentences have been added to better explain this aspect of the problem: “It was decided to operate all devices both in the partial and full load regions. Although operation in partial load conditions may affect power output, there is also a significant accumulation of fatigue damage around rated wind speed for sites with low most-probable winds. However, it is also clear that, depending on the characteristics of machine and site, the sole operation in full load conditions might be preferable.”

[Reviewer] Page 9. More details on tuning needed. ’Manual tuning’ is not scientifically proper. [Authors] The text has been modified, as suggested.

[Reviewer].Page 10. Is load extrapolation included in DLC1.1, and which method is used? [Authors] Load extrapolation was not performed, and this has now been noted in the text.

[Reviewer] Page 10. 0.5% loss in AEP is considerable and could possibly not justify the gain from load alleviation. [Authors] We agree, and we have rephrased this part of the paper to more clearly highlight the possible limits of the proposed solutions. The text now reads as follows: “Apparently, the active tip device has the largest impact on energy capture, possibly due to the choice of operating it also in region II. The maximum AEP reduction is equal to 0.5%, a value that is not insignificant and could offset the advantages in terms of load alleviation. A complete redesign study should be used to combine the variation of AEP with load reductions into a single CoE value. Note however that for the passive and semi-passive solutions AEP reductions are very small, and possibly within the margin of accuracy between any two of these simulation analyses.”

[Reviewer] Page 12, line 28. AEP losses should not be considered insignificant. Also, it should be more clear how exactly the design target of reducing COE is achieved.

[Authors] The comment regarding AEP has been removed from the itemization. The AEP reduction is now considered limited only for the passive and semi-passive so-

lutions, where a maximum variation of 0.1% has been estimated. This small value (arguably within the margin of accuracy of the analyses) is remarkable, as the passive and semi-passive solutions are the most interesting ones in the authors' opinion, because of their superior simplicity. Regarding the comment on CoE reduction, the last sentence of "Conclusions and future work" states: "Finally, the integration of the blade tip concept in a rotor redesign activity (Bottasso et al., 2015a) could shed light on the actual potential beneficial effects on CoE, or lack thereof."

Reviewer #2:

General comment:

[Reviewer] This is a very interesting paper focused on a novel load alleviation device for wind turbines. Besides some grammatical issues noted in the attached document, I believe this paper is worthy of publication. [Authors] Thank you for your positive feedback. The reviewer's suggestions and corrections have all been included in the text.

We hope our modifications to the manuscript and replies to the reviewers satisfy your requests and expectations for this paper.

We look forward to your kind reply, and in the meanwhile, we send our warmest regards.

Sincerely yours, Carlo L. Bottasso (on behalf of the authors)

Please also note the supplement to this comment:
http://www.wind-energ-sci-discuss.net/wes-2016-20/wes-2016-20-AC1-supplement.pdf

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