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Interactive comment on "A simple improvement of a tip loss model for actuator disc and actuator line simulations" *by* Georg Raimund Pirrung and Maarten Paul van der Laan

Anonymous Referee #2

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Review of paper WES-2018-59

A simple improvement to a tip loss model for actuator disc and actuator line simulations

Authors: G. R. Pirrung and Maarten P. van der Laan

The paper presents a simple-to-implement correction to the tangential tip loss model/factor of Shen et al. (2005) for use in actuator disc (AD) and actuator line (AL) simulations. The new model is described and implemented, and quantitative comparisons of tip loss factor and spanwise distributions of normal-/tangential forces are performed against a reference blade-element momentum (aeroelastic) code, HAWC2. The work is original and potentially important to the wind energy science community.

General comments: In its present form, the paper is not written in a clear and concise style. There are a few typos in the manuscript that are relatively minor; however, the presentation and physical reasoning for the proposed tangential correction are both weak and unclear. No comparisons to actual experimental (or blade-resolved CFD) data are provided, though these are available from other publications by authors' colleagues at the same institution (?) and instrumental in supporting evidence that the proposed model captures the associated physics. It is therefore that the reviewer is not convinced that the proposed tangential correction is of any use to the wind energy community. Also, the tangential correction is argued to be of use not only in AD but also AL computations, with the latter one not included in the comparisons. The authors further base their model on recent work by Wimshurst and Willden (2017,2018) and do not provide an adequate literature review and benchmarking against other approaches that have been proposed in recent years.

RECOMMENDATION: REJECT - The paper is not acceptable in its present form, style, discussion of (incomplete) results, and lack of quantitative comparisons against measured data and CFD results available in the literature. This is unacceptable in a reputed scientific journal.

To make the manuscript acceptable requires changes that go beyond a typical major revision in a reputed wind energy science journal. The reviewer encourages the authors to perform considerable more work on quantitative comparisons against experimental data and available CFD simulations, see detailed comments below, and resubmit as a new paper.

1. MAJOR CONCERN There is no experimental data given for comparison. This is unacceptable, particularly if the authors want to make a case that their proposed tangential correction is of use to the wind energy community. In addition, only Case 3 refers to an actual (MEXICO) experiment. The reviewer encourages the authors to include the standard MEXICO cases at 10,15,24 m/s and also the NREL Phase VI rotor. Then comparisons can be made against e.g. the NREL 5-MW or DTU-10MW turbines

where a number of CFD simulations are available in the literature for comparison.

2. MAJOR CONCERN The title claims that the proposed tangential correction factor to be applied to the model of Shen et al. is also useful for AL simulations. This has to be shown with quantitative comparisons. It seems that the authors could conduct those using the EllipSys3D code.

3. MAJOR CONCERN The description of the new tangential correction factor is weak and does not seem to be rooted in any physics. What does the 'h' term describe ? What physical phenomenon is captured ?

Other Comments:

- Abstract: The reviewer does not necessarily agree that a separate tangential correction is necessary. A model rooted in the driving flow physics should be as easy and elegant as possible.

- Introduction (page 1): Not all AL models require a tip correction. This does not mean that using a tip correction is incorrect, but it should be acknowledged that there are other approaches.

- Introduction (page 2): The review of recent literature is incomplete. Some work has been published on the de-cambering effect and using free-wake method results as a look-up table for improved tip corrections. This should be acknowledged and it should be clarified what similar or other physics the proposed model captures.

- Introduction (page 2, last sentence): Why only AD computations ? Inconsistent with manuscript title. Also, the comparisons are meaningless in the absence of experimental data and blade-resolved CFD simulations.

- Pages 2 and 3: Not sue if it is necessary to repeat descriptions of the models by Shen et al. and Wimshurst and Willden. It would make sense having the proposed model being described in conjunction with the older models (and not later on page 6)

- Table 1: Inadequate choice of test cases, see major comments above.

- Page 6, section 4.4: What is the physics behind 'h' ?

- Figure 3: There need to be available measured or CFD data for comparisons. How do differences between tip models integrate to deltas in thrust and power ?

- Page 8: Do not start sentence with 'Tab.' or 'Fig.'. More examples throughout the manuscript.

- Page 9, section 5.2: Justify why these model variations have been chosen. What is the effect on integrated thrust and power ?

- Figure 5: What is 'Reference' ? Add HAWC2 BEM to legend so this is not confusing. This figure (as are others) is of no use as there are no data for comparison. In particular, one would hope for improved comparisons against data for the proposed new model and tangential forces.

- Page 11, Conclusions: The second paragraph is unclear and not precise, e.g. "... when the fit for the axial loading is good".

- Page 11, Conclusions: The last sentence of the manuscript is very concerning as it states that "... refitting is not required if a good fit of the normal force could be obtained using the original tip loss model" How can one know what to choose a-priori, if the method is used as a predictive tool ?

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