Response to Reviewer 2

Title: *Pressure Based Lift Estimation and its Application to Feedforward Load Control employing Trailing Edge Flaps*

Dear Reviewer,

We thank you for your interest in our work and the helpful suggestions to improve our paper. The summary of your review is:

Summary of the review, uploaded 06.11.2020:

Generally a good article, with insights on the mechanisms of utilizing pressure based lift estimation for load control with active flaps. More details on many of the aspects of the experiments and modeling can improve the quality of the article. Detailed comments are included in the uploaded pdf file.

Please see our point by point answer to your comments in the following. Furthermore, changes in the manuscript according to your comments are marked in blue.

Page 1

L1: Shouldn't the title reflect also the fact that wind tunnel testing is involved?

Generally, we agree to your comment. However, due to the lengthiness of the title:

Pressure Based Lift Estimation and its Application to Feedforward Load Control employing Trailing Edge Flaps

it is decided to keep the title as is. The interested reader is referred to the abstract where more information about the paper is given.

L15: The concept of reducing LCOE with active load control is mostly driven by the fact that larger rotors (+AEP) can be produced for the same loading. This should be clear when mentioning cost saving from material.

We agree to your comment that active load control is employed to design larger rotors (+AEP) for the same loading. Additionally, active load control can be used to reduce loads on a turbine and thereby reducing the capital expenditure as the less material is needed. Both ideas are added to the manuscript.

L19: Pitch bearing damage might be more important than response time.

Generally, the goal of any active load control method is to reduce loads. It is implicitly understood that there is an actuation cost in order to achieve this goal. Actuation wear will occur in both pitch and TE flap actuators and cannot be regarded as a disadvantage for one type of actuator only. As an additional disadvantage of the pitch actuators, we added the fact that it cannot effectively mitigate local blade loading.

Page 2

L21: This statement, which is afterwards used as an argument for the present study, is quite confusing. Probably rephrasing it to reflect the challenges/limitations of field testing will clarify this (if that's the intention of the statement).

We have changed the concerning paragraph to clarify the argument.

Page 3

L13 Few pressure taps compared to mounted probes seem like a more feasible option considering realistic application (installation, vibration, drag). Nevertheless pressure taps also potentially face the same clogging risk (which is more due to moisture and not particles considering <0.5mm holes). Maybe the argumentation should be more clear in the text.

Thanks for your comment.

1) We have added 'moisture' in the manuscript.
2) And yes, pressure taps might suffer the same clogging risk as mounted probes. Nonetheless, for a lab scale experiments they are generally the choice for surface pressure measurements. For full scale applications thin film surface pressure sensors might be more feasible. This idea was added to the manuscript.

L32 This is pretty low. You should at least comment of the validity of the cases compared to full scale.

Conducting experiments on research scale turbines generally leads to the issue of low Reynolds numbers. Please compare to the Oldenburg Turbine (https://www.mdpi.com/1996-1073/12/7/1306/htm - here the maximum is Re=120k), Milano Turbine (https://iopscience.iop.org/article/10.1088/1742-6596/524/1/012061/pdf) where the Reynolds number is scaled by a factor 225 to the Innwind Reference Turbine. Herein, they argue, that Reynolds scaling was conducted trying to find a compromise between low Reynolds number and an excessive control bandwidth. This statement was added to the manuscript.

Page 4

L3 I guess you mean 1p (1/rev), it's confusing using 'pi'.

We have added (1/revolution) to clarify this statement.

Page 5

L5 Is this angle used in all comparisons? What about wind tunnel corrections for blockage, flow curvature etc?

We agree that a correction for solid body and wake blockage as well as stream line curvature is generally possible for this setup. However, the aim of the current paper is to compare different lift estimation methods between each other, which are partially calibrated on the underlying steady polars. Therefore, the fundamental outcome of the study is not changed as the correction would be propagated into the lift estimation methods. Nonetheless, if the interested reader is aiming for a CFD comparison all necessary parameters for blockage correction (chord, airfoil type, thickness, span, tunnel dimensions) are given in the paper.
Nonetheless, your point is taken, and the correction was calculated according to Barlow (Low Speed Wind Tunnel Testing 3rd ed, 1999). The result shown in the following figure, where the solid lines correspond to the corrected results. It can be seen that the difference in the linear region and for small flap angles is small, increasing towards larger angles of attack. However, as all following plots in the paper are grounded on the uncorrected values, this plot is not incorporated into the new manuscript in order not to confuse the reader.

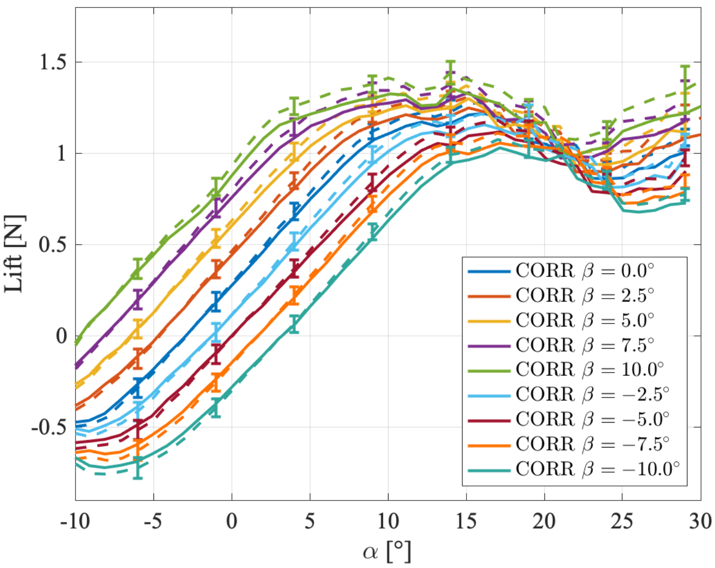


Figure 1 Comparison between uncorrected balance measurements (dashed lines) and blockage corrected results (solid lines).

Page 6

L17 How is the effect of the arm and all other disturbances taken into account when comparing to an undisturbed airfoil?

Generally, the effect of the lever arm and the three hole probe (+ its support) is not taken into account and a conclusive answer can only be given by conducting a full 3d CFD study. However, the effect of the lever arm is expected to be negligible as the spanwise extension is less than 1%. Regarding the three hole probe, there might be a more significant effect due to the extension of the support of the probe. Nonetheless, as the spanwise extension is less than 3.6% the effect is expected to be very small.

L19 This is the first time pressure taps are mentioned in the text. More detail is needed (when mentioned again afterwards): positions, size. Moreover, usually pressure taps are not located in the same spanwise position in order to avoid disturbances)

We agree to your comments.

1) The coordinates of the pressure taps were added in a table in the appendix. Also the diameter of the holes is added to the manuscript.
2) Spanwise Positions: It is agreed that a varying spanwise position, which was not conducted due to manufacturing reasons, would have been beneficial for the results. However, a comparison of the cp distribution to xfoil calculations yields satisfying results. This is expected due to the fairly large spacing between the pressure taps.

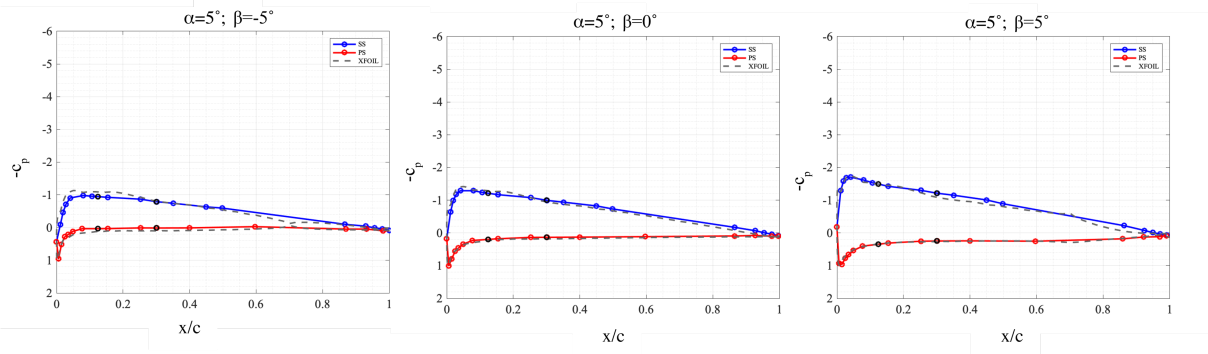


Figure 2 Comparison of measured cp distributions to XFOIL calculations. Exemplary alpha=5° for three flap angles (-5°,0°,5°) are shown.

Page 9

L2 It is very important when presenting these plots to verify how accurate the measured polars are compared to previous tests/CFD/even xFoil, especially considering the added disturbances of the setup. Moreover, the geometric angles of the turntable should not be used without including tunnel corrections. Is this the case? A steady state comparison of force balance and pressure taps should also be presented at some point, since the two methods are compared afterwards.

We agree to your comment. As this paper concentrates on lift estimation (and comparison) Figure 5 (polars of lift, drag and moment) of the original manuscript is exchanged for a figure presenting lift only. Herein, the comparison between balance measurements and lift calculations based on the integration of all pressure ports are shown. Furthermore, a comparison to XFOIL is shown for a flap angle of beta = 0°. Additional XFOIL polars for different flaps angles are not added to keep the plot readable. The accompanying description of Figure 5 is changed in the manuscript.

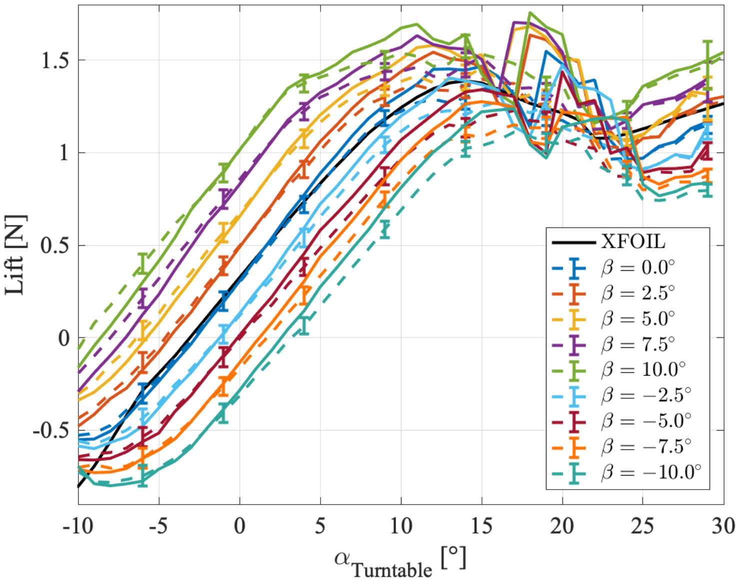


Figure 3 Lift comparison between balance measurements (dashed lines) and full pressure port integration (solid lines). Additionally XFOIL result for beta = 0° is shown.

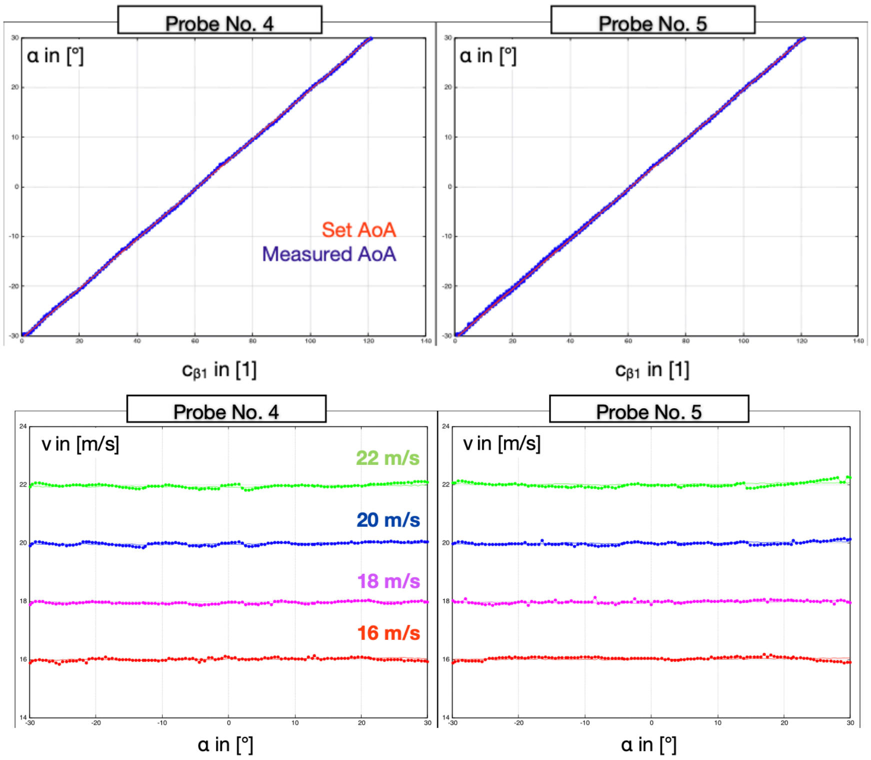
Page 10

L2 The flap effectiveness is usually (dCl/dbeta)/(dCl/dalpha). This is just the lift-flap slope here.

The wording is changed to lift-flap slope and the flap effectiveness is added in the manuscript.

L18 Is the calibration valid for the velocity and temperature range of the experiment?

In our point of view the calibration is valid for the current experiment. The calibration was conducted in a separate wind tunnel test. The probe was swiped from -30° to +30° and the velocity was set from 16 m/s to 22 m/s (please see the following pictures). Looking at the velocities measured in the experiment (Figure 15 in the original manuscript), a slight excursion below the lower velocity range does not seem to lead to extrapolation issues.



Furthermore, the Oldenburg wind tunnel is actively cooled. Therefore, the change in temperature between (and during) measurements is negligible.

Page 11

L5 Is it verified that this 2D correction is adequate, considering the test setup? Is it certain that there are no 3D effects?

We are confident that the 3D effects are negligible in this experiment, in particular as the wing employs an airfoil which is not a high lift airfoil, 3d effects are expected to be small.
Furthermore, a comparison of the equivalent rotating test setup on the Berlin Research turbine to an URANS solver was published in:

Klein, A. C., Bartholomay, S., Marten, D., Lutz, T., Pechlivanoglou, G., Nayeri, C. N., … Krämer, E. (2017). About the suitability of different numerical methods to reproduce model wind turbine measurements in a wind tunnel with high blockage ratio. *Wind Energy Science*, 1–27. https://doi.org/10.5194/wes-2017-42

Herein, a very well comparison between angle of attack and velocity measurements to the numerical results was found. Furthermore, as stated in the manuscript, a comparable approach, using 2d CFD instead of XFOIL is employed by Petersen et al.:

Petersen, M. M., Larsen, T. J., Madsen, H. A., & Larsen, G. C. (2017). Using wind speed from a blade-mounted flow sensor for power and load assessment on modern wind turbines, 547–567.

Petersen, M. M., Larsen, T. J., Larsen, G. C., Madsen, H. A., Larsen, G., & Troldborg, N. (2015). Turbulent wind field characterization and re-generation based on pitot tube measurements mounted on a wind turbine. *33rd Wind Energy Symposium*. https://doi.org/10.2514/6.2015-1467

Therefore, the 2D correction is regarded adequate for the current setup and for the rotating test rig.

L8 Quite a bold statement, considering a number of projects where Pitot tubes were used on full scale campaigns operating for long periods. Nevertheless the concern for long-term industrial application is fair. Maybe re-phrase?

We agree to your comment and we have therefore changed the sentence in order to clarify the difference between application within research projects and in ‘regular’ industrial application. An extensive analysis of pitot tubes / multi hole probes on wind turbine applications and the issues they suffer is given on page 12 of:

Cooperman, A., & Martinez, M. (2015). Load monitoring for active control of wind turbines. Renewable and Sustainable Energy Reviews, 41(0), 189–201. https://doi.org/10.1016/j.rser.2014.08.029

Page 13

L12 Is the linear approximation the one used in the end? Then there are no unsteady effects.

This equation might be misleading. Actually a_eff contains the unsteady effects (in particular wake memory, no added mass). Please refer to

Velte, C., Mikkelsen, R. F., Sørensen, J. N., Kaloyanov, T., & Gaunaa, M. (2012). Closed loop control of a flap exposed to harmonic aerodynamic actuation science of making torque from wind. In *Proceedings of The Science of Making Torque From Wind*.

where their Figure 7 (which is shown here below) shows alpha - lift hysteresis calculated with the same methodology. Thereby it is confirmed, that unsteady effects are included. Otherwise there would be just a flat line.

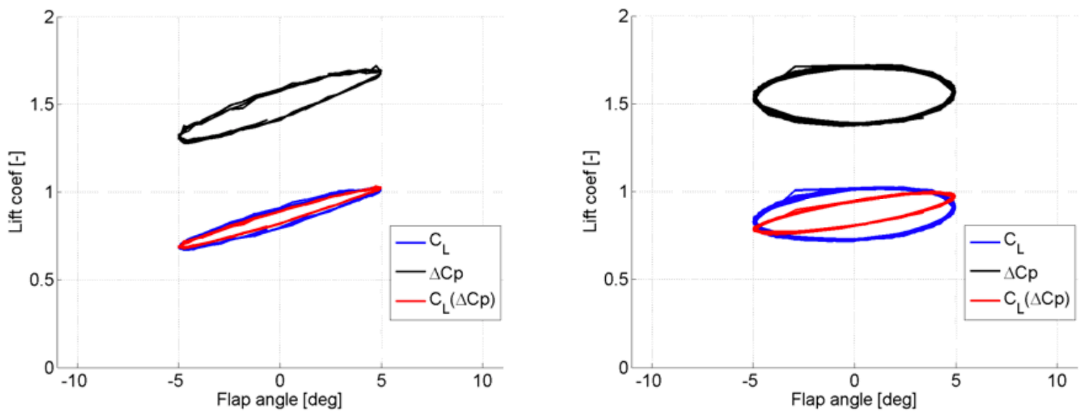


Figure 7 Lift vs. flap at reduced frequency k=0.054, left, and k=0.158 right.

Figure 4 Lift hysteresis plots; the calculation is based on the surface pressure port estimation method employed in the current paper (Velte et al 2012).

L 18 Why only linear function of flap angle, and not employ the look-up data? The delta(Cl) per flap angle is not constant for every angle of attack.

We generally agree to your comment, this would have improved the results. However, the effect is considered small as the presented results for the dynamic inflow lead to angle of attack excursion of +/-2° (remaining in the linear region of the lift curve) around the mean AOA of 5° (please compare to Figure 15 which is copied after this paragraph).
For future application this will be taken into account. Your point is included in the summary section to highlight the improvement potential.

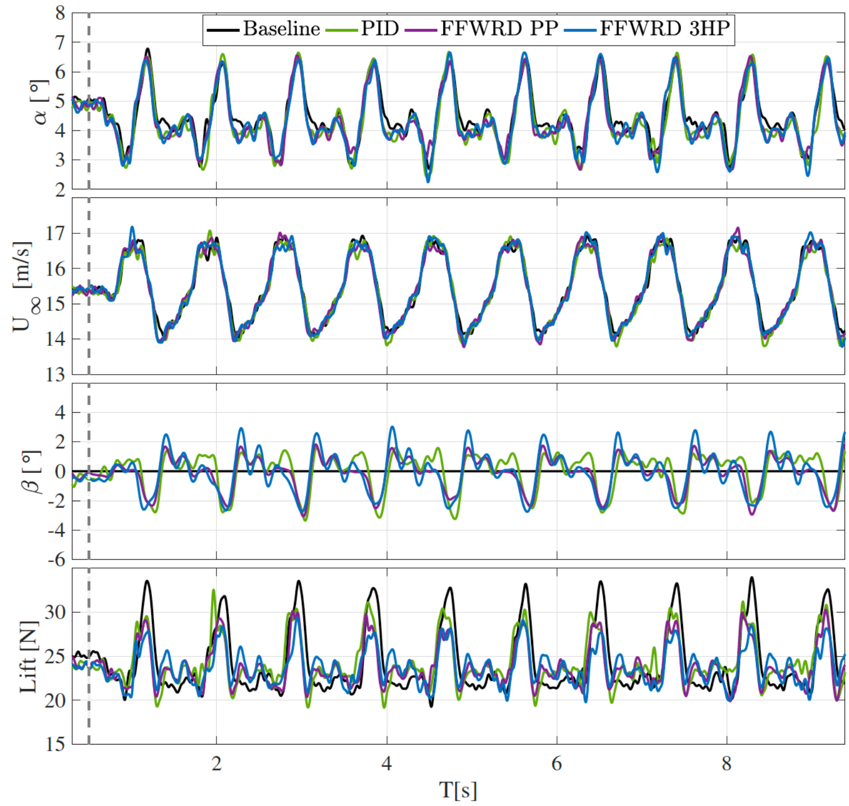


Figure 5 This is Figure 15 from the original manuscript. It is copied here to show that the angle of attack excursion remain in the linear region.

L19 It is latter shown that the free-stream velocity is fluctuating when introducing the flow disturbance? Is the mean only considered in the models?

Yes, the free-stream velocity fluctuates when the inflow disturbance is started. For calculation of the instantaneous lift according to equation 10, the instantaneous velocity is used during all experiments.

Page 14

L9 How was the Z-N method applied? Increasing Kp until self-oscillation etc on the experimental setup, or on a model? Was it done at a certain angle of attack? Was the force balance lift used?

Yes, the Ziegler-Nichols method was employed on the force balance measurement by increasing Kp until self-oscillation. The angle of attack was set to 5°, which corresponds to the operating point used in the dynamic inflow cases. The manuscript is extended by this information.

L16 Is the Lref from the force balance? Is it comparable to the Lest for steady conditions, or is there an error? (Indirectly answered later on in a way)

This is the Lref based on three-hole probe measurements. The subscript for Lref in Figure 8 and 9 are changed to clarify which Lref (balance, 3HP, PP) is used. Generally, controllers aim at reducing deviations away from an operation point which leads to a reduction of fluctuating loads. As a slight deviation in absolute values between balance and lift estimates is not ruled out, a value for the operation point, which corresponds to a 10 second mean lift, is set for the balance and each estimates (PP or 3HP) separately.

L16 There is again an assumption of linear lift-flap slope. Why not, a look-up approach? How well does this work away from the linear region (already at low angles of attack according to the polars)?

Agreed, for future application a look-up table would be beneficial. The idea was added to the summary section. Please refer to our answer to your comment on page 13 Line 18.

Page 15

L3 Similar question here.

Agreed, for future application a look-up table would be beneficial. The idea was added to the summary section. Please refer to our answer to your comment on page 13 Line 18.

Page 17

L3 Linear approximation again here.

Agreed, for future application a look-up table would be beneficial. The idea was added to the summary section. Please refer to our answer to your comment on page 13 Line 18.

Page 19

L8 Is it verified that unsteady effects are not included in neither of the methods (pointing back to a previous question).

The surface pressure measurement (PP) lift estimate takes into account wake memory effects, but no added mass effects. The three hole probe estimate does not take added mass or wake memory into account.

Page 21

L13 This is a considerable error, considering that any industrial application of active flaps will focus on measuring and controlling loads within 1-2%.

We agree to your point. However, as the lift estimation methods might be used within an additional feed-forward branch for a feedback controller, they can help to reduce the reaction time of the complete controller. The controller tuning would have to take into account the uncertainty of the feed-forward estimate.

Page 28

L22 It is generally obvious here that the model-based approach is challenged at high reduced frequencies. Is the model fidelity the main reason?

Higher frequencies lead to an increased model uncertainty as shown in section 6.2. Furthermore, due to the increasing phase lag due to update time (software) and flap mechanics (hardware) disturbances at higher frequencies are more difficult to control for the current setup. A detailed analysis of this is given in section 7.4 until 7.6