Wind Energ. Sci. Discuss.,
doi:10.5194/wes-2016-57-RC1, 2017

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Interactive comment on "Flow angle measurement of a yawed turbine and comparison to models" *by* Tyler Gallant and David A. Johnson

Anonymous Referee #1

Received and published: 7 March 2017

General comments:

The paper presents the results concerning the measurement of the angle of attacks of a yawed wind turbine model tested within a wind tunnel.

I personally do not like that the introduction session does not contain the state-of-the-art review, which seems anyway modest, not complete and focused on the Petersen et al. (2015) research, which concerns with the estimation of the far field velocity by using Multi-hole probes rather than with the measurements of the angle of attack for a yawed wind turbine. Moreovoer, it is not clearly highlighted the novelty of the approach presented in this paper.

The papers aims also at comparing the experimental data with the predictions of the model developed by Morote (2015). The readers must therefore be capable of under-

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standing, by reading the sole paper, the theoretical basis of the model, so as it must be clear the sequence of equations used by the model, which are the model inputs and how these are gathered (measured, modeled?) The description of the Morote model is however poor and unclear (see further detailed comments), and forces the readers to read the Morote paper just to gather a basic understanding of how the model works.

Moreover, it is claimed that the Morote model is more accurate if compared to the predictions of the model presented in Burton (2011). Considering this last sentence, as well as the paper title "Flow angle measurement of a yawed turbine and comparison to models", the experimental data must be compared with the predictions of more than one model. For example, data should be compared to the Burton model prediction, so as to prove that the Morote one is better. The paper must therefore provide enough information concerning the Burton model and must be highlighted the differences among the Burton and Morote models.

To support this last statement, consider the results shown in Figure 10 and 11, which clearly highlight that predictions are getting worse as higher the axial induction factor is (at r/R = 0.72 the axial induction factor is higher than r/R = 0.55). I expect that the predictions will be even worse for axial induction factor typical of multi-MW 3-bladed wind turbines. It is clear that the goal of this paper is not the one of presenting and validating a new method (Morote), but rather to compare exp. data with numerical predictions obtained using different models. However, if only the comparison between exp. data and Morote predictions is given, it seems that the goal of the paper is to validate a model which clearly seems not appropriate to predict the angle of attacks of either an-yawed wind turbines characterized by typical values of the axial induction factor. The only way to have this paper published in this journal is therefore to include comparisons between the exp. data and other numerical predictions.

Detailed comments

Page1, Line 24-25: "The angle of attack is therefore calculated using models (e.g. the

blade-element momentum method) using measurements several steps removed from the leading edge of the blade". Unclear sentence, please rephrase it. . Page 2, Line 20: The local flow angle (LFA) is indicated as \alpha in figure 1. Please change the figure

Page2, Line 28-29: "was essentially constant although they found a variation in \alpha of as much as 2.5 with azimuthal position that they described as the influence of the atmospheric boundary layer". Are the authors referring to the angle of attack or the local flow angle? Moreover, please make use of the punctuation so as to have a better readable sentence.

Page 3. Line 13-15. "These values were then used to calculate the blade angle of attack. Johnson et al. (2012) found that the method resulted in angle of attack estimates within 15-20% of airfoil design data indicating that the use of velocity measurements to validate five-hole probe measurements may be appropriate". Unclear sentence. What exactly means "within 15-20% of airfoil design data"? do you refer to the angle of attack at which the airfoil should operate at rotor optimal operating conditions?

Page 4. Line 5-6. "and subtracting the contribution to the relative wind speed caused by the rotation of the blade". Should it be: subtracting the contribution of the relative wind speed caused by the...?

Page 5. Line 5. "β is the blade pitch at the radial location of interest" It should be clarified that β is the sum of the twist at the radial location of interest and the blade pitch, which is instead radially constant

Page 5. Line 17. "where a0 is the radially dependent induction factor for axial flow at the blade lifting line" Page 5. Line 23 "Here, aa0 is the radially dependent azimuthally averaged induction factor for axial flow" What exactly is the difference between these two parameters? How are they gathered?

Page 5. Line 25. "The equation for g(r) is only valid at certain radial positions, which

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means a phase shift Δ is also required. This phase shift accounts for the change in azimuthal position of the blade as an air particle travels over the blade chord, which would result in a slight variation in the angle of attack along the chord line." g(r) and f(r) are radial-dependent function. What does it mean that they are only valid at certain radial positions? The text gives the idea that the phase shift is necessary to account for the change in azimuthal position that results in a variation of the angle of attack along the chord line. Does this mean that g(r) and f(r) are only valid at certain azimuthal positions, and therefore must be corrected of the phase shift?

Page 6. Line 6 Please replace δ with sinγ in the equation 14. There is no need of introducing extra symbol

Page 6. Line 6 The αgeom used in equation 14 is computed by means of Eq. 8 even when when the flow is not aligned with the rotor disk, i.e. when γ is different from 0? Please be more specific.

Page 8. Line 6-7. "The scale of these values was confirmed by the five-hole probe measurements using a method described by Petersen et al. (2015)." Please include the values computed with this method.

Page 8. Line 8. "Table 2. Summary of Model Axial Induction Factors." The Morote model makes use of a0(r) and aa0(r). Plese add a figures that report these values wrt the radial distance r. Explain also how they are calculated with PROPID and if, and how, they were compared with values gathered with the Petersen et al. method.

Page 8. Line 12-13. "Axial induction factors calculated using PROPID (PROPID, 2016) were used in combination with the Biot-Savart law to adjust local flow angle measurements to the blade angle of attack." Eq. 1 is used to compute the the local flow angle, and the introduction makes reference to Schepers and van Rooij (2008) and Shen et al. (2009) to explain how the angle of attack can be derived from the local flow angle by correcting for induction due to the bound circulation. The authors, however, claim that they are using a different approach. Please provide more details about the adopted

approach and how it differs from the ones of Schepers and van Rooij (2008) and Shen et al. (2009)

Page 9. Line 1. "slight non-uniformity in the flow at the Wind Generation Research Facility, as described by Best (2010)" Please depict the non-uniformity of the flow in a figure, and highlight that it could lead to not-negligible (3-4 degrees) variations of the angle of attacks like those reported in figure 5. This could be done, for example, by using the prediction depicted in Figure 12 to compute an azimuthally-variable αgeom, that, in turn, can be used to compute αeff with eq. 14.

Page 11 Line 1-2. "Discrepancies at λ = 3.6 are likely caused by inaccuracies in the calculated axial induction factors, as was confirmed by modeling the α distribution with arbitrarily changed a value" It would be usefull to see the dependency of the angle of attack to the induction factor, at least for one tsr.

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