Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2018-3-RC1, 2018


Open Access
WIND ENERGY SCIENCE DISCUSSIONS

# *Interactive comment on* "Dynamic inflow effects in measurements and high fidelity computations" *by* Georg R. Pirrung and Helge Aa. Madsen

**Anonymous Referee #1**

Received and published: 23 March 2018

The article deals with the so-called dynamic inflow effect, which takes place after a step change in pitch angle (among other situations involving a fast change of the rotor loading). The authors address some issues that had been described by other authors in the past and for which no satisfactory explanation exists so far. More specifically, the article focuses on the radial dependency of the dynamic inflow constants when pitching both towards lower and higher loading. Experimental and numerical observations done by other authors on the NREL Phase VI wind turbine are discussed in detail and explanations for the mentioned effects are presented. For this, the authors rely on the results obtained by means of a simple vortex cylinder wake model. The topic of the article is of sufficient relevance for the wind energy community and the questions addressed are within the scope of WES. The objectives of the article are clear. The scientific method



employed is appropriate for the current study. The discussion, as fas as this referee can judge, is relevant and backed up. The paper is well structured and well written. The authors give proper credit to relevant work and the literature is correctly cited. The following issues, however, should be addressed before the paper can be accepted for publication in this journal.

MAJOR COMMENTS:

1- The method of your analysis has already been applied by other authors for the study of the same effect on the same turbine. Your interpretation of the results is the main novelty of the paper since it contributes to understanding the pros and cons of the methods used in the past for the analysis of this aerodynamic effect. However, my impression is that no new knowledge about the dynamic inflow effect itself is generated with this work. This referee expects a more clear description of what we learn from this work with regard to the dynamic inflow effect (not with respect to its modeling).

2- The authors conclude that the use of a simple vortex wake model is preferred over other methods for studying dynamic wake effects because the induction in the rotor can be obtained from the model, what allows to obtain the time constants for dynamic inflow models without the detour of using aerodynamic forces. The vortex wake model, however, is based on rather crude assumptions, which are also described in the manuscript. Therefore, this referee would expect that obtaining the wake induction from e.g. CFD results and applying it to eq. 6 would be a more accurate and reliable method for determining the mentioned time constants.

3- A discussion on the applicability of the current results to large wind turbines is absolutely required. The pitch rate of the NREL Phase VI wind turbine was 66 degrees per second. This is very far from a realistic pitch rate for modern wind turbines. Which is, therefore, the relevance of this study for practical applications?

MINOR COMMENTS:

1- Is the vortex wake model the same as the one applied by Schepers (2007)? Is it a different implementation? What is new or different in your model?

2- Page 2, line 28: the load fluctuations are not only to be seen at the 80% station but also, to a lower extent, at the 63% radial station.

3- Page 2, last paragraph. Please state when the pitching step was finished.

4- Page 3, line 8: how realistic is to neglect 2D unsteady airfoil effects? Do you expect this assumption to have an impact on the reliability of your results?

5- Eq. 2: is the relationship between the induction and the loads always linear? What would happen if a low induction leads to the onset of stall? Can you still, in that case, apply this equation?

6- You refer several times to the simple vortex wake model as the analytical model and its results are also termed as analytical. Please explain why you consider this model to be analytical in contrast to the other models that have been used by other authors for the study of this effect.

7- Page 5, 1st paragraph: do you use any type of tip correction and why?

8- Page 5, line 15: is the flow non-uniformity of the rotor plane also neglected?

9- Page 6, line 10: please explain more clearly why in the loaded case the wake velocity is 4 times smaller than in the unloaded case.

10- Some minor typos exist throughout the whole text. Please correct them. Examples:

page 9, line 10: off → of

page 2, line 18: performet → performed

---

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2018-3, 2018.