Dear reviewer,

Thank you very much for the detailed review. We have clarified some points in the article and hope that we have sufficiently answered your questions below (the answers are indented).

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).

- It seems difficult to differentiate clearly between what we learn with respect to the effect from what we learn about modeling of the effect. The main points are that the apparently conflicting findings from previous work between experiment, high fidelity models and simple models are actually in agreement. But they appeared conflicting because the dynamic inflow analyses from measurements and high fidelity had to be based on force responses. This work suggest that the dynamic induction change predicted by a simple vortex model does not - as thought before - contradict the measurements and high fidelity computations. This shows that the effect itself is less complex than previously thought.

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.

- There seems to be a misunderstanding. Definitely CFD would be preferred over the presented simple vortex wake model when computing the force time series. The purpose of this article, however, is to show that it is difficult to extract dynamic inflow time constants from time series of the force response. It is also quite difficult to obtain a reliable unsteady induced velocity from CFD, because the induced velocity has to be computed from the flow field. This is why we state in the conclusions that free wake vortex codes (that are much more complex than the model used in the present work) have an advantage over CFD computations when it comes to determining dynamic inflow time constants: the time series of the induced velocity at the blades is directly available from the free wake vortex computations.

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?

- Dynamic inflow time constants are inversely proportional to the rotor radius, see Equation (4) in the article. Therefore, at the same free wind speed, dynamic inflow effects are 10 times slower for a moderately sized modern turbine with a diameter of 100 meters than for the Phase VI turbine (10 meters diameter). Thus, for the same ratio between pitch rate and dynamic inflow time constant a pitch rate of 6.6 degrees/second is sufficient. Since the step size of 16 degrees used in the experiment is very large, the pitch rate can be even lower if steps of a smaller size are considered. Thus the effects discussed can definitely occur on a modern turbine.
- The following sentence has been added: '*This pitch rate corresponds to 6.6 deg/s for a turbine with 100 meter diameter, because the dynamic inflow time constants are inversely proportional to the rotor radius (Sørensen and Madsen, 2006).*'

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?

- The following clarification has been added in the last paragraph before section 3.1:

*Previously this model was used to analytically derive the time constant for a step change in circulation at the rotor disc, which is transported downstream with the free stream velocity. In the present work, the position of the step change is varied and the wake velocity v_w, see Equation (4), is chosen as the downstream convection velocity. Equation (5) is integrated numerically.*

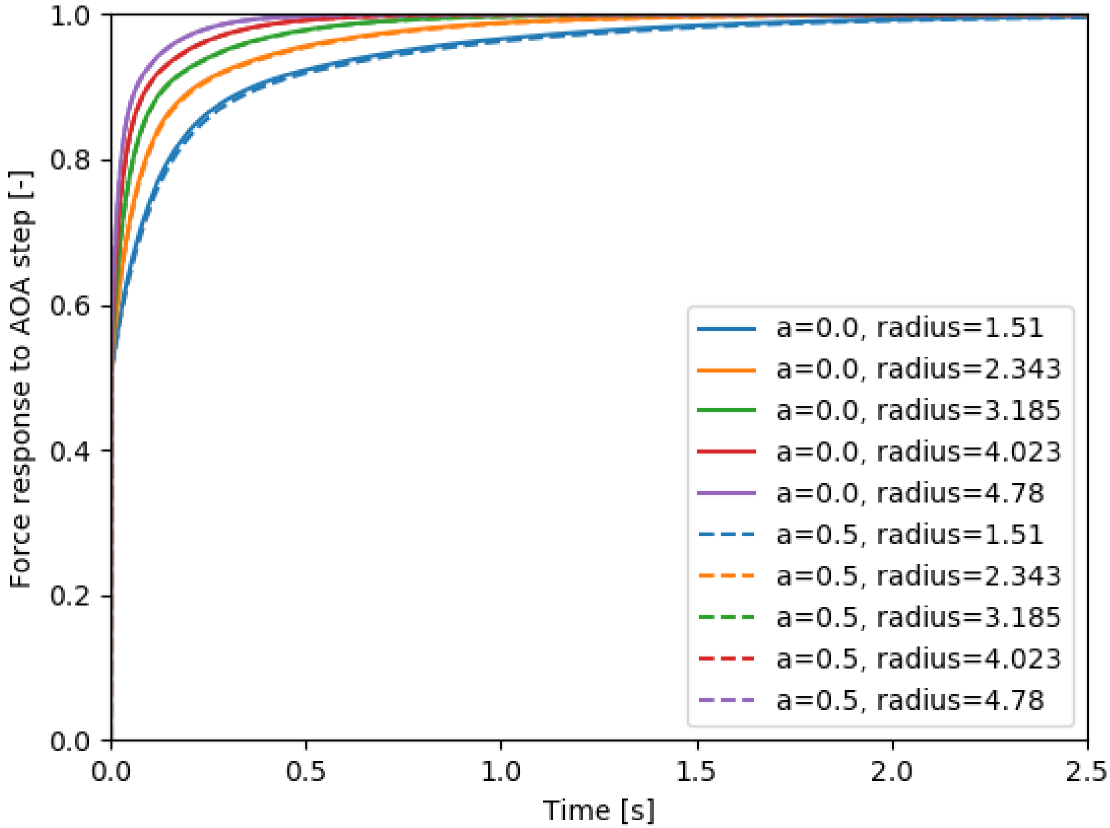
2- Page 2, line 28: the load fluctuations are not only to be seen at the 80% station but also, to a lower extent, at the 63% radial station.

- This is correct, and we state it in the revised article.

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

- At roughly 0.35 seconds in the plot where the maximum force overshoots are observed (except at the 80% station). This is stated in the revised article.

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?




- at 0.35 seconds, the value of the most inboard section has reached roughly 90% of the final value, the most outboard section 99%. This is much faster than the dynamic inflow effect (see Figure 3 in the article). There is no strong influence of the induction because the time constants depend on the local relative speeds, which are dominated by the relative speed due to rotor rotation. Therefore they don't add any more asymmetry between the loading and deloading of the rotor.

The following has been included in the article: *2D unsteady airfoil aerodynamics and dynamic stall are neglected. To justify this, the force response to a step change in angle of attack has been computed using Jones' equations for a flat plate and the relative speeds at the disc. After 0.35 seconds (corresponding roughly to the pitch step duration), the force response reaches between 90\% (most inboard section) and 99\% (tip section) of its quasi steady value, showing that this effect is much faster than the dynamic inflow effect.*

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?

- No, this is an assumption that fails for stalled areas of the blade. Simulations by Schepers showed however that stall only occurs immediately after the Pitch step in the case towards higher loading (see Figures 26 and 27 in Schepers (2007) and the flow attaches within 2 seconds. While this introduces some uncertainty, the averaging of the estimated time constants between 0.7 to 3.7 seconds will lead to improved results here, since the flow is attached for most of this window.

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.

- Analytical is used to state that the time constants can be derived directly from the model as opposed to using curve fitting on other models. This term has also been used by Schepers (2007). It doesn't mean in any way that the model is superior to other models.

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

- No tip correction is used. We have investigated the same effects using a helical vortex model (where the single blades are represented as opposed to the disc approach in the article). The conclusions on the radial dependency and the influence of the convection velocity from that model have been identical, so no tip loss correction is needed to show this behavior. It appears that the cylindrical wake model is the simplest model that is sufficient to explain the dynamic inflow effects. Therefore we focused on this model and removed the slightly more complex helical model to make the article more clear and concise.

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

The following sentence has been added in Section 4:
*'The induction from the trailed vortex cylinder is azimuth independent in the rotor plane but can vary radially.'*

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.

- We added the definition of the wake velocity in Equation 5. We clarified the explanation by adding a reference to Equation 5.

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

page 9, line 10: off → of

- this is corrected

page 2, line 18: performet → performed

- this is corrected