Author Response to Reviews of

Spatial development of planar and axisymmetric wakes of porous objects under a pressure gradient: a wind tunnel study

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RC: Referee Comment, AC: *Author Comment*

Referee 1

1. General considerations

RC: The manuscript is clearly written and presents interesting and generally well-documented results. I recommend the manuscript for submission provided that a few minor changes is made as described below.

AC: *We thank the referee for the time they have invested in this review and we are very glad to hear that they found the results interesting. We will address the referee's comments below. Major changes done to the main manuscript are highlighted in blue.*

2. Specific comments:

RC: L7: Please state that you only investigate positive pressure gradients - it is only for dp/dx>0 that you get deeper deficits and wider wakes (the opposite would be true for dp/dx<0)

AC: *'Adverse' has now been added to the sentence in line 7. It is indeed true that only an adverse pressure gradient results in deeper deficits and wider wakes.*

RC: L13-17: I think this paragraph is not entirely accurate. Strictly, you cannot predict the power output of a wind farm from the wake effect since there are several other effects that govern the power output. However, it is correct to say that it is essential to model wake effects accurately in order to predict wake losses and therefore also the total power output of a wind farm.

AC: *We thank the reviewer for this remark. In the new manuscript, this paragraph has been rewritten to focus on the relevance of turbulence and wind turbine farms. On the one side, we now discuss the overall interaction between wind farms and the atmospheric boundary layer, which governs the entrainment of energy. On the other side, we also remark the relevance of the wake effects and wake losses.*

RC: L78: The reference "Shamsoddin and Porte-Agel (2018)" seems to appear two times in your reference list (L382-385) so one of them should be removed.

AC: *Corrected.*

RC: L79: It is not correct to say inviscid since the work you refer to models a turbulent wake and turbulence inherently involves shear stresses.

AC: *The sentence has been slightly rewritten. The flow is not inviscid, but the viscous terms (among others) have been neglected from the conservation of momentum equation for a turbulent flow to derive the ODE.*

RC: L113: The parameters A, B, alpha and beta are related via momentum conservation, but you fit them as independent parameters. Why not make the fit while ensuring that they are still related in the right way? Does your approach imply that your wake profile does not fulfill momentum conservation? It would be good to include a sentence about this in the manuscript.

AC: *The reviewer is correct, and a 3-parameter fit (using the virtual origin and one exponent and one pre-factor) could be applied to adjust simultaneously the velocity deficit and wake width. Nevertheless, most of experimental works on turbulent wakes follow our approach and perform a 5-parameter fit (see Nedic et al. PRL (2013) and references therein). The reason is that the resolution in terms of streamwise positions need to be too high to guarantee high-quality fitting, and it is therefore better to fit both quantities independently while checking, a posteriori, their consistency in terms of momentum conservation. In this way, momentum conservation is assumed to be a property of the wake and verified using the output of the 5-parameter fit A small discussion on this point has been added to section 2.*

RC: L180: In the work by Neunaber et al. (2021) the reported drag coefficient includes the drag of the tower. In your work, you have no tower so one should expect a lower drag coefficient than what is reported by Neunaber et al. (2021). Have you measured the drag coefficient to confirm that it is indeed what you expect?

AC: *The reviewer raises a fair point. Indeed, the drag coefficient in our case should be slightly lower than the one of the cited work. Unfortunately, our experimental setup does not allow to measure the drag coefficient of the disk so we cannot confirm this value. In the work by Neunaber et al. (2021), the tower would increase the frontal area within the radius of the disk by at most 3.8% (since the tower has a 3.8cm diameter and the disk 59cm). If the total drag coefficient is proportional to this frontal area, that would mean that the drag coefficient, without the tower, would be around 0.92. Following this remark, in the new version of the manuscript, the thrust coefficient has been changed to* $C_T \approx 0.9$. *The same has been done for the thrust coefficient of the cylinder.* $C_T = 0.9$ *better reflects the accuracy of the approximation of the thrust coefficient.*

RC: L215: increasing is misspelled.

AC: *Corrected.*

RC: L230-: You could consider writing that some of the differences you observe between disc and cylinder is also reflecting that the cylinder is essentially a 2D flow case while the disk is more 3D. Generally, 2D bodies produce deeper wake than 3D bodies.

AC: *This is a good suggestion. A few sentences have been added about the general differences between the two flow cases.*

RC: L240: Why is wake of the cylinder skewed? Is it lack of statistical convergence or is the flow in the tunnel asymmetric?

AC: *The baseline velocity in the tunnel has been measured and for an empty test section the flow was found to be relatively symmetric. At least, no strong asymmetries to the extent that they would show up in the wake have been found, especially in the test section (the ZPG case). What could be possible is that the cylinder was positioned at a very small angle, such that the porous holes were not completely symmetrically aligned with the incoming flow. A sentence about this skewed wake has been added to the new version of the manuscript (section 4.1).*

RC: Caption figure 5 : “Radial velocity profiles” sounds like it is the radial velocity and not the streamwise velocity. What you mean is something like “Radial (or horizontal) profiles of the (streamwise) velocity.

AC: *The caption has been changed to the suggested horizontal profiles'.*

RC: L270-272: You mention that the best fit is obtained at an angle of 3 degrees and that this is not consistent with the best fit in the empty tunnel. You mention several reasons for this, but it could maybe also be due to uncertainties in the thrust/drag coefficient (which is not measured) or what?

AC: *The exact value of the thrust coefficient would be important if one would try to calculate the strength of the wake from the thrust coefficient alone. The model has however been applied to a measured value of the velocity deficit in the wake, after which the evolution of the wake is calculated (in a pressure gradient). If the thrust coefficient changes, this starting velocity deficit would have changed, so the starting point of the model would have also changed. After this point, the thrust coefficient is no longer an input in the model. Furthermore, the thrust coefficient would have changed the strength of the wake in both the ZPG and the APG cases, and the evolution of the ZPG wake is an input to the APG case. In consequence, we think that an uncertainty in the thrust coefficient is not a factor in the outcome of the model.*

RC: L289-L290: You write that "We remark that for this case, given the limitations of the experimental setup, the case where the wake evolves both across the test and the diffuser sections was not considered". However, Figure 1 indicates something different – namely that you did perform tests with the cylinder in the test section. Am I misunderstanding something?

AC: *Three cases have been considered and measured. The ZPG case, the APG case and the ZPG case that continues into an APG. The ZPG case and the APG case were done for both the disk and the cylinder, while for the disk we also covered the case where the disk was placed in the ZPG and measurements extended into an APG case. This was not done for the cylinder. The sentence has been slightly rewritten in the new manuscript to clarify this.*

RC: L305-308: To state that there is no Reynolds number effect is not entirely accurate when looking at Figure 7. I would say that there is a low sensitivity to Reynolds number.

AC: *We agree with this remark and no Reynolds number effects' has been changed to low sensitivity to Reynolds number'.*

AC: *We would like to sincerely thank the referee again for their time and for providing this* *valuable review. We believe that the referee's comments have improved the article and we hope that the article can be considered for publication in Wind Energy Science.*