Reply to the Reviewers comments

We thank the reviewer for the high level of detail in his review and constructive feedback. We have therefore revised and modified the original manuscript as to take into account most of his suggestions.

Main comments

$[Reviewer]$: You have chosen to separate the presentation of the results from the discussion of the results. I think it makes more sense to combine the results section with the discussion section and call it Results and Discussion. Then you can introduce a figure or table and directly discuss it, which is more natural to read. You could also make subsections in this section, e.g. a subsection about the grid study, a subsection about the validation with measurements, etc.

The proposed Actuator Disk (AD) method could be described in more detail, see specific comments below.

The results of the grid study show that your AD method does not converge monotonically for the investigated grid sizes. I think it is important to convince the reader that your AD method converges monotonically with grid size and I suggest that you redo this study based on the specific comments below.

$[Authors]$: The presentation of the paper has been adapted in accordance to the suggestions. Namely, there is now a combined Results and Discussion section where the results are introduced and subsequently discussed. Furthermore, a Conclusion section where the main conclusions of this study are presented has also been added.

The description of the Actuator Disc (ACD) has been modified to include the reviewers recommendations, see specific comments in the section bellow. Lastly, the grid study has fully been revised and is now based on the grid convergence error analysis for mixed order numerical schemes from Roy (2003).

Specific comments

1. $[Reviewer]$: Page 1, Abstract: In the abstract you have mentioned that the method has a low computational effort; however, the article lacks the results to show this. In addition, you have mentioned that a grid spacing of 40 cells per rotor diameter is sufficient, but this means that you need quite a lot of cells for wind farm simulations. Other RANS AD modelers would use 8 or 10 cells over a rotor diameter in wind farm simulations, where the grid spacing is based on a grid study.
   $[Authors]$: Results presenting required computational time has been added. In addition a comparison with a state of the art LES/ACL method has been made for case A. (Page 18, lines 13-18)

   Regarding the grid spacing used in this study, you are correct we decided to use a very high resolution that will probably not be the case for full wind farm simulations. Here we are interested in capturing the wake development in detail along a cross–section, hence the high resolution of the grid. Based on the new grid discretization study results (Page 10, Figure 3), the spatial discretization error for using a grid resolution of 10 cells per rotor diameter is approximately 5%.

2. $[Reviewer]$: Page 2, line 28: It would be worth to mention that a constant pressure drop resembles a uniformly distributed thrust force. Do you have any idea why this was chosen in the model wind turbine design? Real modern wind turbines have non-uniform thrust distributions.
   $[Authors]$: The suggestion to include ‘a constant pressure drop across the rotor, which resembles a uniformly distributed thrust force’ has been adopted (Page 3 lines 7-8). We agree with the reviewer that modern wind turbines used within the wind industry have a non-uniform thrust distribution. However in these wind tunnel experiments, the model wind turbine rotors are designed to have a constant pressure drop when operating at their design condition.

3. $[Reviewer]$: Page 3, line 1: You mention that the blockage ratio including the tower is 12%. If I calculate this ratio (without the tower) I get: $0.447^2\pi/(2.710h(x=3.66)) = 12.8\%$, where I have assumed that the height h of the wind tunnel is increasing linearly with downstream distance x: $h(x) = 0.05/11.150x + 1.801; h(x = 3.66) = 1.817$ m. Maybe

I misunderstood something?
[$Authors$]:Yes you are correct, the blockage ratio including the rotor and tower section below the tip of the rotor is approximately 13%. This has been corrected in the paper accordingly (Page 3, line 13).

4. [$Reviewer$]: Section 2.1: You could mention the Reynolds number of the experiments.
[$Authors$]: Following the reviewer suggestion the following has been inserted in the text: The tip Reynolds number for these three cases is approximately $Re_{c,tip} = 10^5$ for the upstream wind turbine. This tip Reynolds number is based on the velocity of the tip and the chord length at tip. For full scale experiments a typical tip Reynolds number is one order of magnitude higher in the order of 10^6 (Page 2, line 33 & Page 3, lines 1-2).

5. [$Reviewer$] Pages 5-6, AD method:

   (a) [$Reviewer$]: You use a reference to define your AD method, but I think it would be useful to present the complete method here since the article is focused on the validation of your AD method.
   [$Authors$]: The reference has been removed and the following has been included: The modified thrust coefficient curve is created in a pre–processing step by replacing the undisturbed wind velocity values of the thrust coefficient curve with the wind velocity values at the disc U_1. To do this Eq. (1) is used, where C_T is the thrust coefficient for the respective undisturbed wind velocity U_∞.

$$U_1 = U_\infty \left(1 - \frac{1}{2}\left(1 - \sqrt{1 - C_T}\right)\right). \tag{1}$$

   (Page 6, lines 6-10)

   (b) [$Reviewer$]: Eq. 8: How is the axial induction factor α_i calculated? Do you use $\alpha_i = \frac{1}{2}(1 - \sqrt{1 - C_T})$, implying that α_i is always a constant? If that is the case I would remove the index i and just write α.
   [$Authors$]: The axial induction factor at each cell α_i is calculated by $\alpha_i = \frac{1}{2}(1 - \sqrt{1 - C_{T,i}})$. The thrust coefficient for each cell is found from a look up table created in a pre–processing step. This look up table is actually a modified thrust coefficient curve based on the the velocity at the disc instead of the undisturbed wind velocity.

   (c) [$Reviewer$]: If you use $\alpha_i = \frac{1}{2}(1 - \sqrt{1 - C_T})$, are you aware that this relation can result in an overpredicted thrust and power? See for example the results of the AD induction method in van der Laan et al. [2]. How do you solve this issue in your AD method?
   [$Authors$]: There is a difference between how the thrust force is calculated in this method and the one described in van der Laan et al. van der Laan et al. (2015). Here the thrust force is estimated at each cell separately based on the velocity and induction factor at that cell. The total thrust is thus found by summing up these individual thrust forces. We are aware that both methods may lead to an overprediction of the thrust force as we have tested them before. We found however that this method results in a slight decrease of the overprediction.

   (d) [$Reviewer$]: If α_i is a constant and the inflow is uniform then the undistributed thrust distribution would always be the same as the uniformly distributed thrust, or do I miss something here? In other words, what is the difference in force distribution between Cases A and B in Table 4?
   [$Authors$]: In our case the axial induction (α_i) may vary over the disc as it depends on the velocity at the disc. When a uniform inflow is considered the difference between the uniform and undistributed thrust distribution is very small. However, when a sheared inflow is considered the difference is more perceptible as the wind velocity will vary more over the disc.

(e) $[Reviewer]$: Table 2: The Trapeze distribution is not defined for $0 < r < 0.2R$, what do you use in this region? It would be helpful to plot the different distributions.
$[Authors]$: For the Trapeze distribution there is no force applied in the region of $0 < r < 0.2R$, a note has been added in the table clarifying this (Page 7, Table 2). In addition according to your suggestion a plot of all four distribution has been included (Page 7 Figure 2).

6. Page 7-8, grid study:
$[Authors]$: A major review has been performed concerning the grid study. The grid convergence error is now based on the analysis for mixed order numerical schemes from Roy (2003) and not from the procedure described in Celik et al. (2008) as was previously. Therefore, Subsection 2.3 Grid convergence analysis (Page 8, lines 10-23 & Page 9, lines 1-8) has been introduced and in the Results and Discussion section new plots of the grid study have been added (Page 10, Figure 3) and the following text: Figure 3 presents the spatial discretisation error results obtained for the normalised axial velocity profiles at three distances downstream of the wind turbine position for case A. The error is estimated to be less than 2.4 % for finest grid (Grid 1). Therefore for the purpose of this investigation a uniform grid resolution of 40 cells per rotor diameter is found suitable for all cases. Also show in Fig. 3 are the normalized magnitudes of the first and second order error terms and of their sum, which are given by Eq. 15.(Page 9, lines 16-21)

   (a) $[Reviewer]$: Figure 2 shows that the wake solution is highly grid dependent. Based on these results it is difficult to state which grid is fine enough. You probably need to run a finer grid level to show grid convergence. This is also confirmed by the percentage of oscillating convergence at $x = 10R$ (45%) as listed Table 5. It probably means that your results are not yet in the range where the solution is monotonically converging with grid size.

   (b) $[Reviewer]$: Table 5, $\overline{p}$: From the grid study you have calculated an average order of accuracy of 3.58 and 5.92 at $x = 2R$ and $x = 10R$, respectively. How do you explain that the order of accuracy is so high considering the fact that you use a second order numerical scheme (central difference scheme)? You could try to also include a fourth grid level and perform a mixed order analysis as used in Réthoré et al. (2014) or in van der Laan et al. (2015).

   (c) $[Reviewer]$: Currently the grid study only covers one doubling of the coarsest grid. It would be more useful to look at bigger range of grid sizes, e.g. 5, 10, 20 and 40 or 10, 20, 40 and 80 cells over a rotor diameter.

   (d) $[Reviewer]$: How is the total thrust force behaving with grid size? If the total thrust force is oscillating with grid size it might be the reason why the results of the grid study are unsatisfactory.

   (e) $[Reviewer]$: Which turbulence model is used in the grid study? You could choose to perform a more basic grid study of your AD method by simulating an AD in a laminar flow (e.g. Re=100) without wind tunnel wall, such that you do not need a turbulence model.

7. $[Reviewer]$: Page 10, Figure 4: You could normalize the velocity contours with the freestream velocity at hub height.
$[Authors]$: Thank you, your suggestion has been adopted the plots are now normalized (Page 12, Figure 5).

8. $[Reviewer]$: Page 11, Figure 6: Is the thrust coefficient different for each thrust force distribution (as shown for the double wind turbines cases in Table 8)? If this is the case, it is difficult to isolate the influence of the thrust distribution on the wake flow. Ideally one could keep the total thrust force constant and only change the distribution.
$[Authors]$: You are correct, if one would like to investigate the influence of the thrust distribution on the wake without using this method one would ideally set the total thrust force to a constant for each distribution. In this method however the total thrust force is calculated at each iteration from the velocity at the disc. Following, the velocity at the disc is as well updated at each iteration by taking into account the thrust distribution at the disc and so forth until the set

convergence criteria is reached. Therefore each thrust distribution will intrinsically create a different flow field at the rotor disc that will in turn effect the total thrust over the disc. To make this clearer the following has been added:

Two questions thus arise, i.e. which thrust distribution within this ACD method better captures the wake produced by a wind turbine and up to which distance does the thrust distribution have an effect on the wake? Here it should be noted that the primary goal is not to isolate the influence of the thrust distribution on the wake flow, as the total thrust over the disc will intrinsically vary depending on the thrust distribution used within the method. Here the goal is to investigate the combined effect of the ACD method with different thrust distributions on the wake flow. (Page 6, lines 25-29).

9. $[Reviewer]$: Page 13, Lines 1-2: Based on Figure 5b, it does not seem that the k-ε model predicts similar k levels compared to the measurements, since it produces 2-7 times smaller k levels at $r = \pm R$.
$[Authors]$: This sentence has been removed. (Page 11, line 6)

10. $[Reviewer]$: Page 16, Lines 5-10 and Table 6. I think a 10% difference in thrust coefficient is quite a lot, especially for the upstream wind turbine.
$[Authors]$: The accurate calculation of the thrust distribution continues to be a challenge as seen by the results from Pierella et al. (2014); Bartl and Sætran (2017). We agree with the reviewer that 10% difference in the thrust for the upstream wind turbine is quite high, however in our case the difference for the upstream wind turbine is on average 5%. For the downstream wind turbine the average difference increases to 10%, which is still lower than the value found by far more complex and computationally intensive methods as seen in the results from the above mentioned papers. To clarify this the following has been added to within the text: There is on average a 5 % difference between the measured thrust and the results for the upstream wind turbine and less than a 10 % difference for all cases concerning the downstream wind turbine with an exception of the results when using the undistributed or uniform thrust. (Page 14, lines 1-4)

11. $[Reviewer]$: Page 17, Lines 4-5: Maybe you should note that the uniformly distributed thrust might not be the best one if a real size wind turbine is modeled that typically has a non-uniform thrust distribution.
$[Authors]$: Your suggestion has been added as follows: Please note however that the uniformly distributed thrust might not be the best one when considering near wake effects, if a real size wind turbine is modeled that typically has a non-uniform thrust distribution (Page 16, lines 5-6).

References

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