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.

1 General comments

$[Reviewer]$: The paper compares wake flow and thrust predictions by an actuator disc approach with experimental data from NTNU's Blind test experiments. Specifically, the performance of four different turbulence closure models and five radial thrust distributions on the actuator disc are investigated. The model turbines' performance is analyzed for a single and double turbine setup and different turbulent inflow conditions.

Given the advantages of lower computational effort compared to LES/DES models or fully-resolved RANS model, this approach is considered to be relevant for a fairly accurate modeling of wind farm flows.

The paper follows an elaborate line of reasoning and brings up a number of clear conclusions. However, the manuscript is missing some crucial elements. A comprehensive literature review on the field of wake modeling is not included, neither are the results discussed with respect to the state-of-the-art in wake modeling. Due to the lack of a wider context the current manuscript does not yet clearly demonstrate the advantages of the chosen modeling approach over other methods.

$[Authors]$: We agree with the reviewer that this model has some advantages over other models in terms of computational requirements. Hence, we have included a comparison between this method and a LES/actuator line (ACL) method for case A (the one wind turbine case). Further by giving a broader context in the field of wake modelling to the manuscript we recognize the added value it presents. Thus, the introduction now contains information regarding other wake modelling methods. Similarly, the computational advantages in relation with a state-of-the art (LES/ACL) method is discussed in the discussion section. The changes are presented in the specific comments in the section bellow.

2 Specific comments

1. $[Reviewer]$: Firstly, I do not completely agree with the chosen structure of the manuscript. The content of chapters "3 Result" and "4 Discussion" is complicated to follow in the current structure.
   $[Authors]$: The structure of the manuscript has been revised. There is now a Results and Discussion section and a separate Conclusion section.

   (a) $[Reviewer]$: I would suggest creating a new chapter 3 called "Precursor simulations" in which the empty tunnel simulations as well as the grid independency study are shortly described. The empty tunnel simulations (Fig. 3 and Fig. 9) as well as the grid independency study (Fig. 2) are rather boundary conditions than actual results in my opinion. Probably, it is not even necessary to show all modeled and measured inflow profiles rather than shortly mentioning that the match is very good (and stating a deviation in %).
   $[Authors]$: A new subsection named "Grid convergence analysis" has been introduced that describes the grid independency study following the reviewers suggestion (Page 8, lines 10-23 & Page 9, lines 1-8). Moreover, we agree with the reviewer that the plots shown in (Fig. 3 and Fig. 9) as well as the grid independency study plot (Fig. 2) are not part of the validation procedure. However, they are part of the verification procedure which we judge adds value to the manuscript, therefore we would like to keep them.

   (b) $[Reviewer]$: In my opinion it would be more straightforward to discuss the results in the actual "results" chapter rather than separating results and their discussion.
   $[Authors]$: Thank you for this suggestion, we have adopted it. There is now a results and discussion section in which the results are presented and subsequently discussed. (Section 3)

(c) [$Reviewer$]: A “discussion” chapter, however, still would be essential to include. Therein, the main findings should be discussed with respect to previous findings in the literature. So far, there is only one reference (Laan et al., 2015) included in the discussion, but there is a huge variety of publications dealing with numerical wake simulations by now. It would add great value to the manuscript to discuss the observed effects with respect to other simulations (ACD, but also other RANS approaches (ACL or fully-resolved)).

[$Authors$]: In accordance to the reviewers suggestion the following points have been introduced within the manuscript: “Porté-Agel et al. (2011) also observed that the effect of representing differently the forces of a wind turbine, such as by a rotating or non-rotating ACD or an ACL was more pronounced in the near wake region, rather than in the far wake region. ” (Page 11, lines 13-15)

“This is also observed by other researchers such as Réthoré et al. (2014), which concluded that the ACD method lacks the ability to simulate the turbulent structures present in the near wake region. Sumner et al. (2013) results also show that in a low background turbulence intensity environment there seemed to be a perceptible dependency of the wake development on the turbulence closure used, in terms of velocity deficit and turbulent kinetic energy.” (Page 12, line 3 & Page 13, lines 1-3)

“This finding is in agreement with results from the study by Laan et al. (2015b); Sumner et al. (2013), where small differences between the simulated wakes are also found when different RANS turbulence models are used in a high background turbulence intensity environment.” (Page 17, lines 2-4).

(d) [$Reviewer$]: It could be useful to create a new chapter “Conclusions”, starting from l.34 on p.16.

[$Authors$]: A Conclusion section has been introduced following this suggestion.(Section 4)

2. [$Reviewer$]: Secondly, several aspects of the thrust modeling require some deeper explanation. As the variation in radial thrust distribution is one of the two major parameters varied in this study an in-depth explanation of its modeling is deemed to be crucial.

(a) [$Reviewer$]: A more elaborate description of the choice of thrust distributions and the associated parameters is needed. A plot showing C_T (or a) vs. r/R comparing the distributions given by the equations in Table 2 would help to illustrate the approach. How are the parameters b (Table 2) chosen?

[$Authors$]: A plot of the distributions given by the equations in Table 2 is now introduced. Here the normalised thrust distributions $f_{Distribution}/f_{Uniform}$ vs. r/R are shown (Page 7, Figure 2).

The parameter b, is found by insisting that $F_{tot.} = \int f_{Distribution} dA$, basically that the applied distributed thrust force over the disc is equal to the total thrust force.

Regarding the reasoning behind the choice of each distribution: (i) the uniform distribution is chosen to match the thrust distribution of the actual rotor of this case. (ii) It is known that full scale wind turbines have a zero thrust value at the position of the hub and at the tip of the blades. The polynomial distribution is a fourth order polynomial intended to respect this. Hence it is fitted to respect $F_{tot.} = \int f_{Distribution} dA$ and to have a zero thrust at the hub and at the tip of the disc. (iii) The triangular distribution is designed to have a zero thrust at the hub and to increase linearly the thrust force along the radius, up to the tip of the disc. (iv) Lastly, the trapezoidal distribution is set-up to resemble the thrust distribution produced using the actuator line method presented in Sarmast et al. (2012).

The following has been introduced within the manuscript: “The uniform distribution is chosen to match the thrust distribution of the actual rotor of this case. Full scale wind turbines however have a zero thrust value at the hub and at the tip of the blades, the polynomial distribution which is a fourth order polynomial is intended to respect this by having a zero thrust at the hub and at the tip of the disc. The triangular distribution is designed to have a zero thrust at the hub and to increase linearly the thrust force along the radius, up to the tip of the disc. Lastly, the trapezoidal distribution is set-up to resemble the thrust distribution produced using the actuator line method presented in Sarmast et al. (2012).” (Page 6, lines 15-21)

(b) [$Reviewer$]: The distribution of the axial induction (or thrust) is not necessarily uniform along the rotor radius, depending on the rotor design and operational state. However, it should be possible to calculate radial distribution of the axial induction factor a and thus the thrust for a given rotor design and operating point. A simple Blade Element Momentum code or turbine modeling tool (FAST, QBlade, ...) should do the job, if the rotor geometry and airfoil polars are available. To my understanding it thus should be possible to define a thrust distribution and eliminate it as a variable.

$[Authors]$: While it is possible to determine a thrust distribution given the rotor geometry and airfoil data through a BEM theory, it is somewhat impractical for industrial applications. Airfoil data of commercial wind turbines are generally not available to the typical industrial user. And as the goal of this method is to be used for industrial application by a typical industrial user, where airfoil data are not available. It therefore seems appropriate to test different thrust distributions and the sensitivity of the method to these distributions. The following has been added within the manuscript: "While it is possible to determine a thrust distribution given the rotor geometry and airfoil data through a blade element momentum theory, it is somewhat impractical for industrial applications. Airfoil data of commercial wind turbines are generally not available to the typical industrial user."(Page 6, lines 21-24)

(c) $[Reviewer]$: Furthermore, It is not clear to me, how the downstream turbine's thrust coefficient $C_{T,T2}$ is calculated in cases B and C. Is it calculated from the fluid-ACD interaction or is the experimental $C_{T,T2}$ value used as an input? Please elaborate on the very short explanation given in l.5, p.7. See also comment 3 (c).

$[Authors]$: The applied thrust coefficient is found from the velocity at the disc (fluid-ACD interaction) and a look up table (modified thrust curve) which in turn is created based on the manufacturers thrust curve, in this case the experimental C_T curve. In more detail for the downstream turbine the thrust coefficient $C_{T,T2}$ is calculated in cases B and C by $C_{T_{T2}} = \frac{2F_{tot,T2}}{\rho U_{ref,B}^2 A}$ and $C_{T_{T2}} = \frac{2F_{tot,T2}}{\rho U_{ref,C}^2 A}$ respectively. The total thrust at the turbine in found by $F_{tot} = \sum F_i$, where F_i the thrust force at each cell. The thrust force at each cell is found by $F_i = C_T(U_{1,i}) \frac{1}{2} \rho \left(\frac{U_{1,i}}{1-\alpha_i}\right)^2 A_i$. Where $C_T(U_{1,i})$ is a modified thrust coefficient. This 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. This is done by using Eq. (1) showed below, where C_T is the thrust coefficient for the respective undisturbed wind velocity U_∞.

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

The explanation of how the modified thrust coefficient curve was missing from the manuscript. In the revised document we have included it by inserting the following: "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. (2) is used, where C_T is the thrust coefficient for the respective undisturbed wind velocity U_∞.

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

(Page 56 lines 6-10)

Furthermore, a typo has been corrected in the definition of the thrust coefficient it is now corrected to "

$$C_T = \frac{2F_{tot}}{\rho U_{ref,B}^2 A}$$

" (Page 7, line 11) and "

$$C_T = \frac{2F_{tot}}{\rho U_{ref,C}^2 A}$$

" (Page 8, line 9)

(d) $[Reviewer]$:Can you elaborate on what is meant by “undistributed” thrust? I did not find an explanation on that.
$[Authors]$: By undistributed thrust what is meant is that

$$F_i = C_T\left(U_{1,i}\right)\frac{1}{2}\rho\left(\frac{U_{1,i}}{1-\alpha_i}\right)^2 A_i$$

(Eq. (8) within the manuscript) is used as it is, in finding the thrust force to apply over each individual cell i, over the disc. Therefore, we skip the step of calculating the total thrust force and then redistributing the thrust force as done with the other distributions. In the revised text an explanation of this is stated as follows: “In this work, apart from using Eq. (8) as it is to prescribe the forces in each individual cell, referred to as the undistributed thrust, four different thrust distributions are tested: a uniform, a polynomial, a triangular and a trapeze distribution.” (Page 6, line 12-14)

3. $[Reviewer]$: Finally, the scientific contribution of this work to the field of numerical wind turbine wake modeling should be stated in a clearer way.

(a) $[Reviewer]$: Elaborate in the introduction why you chose the presented modeling approach. What is the advantage of RANS-ACD modeling compared to other numerical modeling techniques (LES/DES, ACL, ...)? Can you present some numbers justifying this approach with respect to computational effort (time)? Would this modeling approach thus have significant advantages in the modeling of a full wind farm?
$[Authors]$: Following the reviewers comments, the following has been intoroduced: “Advanced methods of wake modelling with CFD may be done by using large eddy simulation (LES) techniques in which the wind turbine forces may either be prescribed with an actuator line method (ACL) or an ACD method. Work along these lines has been performed by numerous researchers such as Breton et al. (2014); Nilsson et al. (2015); Churchfield et al. (2012); Andersen et al. (2015); Calaf et al. (2010); Ivanell et al. (2007). Although LES provides high fidelity results comparable to field measurements, the computational requirements of the method (Churchfield et al. (2012); Laan et al. (2015b)) is still too expensive and therefore not yet suitable for engineering practices of whole wind farm wake computations. A less computationally expensive alternative to LES are Reynolds averaged Navier-Stokes (RANS) simulations. RANS simulations have been used with the ACD method to simulate wind turbine wakes by numerous researchers, e.g. Laan et al. (2015a); Prospathopoulos et al. (2011); El Kasmi and Masson (2008).” (Page 2, lines 1-9)

(b) $[Reviewer]$: As stated in comment 1 (c) already, a discussion of the presented results with respect to state-of-the-art numerical wake modeling is deemed to be crucial. A discussion of both the approach and results by referring to other simulations would set this work into a broader context.
$[Authors]$: We thank the reviewer for his insight. Accordingly as presented in the answer to comment 1 (c), points have been inserted within the manuscript that refer and compare the work of other researchers in the field to the results of this study. In addition to these, a computational time comparison between this method and a state-of-the-art LES/ACL method for case A, has also been introduced as follows: “Lastly, in terms of computational time or CPU hours, herein defined as the number of CPUs $\times$ wall clock time needed to perform the simulation, results are shown in Table **??**. These results present the CPU hours needed to perform the simulations using this method and a LES with the actuator line method described in Sørensen et al. (2015) for case A. It is found that the RANS/ACD method is significantly faster in simulating this one wind turbine case compared to the LES/ACL method. Although the LES/ACL method provides high fidelity results comparable to the measurements, the computational requirements of this method, up to this day, are still to demanding to make it usable for wake modelling in industrial applications.” (Page 18, lines 13-18)

(c) $[Reviewer]$: The presented simulations of mean velocity and turbulent kinetic energy show very promising results, especially those simulated in a highly turbulent environment. However, I do not understand how the upstream turbine’s $C_{T,T1}$ and especially the downstream turbine’s thrust coefficient $C_{T,T2}$ are dependent on experimental

information. For the modeling of a bigger wind farm, it would be important to be able to calculate the coefficients based on the information given in turbine data sheets only.
$[Authors]$: The method does not rely on experimental information apart from the thrust coefficient curve provided by the manufacturer. Therefore, in order to model a wind farm, only information from the turbine data sheets are needed. The answer to comment 2(c) (above) describes how the thrust coefficients $C_{T,T1}$ and $C_{T,T2}$ are calculated.

(d) $[Reviewer]$: Finally, it should be stated if and how the presented modeling approach is reliable with respect to simulations of other wind turbines and different wind conditions. Which part of the modeling still comprises uncertainties? What would be suggestions for further developments on the proposed modeling?
$[Authors]$: We thank the reviewer for his insight. The following is included in the manuscript: "This method has shown to give reliable results for a number of different wind flow conditions and separation distances with respect to the single and the two in-line wind turbine cases. However it has not been validated yet for wind turbines operating in a situation where only a part of the rotor is in the wake of the upstream wind turbine (partial wake situation). Moreover, it has not been validated either against operational data measured within existing wind farms operating in full scale atmospheric conditions. Therefore, future research will focus on validating the method against data retrieved from operating wind farms. Cases in which wind turbines are operating partially in the wake of the upstream turbine will be of special interest as well." (Page 19, lines 15-20)

3 Technical corrections

- $[Reviewer]$: p.1: Abstract: state in one sentence which turbulence model performed best under which flow conditions.
$[Authors]$: When using the $k-\varepsilon$ and $KL\ k-\varepsilon$ turbulence models the results are generally in closer agreement with the measurements.(Page 1, lines 11-12)

- $[Reviewer]$: p.1, l.17: "... CFD code and to the..."
$[Authors]$: This has been adopted(Page 1, line 18)

- $[Reviewer]$: p.1, l.19:"... large wind turbines..", specify what "large" and "small scale" (l.21) is. D = 10m are still model scale
$[Authors]$: We removed the word "large" not to create confusion (Page 1, line 20)

- $[Reviewer]$: p.2, l.25: "... design conditions." Specify what these design conditions are (TSR=?)
$[Authors]$: The design condition in TSR is now specified. The following has been inserted: "i.e. tip speed ratio of six (TSR=6)" (Page 3, line 4)

- $[Reviewer]$: p.3, l.21: "... created by a bi-planar..."
$[Authors]$: This typo has been corrected accordingly i.e bi–planer has be corrected to bi–planar(Page 3, line 33)

- $[Reviewer]$: p.5, l.28: "... a triangular and a trapezoidal distribution." (word trapeze/trapezoidal reoccurs at several places in text an tables).
$[Authors]$: The word trapezoidal instead of trapeze has been adopted throughout the manuscript.

- $[Reviewer]$: p.6, Table 2: as mentioned above: a plot showing the different distributions would be illustrative.
$[Authors]$: A plot showing the different distributions has been introduced (Page 7, Figure 2)

- $[Reviewer]$: p.7, Table 4: as mentioned above: what does "undistributed" mean?
$[Authors]$: This hopefully now has been clarified in the comment above.

- $[Reviewer]$: p.7, l.1: " ... ACDs are?"
$[Authors]$: This correction has been adopted, ACD has been changed to ACDs (Page 7, line 8)

- $[Reviewer]$: p.7, l.4: "... thrust coefficients CT = (...) wind turbine are..."
$[Authors]$: This correction has been adopted, coefficient has been changed to coefficients (Page 7, line 11)

– [$Reviewer$]: p.10, Fig.5 (a) and (b), p.11, l.5 and p.13, l.4: it is first stated that the k-epsilon and the KL k-epsilon model produce similar results on p.11, while on p.13 it is stated that the RNG k-epsilon tends to underpredict the wake recovery. Judging from Figures 5 (a) and (b) I hardly see any difference in the results by the k-epsilon and RNG k-epsilon model. [$Authors$]: We thank the reviewer for his remark. You are correct the RNG k-epsilon in case A does not underpredict the wake recovery. The following sentence has been removed: In contrast the $RNG\ k-\varepsilon$ turbulence model tends to under-predict the wake recovery slightly and turbulent kinetic energy production. (Page 11, line 8)

– [$Reviewer$]: p.13, l.1: it is stated that the TKE profiles in Fig. 5(b) are “not successfully predicted by any of the turbulence models”. Could you elaborate on reasons for this giving a source from literature? Is this due to the weak performance of RANS in low turbulent environments? Is there an influence of the nonexistence of tip-vortex-shedding in ACD models on the TKE profiles?
[$Authors$]: Our results that the ACD method does not seem to capture the shape and the increased TKE present downstream of the tip of the blades in the near wake region, seem to agree with results and conclusions from other researchers. We have included the following within the manuscript: This is also observed by other researchers such as Réthoré et al. (2014), which concluded that the ACD method lacks the ability to simulate the turbulent structures present in the near wake region. (Page 12, line 3 & Page 13, line 1)

– [$Reviewer$]:p.14, l.1: “...capture the position of the tip vortex apart from the polynomial.”
[$Authors$]: This correction has been adopted, tip vortices has been changed to tip vortex . (Page 12, line 1)

– [$Reviewer$]: Why are there several peaks appearing in Fig. 6(b)? Can you double-check for convergence?
[$Authors$]: Regarding the polynomial distribution, the peaks in the TKE appearing in Fig. 6(b) (Fig. 7(b) in reviewed version) are connected to the applied thrust distribution and the resulting shear or velocity gradients downstream of the wind turbine. As may be observed they are more pronounced in the wake region near the wind turbine rotor i.e. at x=2R.

– [$Reviewer$]: p.16, l.22-25: “As the purpose (...) from representing differently (...) using different turbulence models.” This is a very long and hard-to-understand sentence; especially the “representing differently” part needs revision.
[$Authors$]: We thank the reviewer for this comment, the sentence has been divided into two sentences as follows: “As the purpose of using different turbulence models in this study is to investigate the effect of the turbulence model with its defined constants on the wake development. It is crucial to set throughout the domain the background turbulence intensity in accordance to the experimental set–up when using different turbulence models.” (Page 18, lines 1-4)

References

Andersen, S. J., Witha, B., Breton, S.-P., Sørensen, J. N., Mikkelsen, R. F., and Ivanell, S.: Quantifying variability of large eddy simulations of very large wind farms, in: Journal of Physics: Conference Series, vol. 625, p. 012027, IOP Publishing, 2015.

Breton, S.-P., Nilsson, K., Olivares-Espinosa, H., Masson, C., Dufresne, L., and Ivanell, S.: Study of the influence of imposed turbulence on the asymptotic wake deficit in a very long line of wind turbines, Renewable energy, 70, 153–163, 2014.

Calaf, M., Meneveau, C., and Meyers, J.: Large eddy simulation study of fully developed wind-turbine array boundary layers, Physics of fluids, 22, 015 110, 2010.

Churchfield, M. J., Lee, S., Moriarty, P. J., Martinez, L. A., Leonardi, S., Vijayakumar, G., and Brasseur, J. G.: A large-eddy simulation of wind-plant aerodynamics, AIAA paper, 537, 2012, 2012.

El Kasmi, A. and Masson, C.: An extended k–ε model for turbulent flow through horizontal-axis wind turbines, Journal of Wind Engineering and Industrial Aerodynamics, 96, 103–122, 2008.

Ivanell, S., Sørensen, J. N., Mikkelsen, R., and Henningson, D.: Numerical analysis of the tip and root vortex position in the wake of a wind turbine, in: Journal of Physics: Conference Series, vol. 75, p. 012035, IOP Publishing, 2007.

Laan, M. P., Sørensen, N. N., Réthoré, P.-E., Mann, J., Kelly, M. C., Troldborg, N., Hansen, K. S., and Murcia, J. P.: The k-ε-f$_P$ model applied to wind farms, Wind Energy, 18, 2065–2084, 2015a.

Laan, M. P., Sørensen, N. N., Réthoré, P.-E., Mann, J., Kelly, M. C., Troldborg, N., Schepers, J. G., and Machefaux, E.: An improved k-ε model applied to a wind turbine wake in atmospheric turbulence, Wind Energy, 18, 889–907, 2015b.

Nilsson, K., Ivanell, S., Hansen, K. S., Mikkelsen, R., Sørensen, J. N., Breton, S.-P., and Henningson, D.: Large-eddy simulations of the Lillgrund wind farm, Wind Energy, 18, 449–467, 2015.

Porté-Agel, F., Wu, Y.-T., Lu, H., and Conzemius, R. J.: Large-eddy simulation of atmospheric boundary layer flow through wind turbines and wind farms, Journal of Wind Engineering and Industrial Aerodynamics, 99, 154–168, 2011.

Prospathopoulos, J., Politis, E., Rados, K., and Chaviaropoulos, P.: Evaluation of the effects of turbulence model enhancements on wind turbine wake predictions, Wind Energy, 14, 285–300, 2011.

Réthoré, P.-E., Laan, P., Troldborg, N., Zahle, F., and Sørensen, N. N.: Verification and validation of an actuator disc model, Wind Energy, 17, 919–937, 2014.

Sarmast, S., Mikkelsen, R. F., and Henningson, D.: Numerical simulations of wake interaction between two inline wind turbines, Tech. rep., KTH, Stability, Transition and Control, qC 20130412, 2012.

Sørensen, J. N., Mikkelsen, R. F., Henningson, D. S., Ivanell, S., Sarmast, S., and Andersen, S. J.: Simulation of wind turbine wakes using the actuator line technique, Phil. Trans. R. Soc. A, 373, 20140 071, 2015.

Sumner, J., Espana, G., Masson, C., and Aubrun, S.: Evaluation of rans/actuator disk modelling of wind turbine wake flow using wind tunnel measurements, International Journal of Engineering Systems Modelling and Simulation 47, 5, 147–158, 2013.