1 Response to WES2017-16 RC2

The authors thank the referee for the helpful review. The list of issues that were brought up by the referee is addressed in the following.

General comments

In this manuscript, a methodology is presented, in which a large-eddy simulation (LES) model is combined with a numerical weather prediction (NWP) model to simulate the flow through an offshore wind farm. The results of this combination of models are compared with field measurements (both lidar and met mast) in an offshore wind farm in the North Sea. The topic is interesting and it is a direction, in which wind energy community should/will eventually move. Nevertheless, there are some issues that the authors should address, before this manuscript can officially get published. These issues are listed below:

Comment 1

The statement that the authors have made about LES in their discussion section (Page 16, L 5-8) is very premature and too generalizing. The only thing one can conclude about the comparison of the LES results and the measurements reported in this paper (in its current form) is: the type of LES used in this paper and the methodology used in this paper to combine LES and NWP and the way LES is set up in this paper and the way LES was run in this paper and the way the results (both LES and measurements) were post-processed in this paper, resulted in the comparison reported in this paper. In fact, the accuracy of LES in prediction of wind-turbine wakes has been well tested and proved to be satisfactory in the literature. I think, with respect to these results, the authors should explain and explore what has gone wrong or what the inherent limitations are, rather than simply stating We find however no proof that the LES can improve the quality of the comparison... or ...LES is not able to push these values closer to the measured values. Here, another question comes to ones mind: have the authors tested and validated their LES for flow through wind turbines against more controlled and classical wind-tunnel experiments (which exists in the literature)? (if yes, please mention the reference in the text) It is very important that authors first make sure their LES model combined with their actuator disk model works well, and only then they can aim to test the accuracy of LES in such a complicated case that is described in this paper. In other words, the steps should be taken one by one.

Authors' comment: *Thanks for this helpful comment. Before answering in more detail, we would like to clarify that the objective of the paper is to introduce and test the methodology of LES driven by mesoscale model input for wind turbine wake modelling. In the scope of this manuscript we further compare full scale measurements of wind turbine wakes to simulations conducted with the model chain. As mentioned by the referee later in the review, the challenge with LES is to use the right inflow and boundary conditions. The concept of the manuscript is to show that the presented way of using mesoscale model output can serve as a good approximation of these conditions.*

We agree with the referee that, in general, it has been shown that LES of wind turbine models are capable to reproduce wake characteristics. Full-sized wind turbines in the atmospheric *boundary layer (ABL), however, face a wide range of wind conditions that strongly influence the wake characteristics.Replicating the wind conditions for non-neutral conditions is challenging with idealized quasi-static LES, as the atmospheric boundary layer is never in a steady state. In our view, there is no consistent work yet that proposes how to derive the boundary conditions for LES of this wide range of measured wind conditions.*

Regarding the accuracy of the LES in more controlled conditions, we compared the thrust and power curves of the simulated wind turbine with the confidential curves given by the constructor in idealized LES runs. For the modelled wind speeds both curves are almost equal.

Considering the phrase cited by the referee, that "We find however no proof that the LES can improve the quality of the comparison...": This phrase is only related to the mean ambient wind field created by the LES in comparison to the numerical weather prediction model. Here we find no proof, that the mean wind field and measures of the mean wind profile from the LES are closer to the measured wind field. We will add that the study only looks at a very short time horizon and only near-neutral stability conditions and is thus not very representative. For more discussion of the topic we delegate the reader to the authors' response on comment 1 in the answer to referee RC1.

Comment 2

Regarding the inflow boundary condition the authors have written: ... we made simulations without the turbine in parallel and simulated only intervals of 30 min maximum with wind turbine with a 3 min precursor phase for the development of the wake. It is clear that the authors have used a precursor simulation to generate the inflow; however, it is very vague how they have done it. Please state very clearly how you have performed your precursory simulation and how you have linked your precursory simulation to your main simulation. Have you used a buffer/fringe zone to overcome the periodic boundary condition? How have implemented it? In validation of LES results against measurements, it is well known that, an appropriate inflow is crucial. For example, one can consider this quote from Andreas Kempf: In general, LES inflow-conditions involve far more detail than those for a RANS, and if the LES is not provided with this data, the results cannot be expected to be superior to RANS results.

Authors' comment: *The description of the procedure we use might be a bit too complicated in the manuscript. We will add a more detailed step by step explanation of the procedure. What we do is to write out binary data of the wind fields from the simulations without turbines at the time steps at which we want to start the turbine simulations. These data serve as our base of precursor runs. The turbine simulations are then run with the same boundary conditions as the simulations without turbine, just with the added body forces of the wind turbine. Due to the cyclic horizontal boundary conditions, the changes to the wind field from the turbine wake continue to propagate inside the domain. We benefit from the wind direction that is never directly along one of the main axis of the model during the turbine simulations. The wake thus never interacts with the turbine already during the first flow through the model. As there is still a change to the boundary layer turbulence induced by the wake, the turbine simulations are restricted to 30 min. A buffer/fringe zone might overcome this problem, however the continuously changing boundary conditions make the implementation of this zone a complicated task.*

Regarding the quote by Prof Kempf we refer to the answer on the previous comment.

Comment 3

In your LES formulation (Eq. 1), please indicate the term responsible for the SGS stresses. Is kinematic viscosity of momentum the turbulent viscosity?

Authors' comment: *For simplicity this equation represent the Navier-Stokes equation of momentum before any approximation. For the final set of equations the Boussinesq-approximation and Reynolds-averaging are applied. The terms that represents the sub-gridscale fluxes has its origin in term 1 of the shown equation. In the final equations the molecular diffusivity term with the kinematic viscosity is actually neglected, because it is several orders of magnitude smaller compared to the turbulent viscosity. For the SGS terms a 1.5 order closure after [1] is used. For more information about the model we like to refer to [2].*

Comment 4

In Page 10, Lines 5-10, you have discussed about the choice of the relaxation time. You have finally chosen tau=4 h. You have first mentioned that the errors with respect to FINO1 measurements are not a good criterion to assess the value of tau, and then you have said that the choice of tau was a qualitative decision. Please substantiate the choice of tau. Here it seems tau=1.5 h is a better choice based on the errors, and with tau=48 h LES boundary layer develops more independently. So what was your criterion/criteria for choosing tau=4 h?

Authors' comment: *Here we also follow the recommendation of Schalkwijk et al.[3] and Heinze et al.[4] as it is not straightforward to conclude from the comparison between the mean wind field in the simulation and the measurements, which setup is the best. In the manuscript we chose the differences in wind speed and wind direction near hub height as measures for the error. The sources of errors can come from multiple sources, e.g. from a difference in magnitude and phase of the mesoscale model, that is not able to model the scenario, from the higher fluctuations of the mean values in the measurements or from the inertia of the flow in the LES domain that prevents the model to follow the changing background conditions. A lower relaxation time only improves the last aspect. On the other hand it forces the LES profile towards the mesoscale profile, which is not necessarily correct. If the LES has more freedom to develop it might improve the representation of the wind profile. While this might not be the case in the present study, we showed this recently in a proceedings paper [5].*

Comment 5 & Comment 6

Define exactly and mathematically the wake width and wake deficit. Explain how you have calculated these quantities both for lidar data and for LES data.

The correspondence of Fig. 12 and Fig. 9 seem questionable. For example, based on Fig. 12 the width of the wake measured by lidar in the evening period has doubled from y=200 m to y=500 m; however, one cannot see such an increase in Fig. 9. Moreover, in the night period, based on Fig. 12 we have a quick 0.5D jump in the wake width between y=300 and 400 (for the lidar data); this jump is again not clear in Fig. 9. It seems to me a bit of inconsistency.

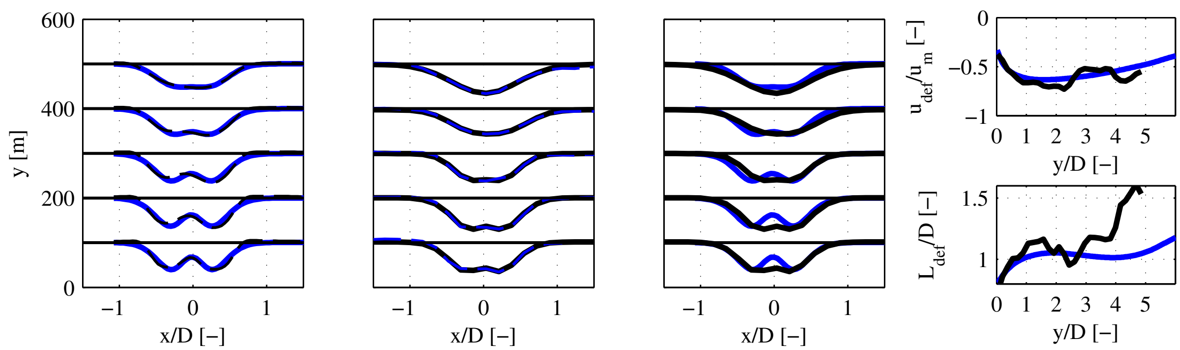

Figure 1: Wind field at 1:30 UTC. (a) LES wake profiles (dashed) and fits to the profiles. (b) Lidar wake profiles (dashed) and fits. (c) Fitted profiles from LES (blue) and Lidar (black). (d) Normalized deficit and wake width from LES (blue) and Lidar (black).

Furthermore, the differences in the wake width in Fig. 9 (between LES and lidar) does not seem as large as in Fig. 12.

Authors' comment: *We use a Gaussian fit like in [6]. We describe the parameters in p.15 L.17: "The fit amplitude is defined as the wake deficit and the 90th percentile width of the fit as the wake width." In reaction to the review we modified the equation to include the near wake. The fitting function now consists of two overlayed Gaussian-like functions and is given by Eq. 1.*

$$u_{def}(x) = b\ \exp\left(-\left(\frac{x-c}{d}\right)^2\right) - e\, b\ \exp\left(-\left(\frac{x-c}{f\, d}\right)^2\right) \tag{1}$$

With this function, the area of lower deficit in the near wake is represented by the second exponential function, with e,f $\in [0,1]$. The wake width (90th percentile) we get from this fit is $L_{def} = \sqrt{2}\cdot 1.64 \cdot d$, as deficit we chose the minimum value of each fitted u_{def}. In the revised manuscript we will modify the figures about wake decay and width and add figures like Fig.1 to show how the parameters were derived. The fit to the measured data still delivers quite noisy trajectories, which we discussed in the Discussion section of the manuscript.

Comment 7

In Fig. 10, the difference between panel (a) and (b) is not clear. It is not mentioned in the caption, and it is not even discussed in the text.

Authors' comment: *The difference is simply that panel (b) shows the deficit normalized by the ambient flow field as in Fig. 9. We will add a description.*

Comment 8

Please indicate in Fig. 1 the position of the met mast FINO1. In the same figure, please indicate the North direction.

Authors' comment: *Will be added.*

Comment 9: Editorial comments

Some editorial comments:

- Make a distinction between RMSE symbol used in Table 1 and the RMS of fluctuations used in the rest of the paper (e.g. Fig. 7, 8, etc.).
- Define all the abbreviations the first time you use them in the text. For example: DEWI, PPI, PALM.
- u_g in Eq. 1 has the LS subscript, but in Eq. 3 does not. Are they the same variables? If yes, use a consistent notation for both.
- Page 5, Line 10: I think you need a comma after Compared to onshore
- Page 2, Line 3: are should be changed to is.

Authors' comment: *We will correct or edit the manuscript based on the comments*

References

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[3] Schalkwijk J, Jonker H J J, Siebesma A P and Bosveld F C 2015 *Monthly Weather Review* **143** 828–844 (*Preprint* `http://dx.doi.org/10.1175/MWR-D-14-00293.1`)

[4] Heinze R, Moseley C, Böske L N, Muppa S, Maurer V, Raasch S and Stevens B 2016 *Atmospheric Chemistry and Physics Discussions* **2016** 1–37

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[6] Vollmer L, Steinfeld G, Heinemann D and Kühn M 2016 *Wind Energy Science* **1** 129–141