1 Response to WES2017-16 RC1

The authors thank the referee for the helpful review. We tried to identify the main requests to the manuscript from the text and address them in the following.

General comments

This paper addresses the incorporation of mesoscale model output into a large-eddy simulation (LES) using cyclic (periodic) lateral boundary conditions. The LES wind speed, wind direction, and potential temperature fields are guided toward corresponding mesoscale simulated values, via a combination of advections, geostrophic wind forcing, and relaxation, allowing the LES to track some of the low-frequency variability captured within the mesoscale solution. Comparison of simulated and observed meteorological parameters indicates generally good agreement between the mesoscale and LES simulated fields, as well as between the simulations and observations, on timescales of a few hours or greater. The emphasis then switches to comparing simulated turbine wakes, modeled using an actuator disk with rotation, within an LES forced with mesoscale input, to lidar-observed wakes from an offshore wind farm. The simulations are shown to capture observed bulk wake properties reasonably well in the aggregate, with success quantified mostly using best fit parameters to a Gaussian wake model. The examination demonstrates both the successes and the limitations of the proposed simulation framework; the LES closely follows the mesoscale wind speed and direction profiles, as well as changes of temperature, however is not capable of improving bulk vertical wind shear beyond the mesoscale simulations, and fails to reproduce some observed wake properties. While starting with a promising and interesting premise, the value of the study is compromised by both some methodological shortcomings, and, as importantly, by Discussion and Conclusions sections that fail to engage interesting components of the study, instead proceeding with an unsubstantiated overstatement of the utility of LES, before transitioning into a desultory presentation of the general difficulties of high-fidelity simulation, and comparison of simulations and observations. These could be the most informative and illuminating sections of the paper, especially when a new and well motivated technique is examined, such as is the case here. Instead, the paper ends up feeling unfinished, with much potentially useful discussion omitted.

Authors' comment: *The authors will revise the Discussion and Conclusion sections to focus on the aspects mentioned in the review. Regarding the methodological shortcomings mentioned by the referee, we could not identify which shortcomings were ment by reading the review. We however assume that this comment is related to the requested additional analysis in the following comments and a more profound discussion of the model chain ability to reproduce the ambient wind and the wake.*

Comment 1: Evaluation of the representation of the ambient wind field

A key objection is the assertion that LES does not improve representation of the ambient wind field beyond a mesoscale model, based upon the observation that the LES is not able to push simulated parameter values from the mesoscale simulated values closer to the observations. First, there is an insufficient basis from this small study to generalize about the ability of LES to improve upon a mesoscale prediction. Further, any such improvement depends upon the desired quantity. While wind speed and direction are certainly crucial, turbulence quantities are also important, influencing power production, stress loading and wake evolution, for example. If done correctly, LES can provide good representations of these characteristics. However, the expectation of the LES to push certain variables closer to observed values than as represented within the mesoscale simulation is a bit misplaced, especially given that i) the LES herein was forced toward those mesoscale values and ii) the nearly steady and homogeneous conditions simulated herein are precisely the conditions for which mesoscale turbulence parameterizations would be expected to function quite well. A more reasonable expectation, in my view, would be that the LES could resolve the classical turbulence spectrum consistent with the slowly varying flow component as simulated by the mesoscale model. I would be interested to know how well the LES met this more reasonable expectation. So, how did the LES conducted herein perform in that respect? Such was not a central inquiry of the present study, however some hints were provided. These limited results lead me to question if the LES conducted herein were somehow deleteriously impacted by the incorporation of the mesoscale forcing. Evidence for this hypothesis includes i) the much lower magnitude of the ten-minute simulated variability, relative to that which was observed, shown in Fig. 7, ii) the smaller variances shown in Table 1, especially under the influence of the mesoscale forcingA Tnote how much larger the variances are when u-advection is ignored, and iii) the absence of variability in the background simulated flow, as well as symmetry of the wake structure, relative to the observations, shown in Fig. 9. These questions can be answered via more substantial assessment of the LES flow field, which is my key recommendation. At a minimum, some comparison of simulated and observed spectra and stresses should be carried out if possible, and if not, at least spectra and/or stress profiles from the simulations should be presented and compared with the results of other studies. Only after establishing that the LES is capturing the classical energy spectrum well can assessment of its applicability be undertaken.

P11, Sentence beginning on Line 2: I do not agree that the ten-minute variability is well reproduced by the models. The model parameter values appear to exhibit significantly less variability than the data.

Authors' comment: *The referee rightfully states that the present study does not present a sufficient basis to generalize about the ability of LES to improve upon a mesoscale prediction. The key recommendation is to perform a more substantial assessment of the ambient flow conditions with a focus on the resolved turbulence frequencies.*

To address this recommendation we propose to include the following aspects in the revised manuscript. Firstly, a comparison of the observed and simulated spectra for the period of the two simulated days (Fig. 1). As shown in the figure the LES model shows the typical stronger falloff in the high frequency domain, related to the cut-off of the highest frequencies by the implicit filtering of sub-grid frequencies. As stated in the manuscript, the variance on the 10 min scale is nearly preserved. At lower frequencies the LES spectra first falls off, which is not the case for the measurement. This gap in the time period ranges from about 0.5 to 12 h. To be able to reproduce the frequencies in this range, the horizontal extension of the LES model has to be much larger at least [1]. The spectra from the mesoscale model is not shown in Fig. 1, because we use only hourly resolved data. However, Vincent et al.[2] i.a. have shown that mesoscale models are not able to resolve most of the measured fluctuations in the multiple hour range. For the discussion of the general ability of the model chain to model the

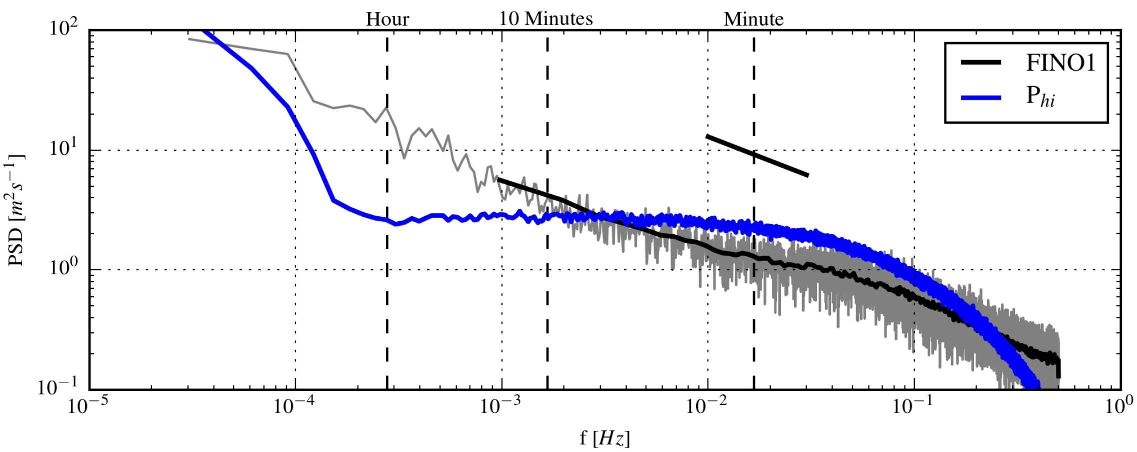

Figure 1: Power Spectral density from the LES (blue) and from the 1 Hz cup measurements at 90 m at FINO1 with different window sizes over the two day period. 2^{15} s (grey). 2^{10} s (black). Black isolated line depicts the slope from Kolmogorov cascade.

turbulence spectra we will include the findings of [1, 3] in the discussion section and compare the approach with the nesting approach discussed in Munoz-Esparza et al.[4].

Considering the above-mentioned, we still find no proof in this study that the modelling of the mean wind profile is improved by the downscaling with LES. Of course, LES introduces turbulence quantities that are crucial for the wake development, which is the main motivation of the research presented in this paper. The improvement of mesoscale models by using LES on the other hand is not a motivation for this specific paper and we will at least weaken the statement in the revised manuscript.

Comment 2: A more comprehensive examination of the wakes

Following that, I think a more comprehensive examination of the wakes would also strengthen the paper. While the formal quantitative comparison is restricted to the portions of the wake for which the Gaussian model can function, other aspects of the wakes (far wake, meander, etc.) could be discussed at least qualitatively. These examinations could lead to a much more illuminating discussion of both the promise and the difficulties regarding the application of mesoscale information into quasi-idealized LES with cyclic boundary conditions, an interesting and timely topic that deserves careful examination. This paper represents a good first step in that direction that, with some polishing, could be a very useful contribution to the literature.

I think more discussion/analysis of the wake widening would be helpful. Is the simulation perhaps not capturing some interesting physical process, such as maybe hub vortex shedding, which leads to widening/meandering of the near wake?

P13, Fig. 9: Seems to be much more variability in observed than LES background. Perhaps this is important in wake spreading? Also, how about showing more of the far wake regions? Even if analysis is restricted to 3-5 D due to the wake recognition algorithm, it would be nice to see how the far wake widens and dissipates in the simulations relative to the observations.

Authors' comment: *We agree with the referee that a examination of other aspects of the wakes, e.g. far wake, meandering would be a very interesting topic. In this manuscript we are, however, restricted to the available measurements, which were performed with the focus on measuring the mean wake profile at hub height. Range and sampling rate of the measurements are too low to study far wake or wake meandering, respectively. We expect that the presented methodology allows to do such a comparison with measurements in changing atmospheric stability conditions, that are believed to be the main reason for different observed meandering behaviour. We suggest to add a paragraph on this topic in the discussion.*

Comment 3: Table 1

P9, Table 1: I am not able to understand this table. First, why would statistics of the measurements (F1) be different for different model configurations (rows)? More explanation would help clarify.

Authors' comment: *We will focus a bit more on describing the table. What is shown in the columns and termed σX is not the changing statistics of the measurements but the statistics of the difference between model result and measurement that change for every model setup. As the notation might be a bit missleading, we will exchange it.*

Comment 4: Momentum advection

Second, why were sigma wd and sigma ws so much larger when momentum advection was turned off? This is potentially important. It seems this might be doing something significant within the LES. I think looking at spectra, for example, could provide some insight.

Authors' comment: *Without momentum advection, the inertia of the flow is too high to follow the trend given by the mesoscale model. The nudging term is already added to overcome this inertia, but momentum advection appears to further decrease the difference to the input trend and the measurements. Figure 2 shows that the mean wind speed without momentum advection is much higher on the second day between 12 and 18 UTC. Here the flow is still reacting on the sudden increase of the geostrophic wind speed (large scale pressure gradient) around 6 UTC (see Fig.5d), naturally resulting in an oscillation of the flow in the domain. The sink of momentum by the momentum advection terms as shown in Fig. 5h dampens this oscillation. We will add this figure for clarification.*

Minor comments

P1, L14: replace first occurrence of of with to. P1, L15: which has been established. . . P1, L17: Sentence beginning on this line. Please describe briefly some of the errors and why those are so large. P1, L20: Due to the generally lower. . . frequently more persistent. . . P1, L22: Stable conditions are not unique to offshore environments; onshore sites typically feature stronger static stability due to more rapid nighttime cooling over land than water. P1, L25: Please add simplified or Fast running to the sentence beginning on this line, as there is a wide

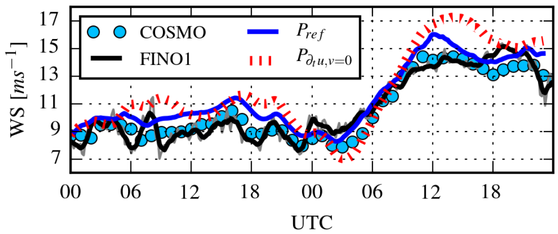

Figure 2: Comparison of 70 m wind speed from different model setups of PALM, the mesoscale model and from the measurements at FINO1.

range of engineering models, some of which are very high fidelity and therefore too slow to be used in the described capacity. P2, L5: please remove exemplary. P2, L8: please remove permanent. P2, L13: replace fair with meaningful. P2, L19: replace a lot of with many. P2, L25: replace us to include with for inclusion of. P2, L34: replace wind turbine with actuator. P3, L20: Either include enough detail about precisely what is meant by enough and a lot of so that another researcher may duplicate your data processing methodology. P3, Eq. 1. Since turbulence closure is an important aspect of LES, please describe the approach utilized herein. P6, L11: data is are averaged. P6, Eq. 3: Please define f3. P7, L1: density and pressure pressure and density, respectively. P7, L10: are close to agree well with. P10, Fig. 7 caption: Is the black line the hourly average, and the gray line the ten-minute average? Also, the caption claims that the power law coefficient is defined in the text but I could not find that. P11, L23: What are the constant values of the drag coefficients used for the nacelle and tower? P15, L1: Please replace a lot with something more specific. P15, L10. Any speculation on why the thrust coefficient so much lower in the operating lidar than in the simulation, if I am understanding correctly? P15, L14: Space between 9 and (. P15, L18: Please explain why you think the LES has these biases? Higher deficit in the morning, morning, lower other times. P15, L20: despite being operated in . . .

Authors' comment: *We will address the remarks in the revised manuscript.*

References

[1] 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`)

[2] Vincent C, Larsén X, Larsen S and Sørensen P 2013 *Boundary-Layer Meteorology* **2** 297–318

[3] 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

[4] Muñoz-Esparza D, Kosovic B, Mirocha J and van Beeck J 2014 *Boundary-Layer Meteorol.* **153** 409–440