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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Transient LES of an offshore wind turbine" *by* Lukas Vollmer et al.

Anonymous Referee #1

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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

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Discussion paper

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.

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 forcingâĂŤnote 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.

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.

Below are a handful of minor corrections and additional suggestions:

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 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. 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. 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. P10, Fig. 7 caption: Is the black line the hourly average, and the gray line the ten-minute

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average? Also, the caption claims that the power law coefficient is defined in the text but I could not find that. 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. P11, L23: What are the constant values of the drag coefficients used for the nacelle and tower? 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. 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 . . . End of page 15: 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?

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