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Interactive comment on "Large-Eddy Simulation Sensitivities to Variations of Configuration and Forcing Parameters in Canonical Boundary-Layer Flows for Wind Energy Applications" *by* Jeffrey D. Mirocha et al.

Anonymous Referee #1

Received and published: 8 November 2017

General comments:

The paper analyses the sensitivity of large-eddy simulations to various model configuration and physical forcing parameters. Simulations results of three different LES models are compared among each other as well as to mast measurements. The objective is to prove that LES are a valid tool to provide flow parameters relevant to wind energy applications.

Although LES as a tool for assessing wind energy relevant flow parameters is not novel by itself – at least in the scientific community – the intercomparison of several LES models and typical model configurations is a valuable contribution to the community. Even more so as the simulation results are being validated with observations the lack of which is very often the weak point of such studies.

The paper is in general well organized and clearly written. I can recommend it for publication after some minor revisions.

Specific comments:

Section 1: Aren't there any similar studies (comparison of different LES models and/or model configurations for atmospheric flows) in the available literature? If there aren't, state that to strengthen your paper. If there are any somehow similar studies, mention them.

Section 2.1: Why did you not investigate a stable case? Is there any particular reason for it? Because evidently, stable regimes are very important for wind energy.

Section 2.1: Elaborate a bit more on the measurement data used to validate the simulation results. It reads as if the SWiFT tower is only 50 m high, but I guess it is 200 m high (if not: where does the data above 50 m come from?).

Section 2.1: How did you estimate the geostrophic wind speed from the wind profiler measurements? Which additional uncertainty is introduced by this method?

Page 9, lines 19 ff.: The simulation results are evaluated when the average flow velocity has reached its first maximum. Hence, the simulation is not steady at that point, it will take much longer time until the inertial oscillation has been damped completely. Nevertheless, your approach is valid as such long simulations are not feasible and the important point is that all the simulations are compared at the same state of evolution. You could just clarify this a bit more.

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Page 10, line 9: How did you estimate the “representative” values of HS? From literature, previous experience or measurements?

Section 2.6, Tables 1 and 2: Please be clearer on which parameters come directly from measurements and which are just estimated or set arbitrarily.

Tables 1 and 2: Units are missing!

Figure 3: It appears as if the boundary layer height is considerably different in the three simulations (ca. 1600-1800 m in WRF, 1400-1600 m in SOWFA, 1200-1400 m in HiGrad). Can you comment on that?

Page 20, line 7: Something is missing here, I think you mean RMSE and MAE at the tower (see Tables 3, 4). And furthermore: What does “Tower MAE” or “Tower RMSE” mean? At the (imaginary) turbine tower or at the met mast? In specific heights or averaged over all heights? Looking at page 26, I find the answer, but that should be explained here, too and in more detail.

Page 20, last sentence: That is not exactly correct, as Table 3 shows that the rotor MAE is considerably higher for the higher-order advection scheme.

Page 22, line 13: “The WRF model shows the greatest sensitivity to these parameters with HiGrad showing the least.” I don’t agree: From Fig. 9 it looks as if WRF (bottom left) shows the least variability whereas SOWFA shows the greatest (middle left).

Fig. 10: Add in the caption that these are the runs with higher resolution. Otherwise, this figure cannot be distinguished from previous figures.

Page 24: The subsection is called “Sensitivity to model grid resolution” but actually only the higher resolution runs are compared among each other. The comparison to the lower resolution runs is missing.

Page 26, lines 9 ff.: The TKE discrepancy is quite huge, even if compared with the unwaked case. Can you cite any studies from the literature that provide estimates of tower wake effects on TKE? To my experience the effect should be large but not factor 3.

Section 3.2.2: WRF is reproducing the measurements amazingly perfect including the variability. Have you investigated a second convective case? Otherwise you cannot be sure if this is just a fluke or if WRF is generally that well suited for convective conditions. Can you comment on this?

Page 32, line 16: What was the result of the sensitivity test towards the surface heat flux? Does it confirm the guess that H_S was possibly too large in the simulations?

The summary/conclusions section is quite short. Which settings turned out to be the best (or better than others)? Can you give any recommendations?

Page 36, lines 1-5: These are indeed the next necessary steps. In these potential follow-up studies you should – in my view – strongly separate physical forcing and numerical parameters. Even in this manuscript they are from my point of view too much intertwined. The paper could benefit from some more separation between forcing and numerical parameters.

Technical corrections:

Page 4, line 5: “focuses” –> “focus”

Section 2: All the subsections need to be renumbered (1.1 –> 2.1 etc.)

Page 9, line18: “Sect. 1.3” –> “Sect. 2.3”

Page 13, line 1: “plan” –> “planar” or “plane” and “x-z” –> “x-y”

Page 13, line 7: “used grid resolution” –> “used a grid resolution”

Page 17, line 16: “Sect. 1.6” –> “Sect. 2.6”

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Page 22, line 9: U_g is not 9 m/s but 6.5 m/s in the neutral case (according to Table 1).

Page 24, line 7: U_g is not 9 m/s but 6.5 m/s in the neutral case (according to Table 1).

Page 26, line 22: “August 04 case” –> “August 17 case”

Fig. 16: Switch the two upper plots to make it consistent to Fig. 11 (or switch there).

Figures in general: Try to improve the readability of the figures by increasing font size, make lines slightly thicker, show only relevant parts etc. The dash patterns are sometimes very similar so that the curves can hardly be distinguished on a b/w print.

Fig. 11: Introduce different dash patterns to allow distinguishing the curves on a b/w print.

Page 31, line 12: insert “a” between “over” and “two”

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