Synopsis:

The manuscript “Model sensitivity across scales: a case study of simulating an offshore low-level jet” by the authors P. Hawbecker, W. Lassman, T. W. Juliano, B. Kosovic and S. E. Haupt deals with the investigation of the sensitivity of mesoscale-to-microscale simulations of a specific case of an offshore low-level jet (LLJ) to varying sea surface temperature (SST) datasets. The authors aim to understand how the representation of physical characteristics of the observed LLJ changes between mesoscale and microscale simulations. Moreover, the authors investigate the question whether a better mesoscale setup leads also an improved result of the microscale simulation coupled to this mesoscale simulation.
The study shows for the specific case investigated that low-level shear and jet nose height are better represented in microscale simulations compared to mesoscale simulations. Specifically, low-level shear improves by reducing near-surface wind speeds and lowering the jet nose height to be closer to observations. The study highlights the challenges in predicting the mesoscale setup that will result in the best performance of the microscale model. While the best performing mesoscale setup (CMC SST data) ends up being the best performing microscale setup, the second-best performing mesoscale setup becomes the worst performing microscale setup. This suggests that the differences between mesoscale and microscale numerical methods and model setup are large enough that one of the best performers on the mesoscale may lead to one of the worst performances of the microscale model.

While the paper is overall well written and addresses with mesoscale-microscale coupling a hot topic in the field of wind energy research, my main concern refers to the question how generalizable the results presented by the authors actually are. I think it would strengthen the manuscript a lot if the authors could show that their conclusions from the single low-level jet case at one site can be transferred to other low-level jet cases at the same site and a next step also to low-level jet cases at another site. If no additional sites or measurements are included in the manuscript it should be mentioned more clearly that the reader should be careful to transfer the findings of this paper to other sites.

I have the following specific comments on the paper:

1. Line 64: Please change “shear over the rotor swept” to “shear over the rotor swept area”.
2. Line 91: Please change “Initial and boundary conditions for model” to “Initial and boundary conditions for the model”.
3. Line 91: I ask the authors to provide a motivation for using the MERRA-2 data for providing the initial and boundary conditions. E.g., it would be an important information if previous papers have shown that this reanalysis dataset performs best in the region under investigation by the authors.
4. Line 115: I ask the authors to add additional information on the correction of measurement data for tidal variation.
5. Figure 2: I found it slightly difficult to determine the height of the maximum wind speed from this plot (is it really always at about 120 m as stated by the authors in the text?). I think it would be helpful for the reader if the authors added markers showing the position of the maximum at each time to this figure.
6. Line 130: Is a resolution of 10 m for the simulation of a stably stratified marine atmospheric boundary layer that shows an LLJ sufficient for an LES? I think it would be good to slightly lower expectations already at this point.

7. Line 151: I ask the authors to please clarify whether the output that is produced every 10 minutes is instantaneous or time-averaged data.
8. Line 163: Please replace “closer” by “closure”.
9. Line 178/179: The authors mention several times that it is computationally expensive to run the LES simulations. My suggestion is to provide information on the resources that were actually consumed for the simulations by the authors. How many core-h on what type of HPC infrastructure have been consumed?
10. Line 188/189: In recent years a number of criteria for detecting low-level jets have been suggested in literature. I think the authors should refer to these criteria.
11. Line 199: From my point of view the observation of a too strong shear in the microscale simulations of the authors was to be expected. The chosen resolution is too coarse to really be an LES of a stable atmospheric boundary layer. I expect that this leads to a too low turbulence in the model. Thus, the shear becomes too large.
12. Figure 5: Please add markers that provide information on the core height also in this figure.
13. Line 257/258: “The mesoscale domains befit from slightly under-predicting wind speeds below hub height and over-predicting wind speeds above” Doesn’t this sentence contradict the result that the shear in the mesoscale model is low? (see e.g. figure 9) In Line 260 the authors state: “Analysing the time series of the ensemble mean of bias in low-level shear the mesoscale domains underpredict low-level shear while LES over-predict.” This sounds contradictory to the sentence in line 257/258 to me.
14. Line 268: “It is interesting to note that the mesoscale domains produce larger negative values of SHFX than the LES domains, which indicates more stable conditions”. Couldn’t this be checked by checking the profile of potential temperature? Another possible explanation is that the eddy viscosity could be overestimated by the mesoscale model. This would fit to the wind profiles having less shear.