Referee comment on "Self-nested large-eddy simulations in PALM Model System v21.10 for offshore wind prediction under different atmospheric stability conditions"

This article presents a comparison of self-nested large-eddy simulations using PALM with the field measurements from FINO1 offshore met mast. The topic of localized mesh refinement to reduce the computational expense, while resolving smaller turbulent scales is of interest to the scientific community. Therefore, evaluating existing modeling frameworks for such applications is important and falls within the scope of the journal.

Overall, the authors have done a decent job in evaluating the model and the results are presented in a clear manner. I do have some comments, which I believe should be addressed before the article is considered for publication:

Major Comments:

1. The primary motivation behind self-nested LES (as also stated by the authors) is to resolve more turbulent scales. The finest grid resolution is chosen to be 1.25 m in the current study. What is this choice of grid resolution based on? As discussed by the authors on page 9 line 180, the chosen resolution does not fully resolve the inertial sub-range, which is understandable but then the question is what additional benefit could you get by partially resolving the inertial sub-range if you cannot reach the furthest smaller scales (like the dissipation scales), compared to only resolving the largest energy containing scales?
2. The authors choose a height of 119 m for comparison and call it as the 'hub height'. In practice, the hub height depends on a specific turbine model, and as the simulations are performed without a turbine, why is this height chosen for the comparison and referred to as the 'hub height'? As seen later in the manuscript, the turbulence quantities are compared at lower heights due to the measurement dataset, it is slightly confusing to see a particular height chosen as hub height, but then later not used for key comparisons.
3. The FINO1 data is available at the heights of 40, 60 and 80 m, and the authors extrapolate mean wind speed using logarithmic law and assume a constant variance. The accuracy of these approaches should be demonstrated in the article. Especially, how accurate is the assumption of a constant variance from 80 m upwards?
4. On page 6 line 130, the authors state that for onshore sites a power law exponent is 1/7. A reference to this figure should be provided, as a power law exponent can change much like the roughness length based on the land cover in onshore conditions.
5. On page 11 line 194-195, the authors hypothesize that the difference in the spectra between the measurements and simulations in the SBL case is due to the boundary layer height being below 119 m. As per my understanding, the comparison between simulated and measured spectra is done at 80 m, or is it that the measured spectra at 80 m are compared with simulated ones at 119 m? If so, how accurate is this comparison? Additionally, it is true that the SBL heights can be significantly lower than

the NBL and CBL ones, but are they actually below 80 m? A reference to back this should be provided, or this hypothesis should be revised.

6. On page 14 lines 223-224, the authors associate the difference between the simulated and measured power law exponents to the coarse measurement resolution. In order to back this up, the plots of simulated, simulation fitted power law, measured and measurement fitted power law should be shown (at least for a few simulations).
7. The last line of the conclusion (page 16 line 251) seems rather vague. What do the authors recommend? Either do only neutral simulations or do one-way nesting for stable and convective conditions? Do they recommend not using two-way nesting in any scenario?
8. A comparison of parameterized/unresolved turbulent scales could be added to understand how the unresolved scales change due to the nesting strategy.

Minor Comments:

Some minor technical comments are given here:

1. The abbreviations should be defined on first use. For example, the abbreviation of large-eddy simulation (LES) should be defined on page 1 line 16.
2. Figure 2 is not referenced in the text.
3. Probably missing ‘child’ in ‘parent and domains’ on page 8 line 167.
4. Probably missing ‘with’ between ‘along other’ on page 15 line 240.