Response to the comments

Maria Krutova

March 2023

Thank you for the detailed reviews and valuable comments. We will account the requests on clarifying the technical details and LES inputs. The current response contains up-to-date data, but some values may change if the final revision uses updated simulations.

Reply to the reviewer comments 1 (RC1)

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?**

   Initially, we aimed to reproduce the study by Nybø et al. (2020) in PALM. Nybø et al. (2020) was using SOWFA code for LES fields generation. We planned a cross-comparison between PALM, SOWFA and sonic anemometers data. Therefore, most of the domain parameters originate from SOWFA study to keep the simulations comparable to each other and measurements. The issues with the two-way nesting under strong wind shear were interesting enough to report in a separate paper.

   While the two-way nesting effects could be noticed with only one child domain, defining 2−3 child domains allows more detailed comparison between one-way and two-way nesting. E.g., the spectra comparison shows that more scales can be resolved in the nested area of the coarsest domain if the two-way nesting is applied. At the same time, we also showed that refining the grid spacing below 5 m does not improve the accuracy enough to justify additional computational costs. Hence, further refinement is not justified for the free-flow areas, unless a specific grid spacing is required.

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.

We chose the height of 119 m after Nybø et al. (2020) as an outlook into larger, 10 MW, wind turbines. Hence, all the measurement time series were selected by Nybø et al. (2020) to have similar free-flow wind speeds at the specific height, and we could re-use already processed time series. We agree that relying on these pre-defined characteristics may be confusing. For this study, we rename the 'hub height' of 119 m to the 'reference height' where the free-flow wind speeds are similar across the regarded cases.

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?**

   After a discussion, we conclude that we cannot reliably estimate the turbulence intensity at 119 m with the available data. Since we are not performing a time series comparison for 119 m, we decided to remove the estimation of turbulence intensity value at 119 m. Instead, Sections 2 and 3.2 will be re-worded to describe a comparison to the turbulence intensity calculated at 40−80 m. The current LES setup produces turbulence intensity profiles within a 10% relative (∼0.6% absolute value) deviation from the measurements, which we take as acceptable, considering that PALM does not allow specifying exact turbulence intensity profiles via simple initialization.

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.**

   This value was provided as an example to give a sense of the exponent's order. As was commented in the following line, that the value is not a universal constant:

   Often, the approximations do not reflect seasonal and diurnal variations in the wind profiles (Bratton and Womeldorf (2011); Jung and Schindler (2021)).

   We add a reference to Touma (1977), which expresses the same concern on using 1/7 exponent and studies different stability 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?**

   The comparison is performed for 80 m time series.

   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 rev

   We reviewed the existing studies on low SBL and unnatural profiles, particularly, Muñoz-Esparza et al. (2012) and Kettle (2014), and examined 10-minute averaged profiles for the same period measured with the cup anemometer. The cup anemometer measurements reach up to 100 m, hence, we cannot define the SBL height exactly. After the review, we agree that the hypothesis should be retracted, as we do not see anomalies in the measured stable profiles below 100 m, which could be implying an unusual behavior of SBL.

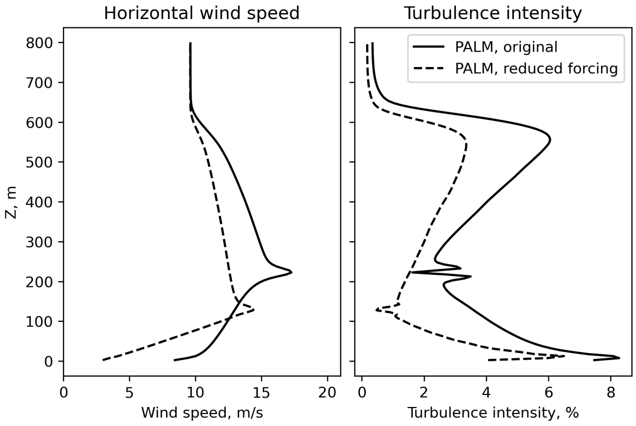


Figure 1: Comparison of SBL precursor velocity profiles, original and reduced forcing run

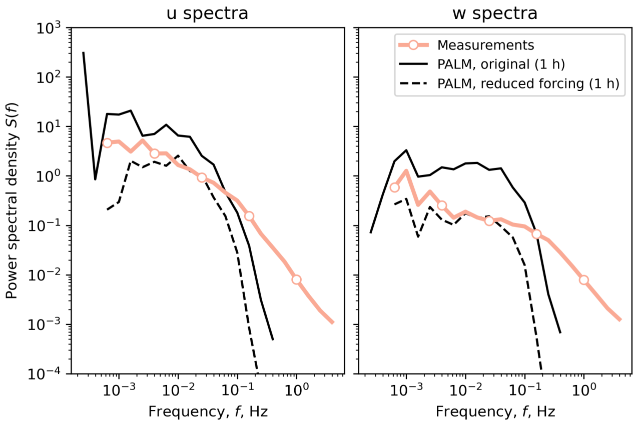


Figure 2: Comparison of SBL precursor spectra, original and reduced forcing run

We performed a test simulation of an SBL precursor for the same wind speed but weaker pressure gradient (-0.0001 Pa/m instead of -0.0005 Pa/m) and slightly stronger surface cooling (-0.3 K/s instead of -0.2 K/s). Due to decreased forcing, the developed profiles deviated from the reference measurements and showed stronger shear but lower turbulence intensity (Fig. 1). However, we observed a better agreement with the measurements spectra (Fig. 2). Therefore, we are able to match only one of two: either SBL profiles or SBL spectra – and observe a strong discrepancy in another. We conclude that the cause of discrepancy could lie in the way PALM handles stable conditions.

To our knowledge, there are no throughout studies on SBL turbulence characteristics performed with up-to-date PALM code. Early PALM model (listed under code IMUK) participated in a cross-comparison of LES of SBL in Beare et al. (2006). While a comparison to observations was performed, it regarded only momentum and heat diffusivity and fluxes. Wurps et al. (2020) regards different grid resolutions and Ning et al. (2021)

performs a comparison to SOWFA, both do not use measurements for verification and validation. Hence, simulating stable conditions in PALM with the respect to turbulence characteristics may require verification a study.

It should be noted that this conclusion refers only to a simple initialization via velocity and temperature profiles and surface cooling. Other ways of simulating SBL in PALM via dynamic driver from WRF data or synthetic turbulence behave differently.

We are currently examining, whether opting for matching SBL spectra instead of matching profiles can benefit this study.

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).**

   The line

   The discrepancy between exact values of α *in measurement and simulated fits could be explained by less precise power law fit in the measurement profiles: only three points were available for the fit.*

   was referring only to the low of levels in the sonics data. Fitting power law to cup anemometer profiles with more levels produces more accurate results, we will revise this subsection based on the new comparison.

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?**

   The line

   We, therefore, recommend using either true neutral simulation or one-way nesting for the wind turbine wake simulation.

   is changed too

   We, therefore, recommend using one-way nesting for the wind turbine wake simulation. In the case when two-way nesting is preferable, only a true neutral setup does not produce secondary circulation.

8. **A comparison of parameterized/unresolved turbulent scales could be added to understand how the unresolved scales change due to the nesting strategy.**

   Figures 3 and 4 present plots of resolved and subgrid scales. Subgrid scales can be only output in PALM as spatially averaged profiles, hence we also the the resolved scales are as the spatial averages from the same output.

   Nesting mode has a small effect on resolved scales in general. The effect is most pronounced in CBL simulations 3b, when comparing $\overline{wv}$ quantities: the resolved scale profiles in child domains are located slightly tighter in the case of two-way nesting than in one-way.

   The subgrid scales consistently remain near zero for all levels except few near-surface cells where the turbulence intensity remains high 4. Consequently, the near-surface subgrid scales are comparable to resolved scales. Yet, the subgrid scales at lower levels tend to zero as the grid resolution is refined. The resolved scales in two-way nested run show non-monotonic behavior for the first point above the surface. Given that the resolved scales are spatial averages, the behavior may be attributed to near-surface flow difference in the nested and non-nested area.

Minor comments

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.**

   Corrected

2. **Figure 2 is not referenced in the text.**

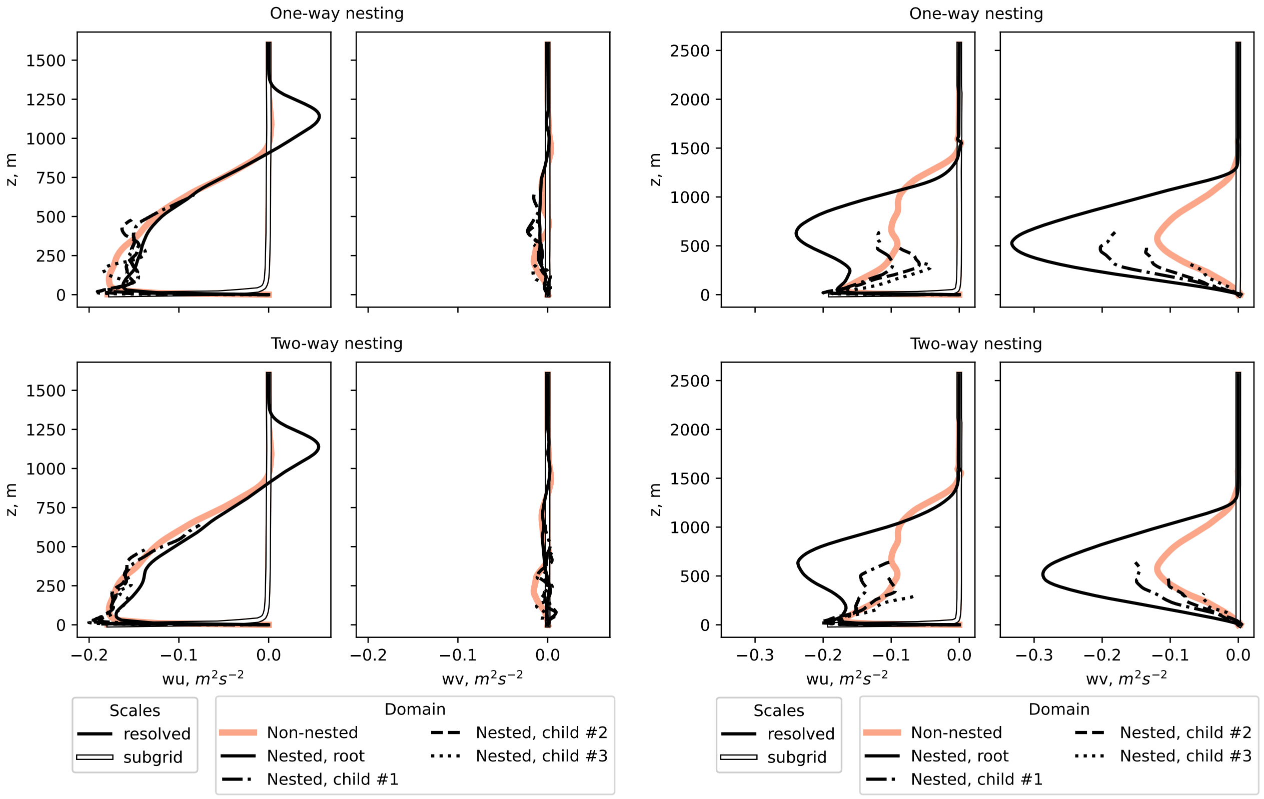
   The reference is added to line 159:

   In the absence of the surface heat fluxes, i.e., in the true neutral case, the one-way and the two-way nested simulations behave similarly with the respect to grid spacing and feedback between domains (Fig. 2).

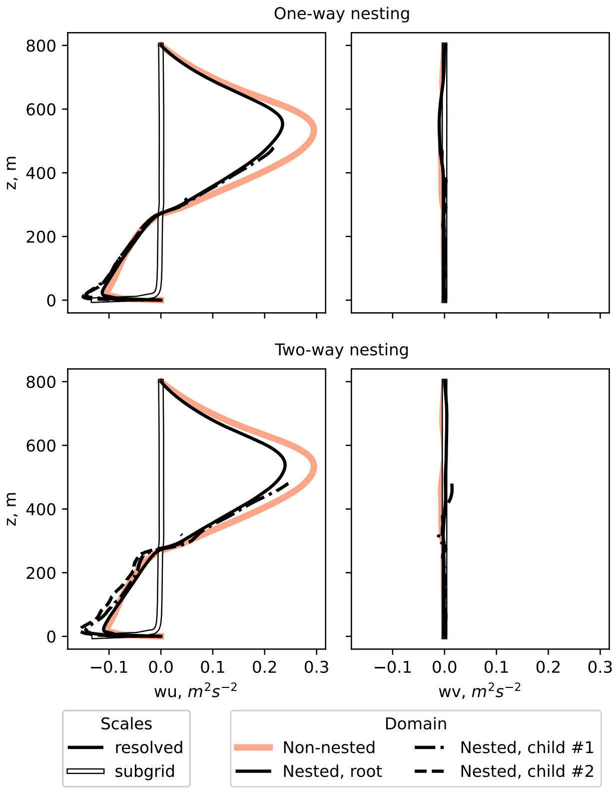
3. **Probably missing ‘child’ in ‘parent and domains’ on page 8 line 167.**

4. **Probably missing ‘with’ between ‘along other’ on page 15 line 240.**

   The typos are corrected.

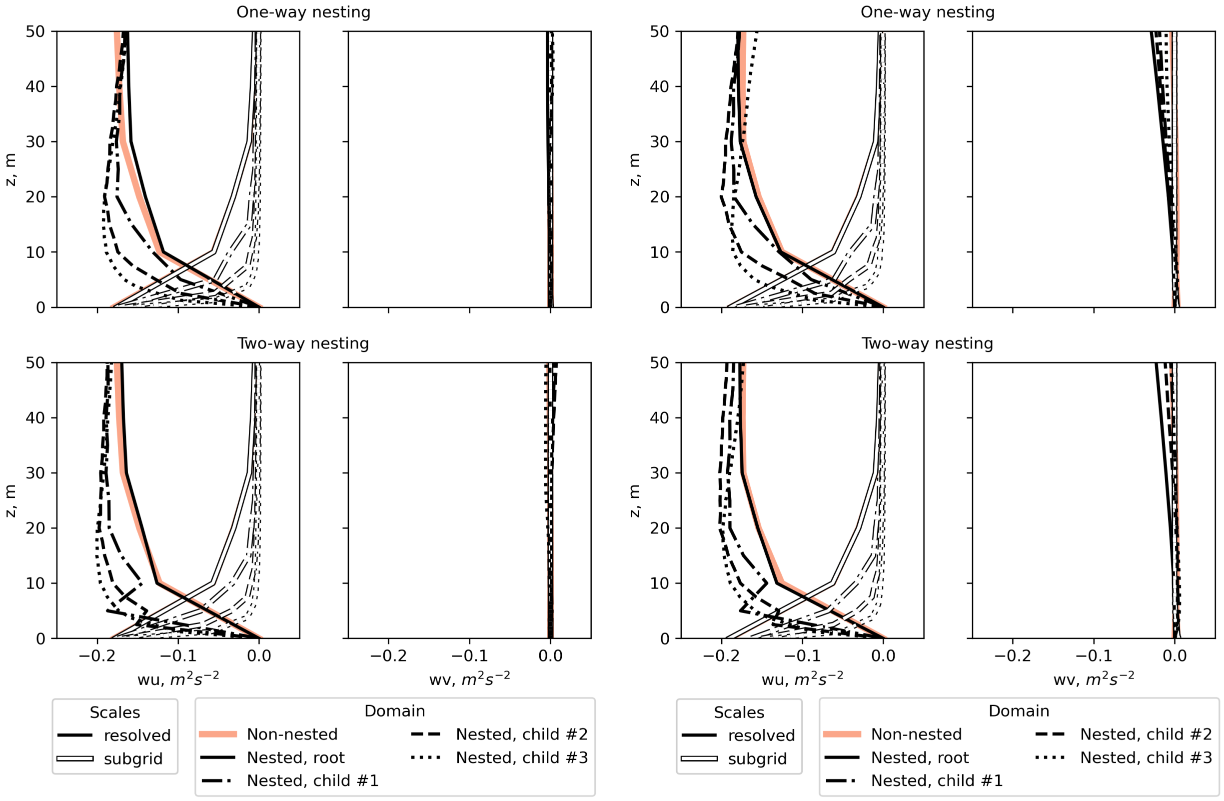


(a) NBL resolved and subgrid scales (b) CBL resolved and subgrid scales

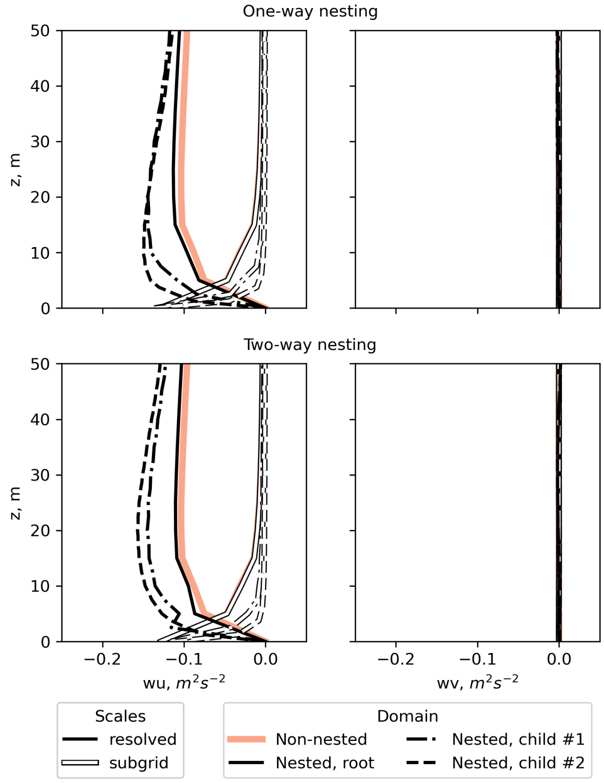


(c) SBL resolved and subgrid scales

Figure 3: Comparison of resolved and subgrid scales for different stability simulations and nesting modes



(a) NBL resolved and subgrid scales (b) CBL resolved and subgrid scales



(c) SBL resolved and subgrid scales

Figure 4: Comparison of near-surface resolved and subgrid scales for different stability simulations and nesting modes

Reply to the reviewer comments 2 (RC2)

Major comments

1. **Line 40: "... at the hub height of 119 m" From the paper it does not get clear to me why 119 m are decribed as hub height. According to the information provided on `https://www.alpha-ventus.de/english` the hub heights in the wind farm alpha ventus that is situated next to the FINO1 met mast are 90 and 92 m, respectively. Moreover, I do not understand why not one of the real measurement heights is applied. Doesn't the introduction of the additional height to which measured data is extrapolated mean an additional source of uncertainty?**

   The estimated wind speed at 119 m serves to unify three different stability cases by the mean wind speed at the same level. No direct comparisons are performed at 119 m except of matching LES velocity profiles with the measurements. We will detail this purpose more strongly in the Data section and emphasize the available measurement levels in other sections.

   Later in the paper it is written that the boundary layer height at the times of the measurements under stable stratification might be lower than 119 m. The equation in line 45 is however only applicable in the surface layer, i.e., in the lowest 10 percent of the boundary layer. From my point of view this arises doubts whether the derived 119 m data can actually be used for comparisons in the case of the stable boundary layer.

   We do not possess measurements above 100 m for the regarded period of SBL conditions. Therefore, the actual height of SBL layer cannot be estimated reliably. The hypothesis on unusually low SBL was suggested to explain strong discrepancy between LES and measurement spectra. We performed a test simulation with matching SBL spectra but not profiles (Fig. 1 and 2). Currently, we are leaning to a conclusion, that SBL in PALM requires a verification study with the focus on turbulence characteristics.

2. **Figure 1: It seems as if the domain of the precursor run has exactly half the width of the domain of the main run in the neutral case. This setup is not ideal in order to break up long streaky structures. An excerpt from the PALM documentation reads: "Note that the initial flow field has a perfectly regular structure with a periodicity of the precursor run. This regularity can persist for a very long time. To break up this regularity, use a domain width that is not an integer multiple of the precursor run domain width. The occurring flow field discontinuity at the lateral domain boundary initiates a fast break up of the regularity."** (`https://palm.muk.uni-hannover.de/trac/wiki/doc/app/examples/turbinf`). **Figure 2 shows indeed elongated structures in the u- and the w-component that seem to extend over the whole length of the model domain. Are these structures realistic or only a result of the chosen setup? What does it mean for the comparability between simulations and measurements?**

   The regularity can be broken by using parameter `y_shift` in the main run to ensure the breaking of the regular structures. This is not a precursor-specific parameter, and it is also recommended for the main run to ensure breaking of any possible regularity left in the precursor.

The structures forming in NBL and CBL runs are generally larger than in SBL due to higher turbulence intensity. Hence, the SBL run would look more uniform in the same color scale. The stripes forming in NBL and CBL averaged flows are not stationary and change over time.

3. **Line 74: Does the nesting procedure also apply linear interpolation of the wind speed components in the vertical direction close to the ground? Wouldn't it make more sense to apply a logarithmic interpolation here? A linear interpolation should actually result in an underestimation of the resulting wind speed, shouldn't it?**

   Yes, the interpolation is linear and is performed similarly for all quantities and levels. The interpolation is performed only for the nested boundaries, consisting of a child domain's side and top surfaces. The bottom surface is always located at $z = 0\,\mathrm{m}$; PALM does not have an implementation of a free-hanging child domain. Hence, the bottom surface uses boundary conditions (Neumann or Dirichlet) prescribed in each child input parameter. Then the near-surface quantities are obtained when the prognostic equations are solved for a child domain.

   Indeed, it would be interesting to check, whether introducing a logarithmic near-surface interpolation affects the nesting results. However, that requires modifying existing PALM code and for now falls outside the scope of initial study.

Points 4−7 refer to the same concept of defining vertical profiles in PALM. We see these questions were raised due to an insufficient explanation of the precursor-main run approach in the article. The explanation was shortened initially to avoid focusing on the PALM general concept that is not directly related to nesting. Seeing that it causes confusion, we expand Methodology section on the precursor.

The precursor-main run approach is chosen to ensure that nested and non-nested runs are initialized with exactly the same flow field. Simulating a precursor run in a smaller domain also saves computational time on generating steady turbulent inflow with required vertical profiles. Depending on the stability condition, the precursor may need to run for 2−6 simulated days before the flow becomes stationary. The simulated flow is then used as an inflow for the main run, which does not have to be simulated for the same amount of time to study the nesting effects.

We add clarifications to Section 3.2 and expand Table 4 with the column 'Time' to reflect the simulation time required to reach a steady state in the precursor (Table 1). We also add another table to provide the characteristics of the flow in the steady state, i.e., inflow characteristics of the main run: velocity at 119 m, capping inversion, and surface temperature (Table 2).

A detailed answer to each point is provided below.

4. **Line 101: How can there be a geostrophic wind without Coriolis force?**

   This is a collision of notations. The input parameters `ug_surface` and `vg_surface` are used in PALM to describe geostrophic wind near the surface and provide the initial state

Table 1: Input parameters of the precursor runs (Updated Table 4 from the original manuscript).

	$\overline{U}_0$, ms^{-1}	dp/dx, Pa m^{-1}	z_0, m	T_s, K	$\overline{w'\theta'}$, K ms^{-1}	dT_s/dt, Ks^{-1}	Run time, h
NBL (coarse)	13.8	-2×10^{-4}	1.2×10^{-3}	300.0	0	–	27
NBL (fine)	14.1	-2×10^{-4}	1.7×10^{-3}	300.0	0	–	48
CBL	11.5	-1×10^{-4}	5×10^{-4}	281.3	0.015	–	146
SBL	13.0	-5×10^{-4}	8×10^{-4}	289.5	–	-0.2	72

Table 2: Steady state of the precursor runs – turbulent inflow for the main run (New table).

	$\overline{U}_{119}$, ms^{-1}	TI, %	T_s, K	L, m	Capping inversion, K/100 m
NBL (coarse)	12.1	6.03	300	10^6	0
NBL (fine)	12.9	6.94	300	10^6	0
CBL	12.0	6.27	294.7	-342	7.4
SBL	12.7	3.78	291	529	9

for the model. The initial profile is then considered a constant value along z-axis at $t = 0$ s. The initial profile changes due to the influence of external forces until the steady state is reached. When the Coriolis force is absent, the initial profile can still be prescribed with `ug_surface` and `vg_surface`. However, a pressure gradient should be added to compensate for the surface friction losses in the absence of the Coriolis force. The reason we chose the pressure gradient approach instead of the Coriolis force is explained in reply to point 7.

To avoid confusion, the text in line 101

the geostrophic mean wind $\overline{U}$

is replaced by

the initial mean wind $\overline{U}_0$

5. **Line 101: The simulations are run without a capping inversion. This is a difference to the situation in the real atmosphere. What does it mean for the comparability between the simulations and the measurements?**

   The prescribed temperature gradient serves only as an initial temperature profile. During the precursor run, the profile gets altered due to a heat flux (CBL case) or surface cooling (SBL case), resulting in a capping inversion. A true neutral simulation does not develop a capping inversion due to the temperature remaining constant during the simulation.

   We list a capping inversion in the new table describing the steady state in precursor runs used as the turbulence inflow for the main run (Table 2 in the response).

6. **Line 106: "We run main simulations for one hour" Is this sufficient to get a stationary solution? For that the inflow should not change with time and the flow should at least flow once through the model domain. I assume that the simulation time used is actually to short. What was the averaging period used? From my point of view it does not make sense to start with the averaging period directly at the beginning of the large-eddy simulation.**

   We use the precursor-main run approach. The long precursor run generates a steady flow field with the required wind speed and turbulence profiles. In this run, the vertical profiles are affected by the pressure gradient, surface friction, and heat flux.

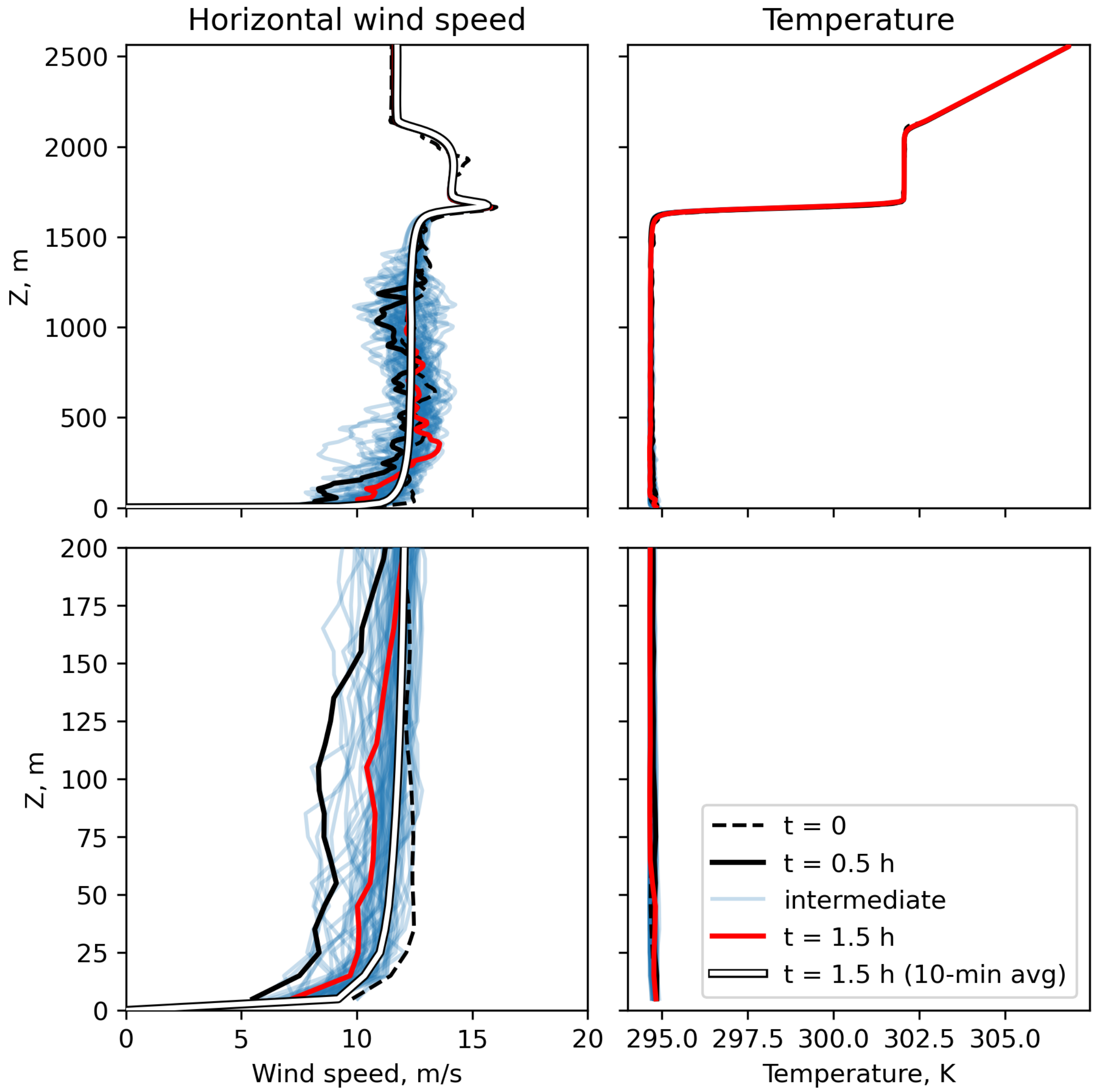


Figure 5: Example profiles from CBL two-way nested run, root domain. The intermediate profiles are plotted with a step of 60 s in the period $0.5 - 1.5$ h from the start of the main run. The lower row shows the same profiles zoomed into the surface layer. Averaged velocity profile is provided for the comparison.

The main run copies the averaged stationary profiles from the last step of the precursor run (assumed to reach a steady state) to initialize the flow field in a larger domain. The turbulent inflow is then recycled over the main run domain. The averaged profiles generated by the precursor remain constant with random fluctuations added to them (Fig. 5). The fluctuations are rather strong in NBL and CBL runs due to naturally stronger turbulence. Yet, the instantaneous velocity profiles do not deviate far from the original inflow profile. The fluctuations in the temperature profile remains negligible in the main run.

We simulated an extended CBL two-way nested run, but did not observe any development

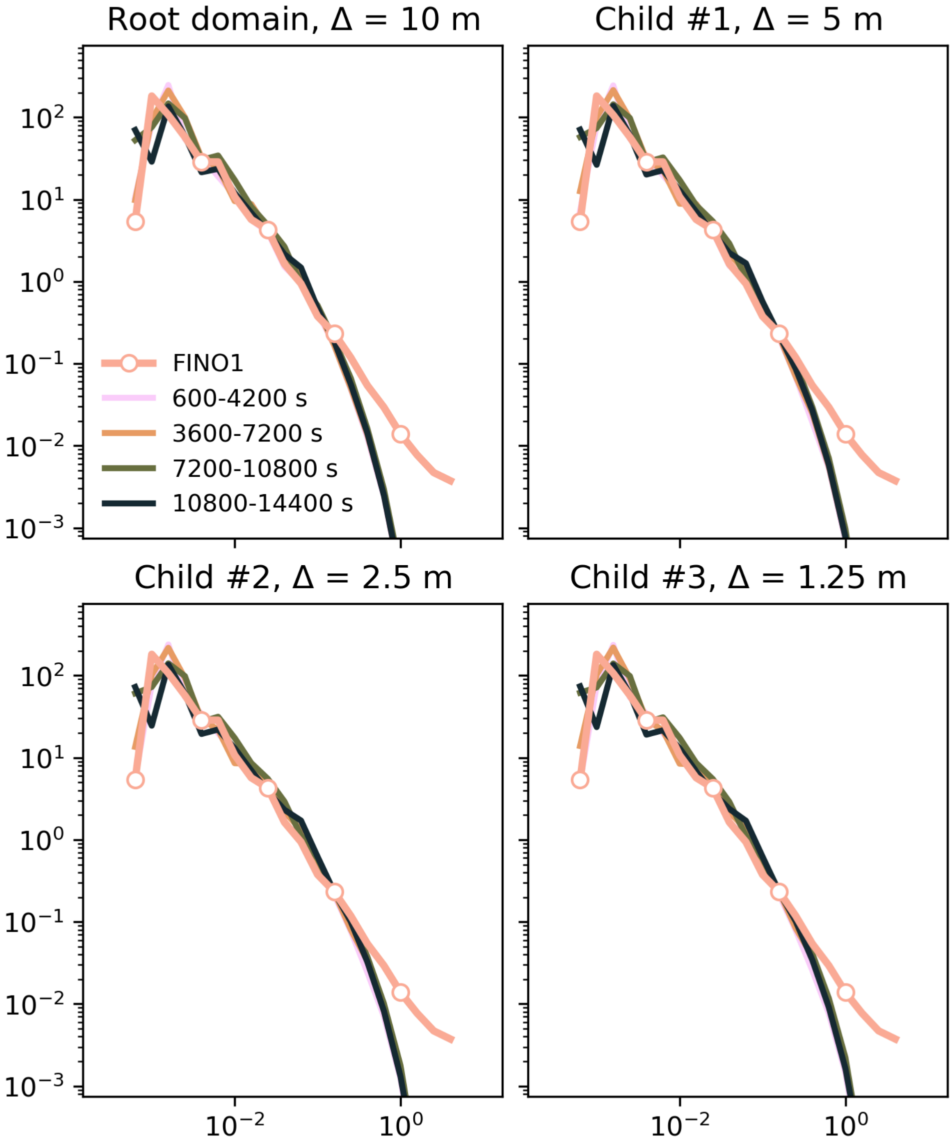


Figure 6: Example CBL spectra from 4-hour two-way nested simulation.

in the inertial subrange of the spectra. The fluctuations in the energy containing eddies are explained by the length of the time series – one hour to match the measurement time series. Indeed, Hellsten et al. (2021) ran their nested simulations for longer time. Yet, they either studied smaller velocities, i.e., require more time for the flow to pass the domain ones, or were interested in the flow development (cubes case, flow over hill). Our interest is primarily turbulence statistics which were less affected the extended simulations.

It was, however, an oversight on our part, as there is definitely a quick development happening in the beginning of the main run. To obtain one hour time series for the comparison, the original runs should have been extended by at least half-hour.

Since the time for revision allows this, we will extend the existing simulation to get new time series without any interference from the domain initialization.

7. **Line 173-176: It did not get clear to me why the Coriolis force had been omitted in the simulations. Moreover, I miss a discussion on how strong the impact of neglecting the Coriolis force actually is and what it means for the comparability of the measurements and the simulations. I recommend to add a thorough discussion of the topic to the manuscript.**

   The measurements time series were already rotated, so that the x-axis aligned with the series average wind direction.

   We needed to closely match the velocity and turbulence profiles of the measurement data. PALM does not possess the capability to define turbulence intensity profiles in a simple initialization. Instead, the profiles develop naturally during the simulation based on the input parameters. Several re-runs may be required to tune the parameters (initial velocity, pressure gradient, heat flux, and roughness length) until the desired profiles are reached in the steady state. Using the Coriolis force also requires accounting for the flow rotation and adjusting wind speed components to match the wind direction in the measurements.

   A non-neutral precursor run takes a considerable amount of time, e.g., 10 hours of real time, to simulate ∼6 days until the steady state of CBL. Depending on the initial guess, the precursor may require several attempts until the simulation produces velocity and turbulence profiles close to the measurement data. Considering this, we preferred a simpler approach by replacing the Coriolis force with the pressure gradient along the x-axis. This way, we do not have to consider the flow rotation, i.e., reduce the number of input parameters to be tuned. The main outcome of this decision is weaker fluctuations of v-components compared to the measurement series. We attempted to compensate for this by considering total horizontal velocity instead of regarding u and v-components separately in the measurement series.

Minor comments

1. **Language check: Please check carefully the use of articles again. From my point of view in the current version of the manuscript the article "the" is used in many places where it would usually not be used.**

   The manuscript will be checked again for the revised submission.

2. **According to the information provided in the manuscript a deviation between the mean wind speed in the measurements and in the simulations is obvious**

(see table 1 and table 4). How does this impact the meaningfulness of the results? It would be good to add a discussion on that topic. At least it should be stated that even the mean flow condition observed in the experimental data is not met by the mean flow in the simulations.

The input parameters passed to PALM do not define the steady flow but only serve to set the initial profile. Due to the surface friction, pressure gradient, and (if present) heat fluxes, the profile is altered over time in the precursor run. We simulate the precursor run until the velocity components reach a steady state.

To avoid confusion, we rename the column $\overline{U}, \mathrm{ms}^{-1}$ in Table 4 (see Table 1 in this response) to $\overline{U}_0, \mathrm{ms}^{-1}$ in order to show that this is not the final value. We also add value of $\overline{U}_{119}$ to provide the steady state velocity at 119 m (see an additional Table 2).

3. **Line 31: Please change "... are then compared between the model results and measurements ..." to "... are then compared with measurements"**

   The wording is corrected.

4. **Line 60: "Universität Hanover" –> Please change either to "Universität Hannover" (my suggestion) or "Hanover University" or "University of Hanover".**

   The typo is corrected to *Universität Hannover*

5. **Line 78: "In the two-way nesting case, each child domain interpolates its solution back to the respective parent domain" Wouldn't it make sense to include also an averaging process in bringing the fine-grid data to the coarse grid? It should be considered that PALM uses the volume-averaging approach.**

   PALM model description and documentation use a term 'anterpolation' for the process of returning a child's domain solution to its parent. Here, the anterpolation means first-order numerical integration of the child cells within each parent cell. Since it is not a common term in LES, we avoided using it, which lead to oversimplification and an erroneous description of the process. This will be corrected.

   PALM uses anterpolation scheme proposed by Clark and Farley (1984), the technical details behind the implementation are explained in Hellsten et al. (2021), Section 3.5.

6. **Line 83: "development and" –> "development"**

7. **Line 84: "reaches steady state" –> "reaches a steady state"**

   Typos are corrected

8. **Line 103: The SBL case uses surface cooling over time. Does it mean that the inflow changes with time? If so, wouldn't this create vertical movements? In case that indeed vertical movements are observed, are any measures taken to damp those movements?**

   The temperature changes only during the precursor phase. The turbulent inflow in the main run is not affected and is only subjected to random fluctuations.

9. **Line 111: I suggest using the notation $a(t, x1)$ $a(t, x2)$ instead of $a(t)$ and $b(t)$. a and b are introduced once as time series and once as points. The points however do not change with time.**

Since the specific locations are not mentioned (only the distance between points is important and not many levels are available for the comparison), and all expressions are presented with a focus on the time series, we correct the wording

points a *and* b

to

time series $a(t)$ *and* $b(t)$

10. **Line 117: Please change "synchronity" to "synchronicity".**

    The typo is corrected.

11. **Table 5: Please add an information on the grid spacing used in the CBL and in the SBL cases.**

    The table is re-formatted to add the 'Spacing' column.

12. **Figure 3: It seems to me as even in the case of the one-way nesting the flow field is developing between the inflow boundary and the outflow boundary. The values of the u-component close to the outflow boundary seem to be lower than the values close to the inflow boundary. Is this behavior expected?**

    This effect also appears in the non-nested simulation. The decrease is relatively small ($\approx 0.5\,\mathrm{m/s}$ or 1%) and grows with the domain length, but not the time. We are currently reviewing whether the effect is caused by the averaging, as it is not as pronounced in the instantaneous fields.

13. **Line 233: "We performed nested LES of three stability cases for the same wind mean wind speed of 12.5 ms-1". I do not see this statement be supported by what is reported in the paper. Neither the measurements nor the simulations show a mean wind speed of 12.5 ms-1. Please revise the statement.**

    We add Table 2 to show the parameters of the turbulence inflow and their difference from the precursor input parameters in Table 1. The wind speed at 119 m is not exactly $12.5\,\mathrm{ms}^{-1}$, but the variation between cases is lower than could be expected from Table 1, which lists precursor parameters.

    We also change the line from *We performed nested LES of three stability cases for the same wind mean wind speed of* $12.5\,\mathrm{ms}^{-1}$ to *We performed nested LES of three stability cases for the horizontal mean wind speed of* $12 - 13\,\mathrm{ms}^{-1}$ *at the reference height 119 m*

14. **Line 256 and 260: "Deutsches Windenergi Institut" –> "Deutsches Windenergie Institut"**

    The typo is corrected

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