Interactive comment on “Validation of the PALM model system 6.0 in real urban environment; case study of Prague-Dejvice, Czech Republic” by Jaroslav Resler et al.

Anonymous Referee #2

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I would like to commend the authors for this comprehensive study and the effort that went in this experimental campaign for model validation. The manuscript is interesting and useful. However, in my opinion there are several points that the authors should consider for improving the presentation of their results/comparison and, also, the reproducibility of their simulations.

First of all, we would like to thank very much the reviewer for this very valuable detailed review with a lot of helpful suggestions. We tried to carefully follow all comments and suggestions of both reviewers to improve the manuscript, making it better structured, more condensed and focused on the important findings, and more attractive for an “interested reader”. The details about our changes are provided in the responses to the individual comments.

Main comments

1) The authors correctly point out line 901-910 that the domain may be too small to develop proper turbulence conditions. Indeed, this is quite certain in my opinion based on my personal experience with 3D nesting in LES. They have an external grid of 4x4kmˆ2 and an internal grid of about 1.5x1.5kmˆ2. The outer grid resolution in the PALM model is 10meters while the internal grid is 2m. There are two issues: 1.25km is not sufficient to create an organized and realistic turbulent flow from the outer edges (where turbulence is synthetically generated) and the transition to the inner grid has a refinement factor of 5 and will take a lot before turbulence at smaller scales is generated. The authors seem aware of these issues. My point is that they do not try in any way to evaluate the turbulent state nor the relevance of the error induced by their simulation setting.

The authors write that within the urban canopy the turbulence will adjust faster due to the obstacles. This is true but did they try to quantify if they have developed turbulence? The authors should add and discuss the spectrum of turbulence and its evolution in space e.g. calculate it right before moving from coarse to fine grid (considering wind direction) and in some reference locations moving away from the leading edge of the fine grid towards the trailing edge. Ideally for a position relatively near the ground and one more elevated.

It would be also interesting to see vertical profiles of horizontally averaged statistics over the inner portion of the inner grid, this to see if we observe typical canopy layer, mixing layer and boundary layer profiles.

We agree with the reviewer's comment. The transition of the flow from a mesoscale model, where the turbulent transport is parametrized, to an LES model, where the relevant turbulence scales are resolved, requires a sufficiently large fetch length. The required fetch length depends strongly on the mean wind speed, boundary-layer depth and stability, and this is discussed in detail in a companion paper by Kadasch et al. (2020) in this special issue. The presence of obstacles and the orography also play a role as shown by Lee et al. (2018).
The same considerations are also valid for the LES-LES nesting where the turbulent flow undergoes a transition from the coarse to the fine grid. A detailed discussion about this can be found in a companion paper by Hellsten et al. (2020).
In order to strengthen the manuscript in this regard, we have decided to present a proper analysis of this. For this we re-run parts of the winter- and the summer-simulation at specific points in time in order to present results from different atmospheric conditions, i.e. for a neutrally-stratified to weakly stable situation represented by the winter case and convective conditions represented by the summer case. We have added a new subsection 4.1.3 "Spatial development of the urban boundary layer" where the adjustment of the flow in the parent domain is discussed in terms of the TKE at different heights. As expected, this analysis shows that the TKE has not been fully adjusted when the flow enters the child domain, especially at higher levels. We explicitly note that a larger outer coase-grid model domain would be desirable, and this would also account for mixing processes at higher levels. However, this analysis also shows that turbulence has already been developed and has similar strength compared to locations significantly further downstream, so that we do expect that the error made by this insufficient adjustment does not significantly affect simulation results on the street canyon scale.

With respect to the transition from the coarse-grid to the fine-grid domain, we now present frequency spectra at 50 m (mainly above the building roofs) evaluated at locations with different distances to the inflow boundary. Especially in the winter case the spectra indicate that a fetch length of a few hundreds of meters is required to avoid adjustment effects. In the summer case under convective conditions the flow transition is even faster and less than 50 m of fetch length are required, which is in agreement with findings presented in Hellsten et al. (2020) for buoyancy-driven boundary layers.

2) A general comment is that the paper is too long and some of the comparison do not really add new information. On the contrary some discrepancies in the model results with respect to the measurements are not discussed but just mentioned. Below some more detailed comments.

We are aware of this fact which partly follows from the quantity of the material but the problem is also caused by deficiencies in our presentation. This point about quantity is also discussed in RC 1 p. 2) and in our answer to that, To address this comment, we went through all the text and we tried to restructure and condense it and to remove all unnecessary parts and improve and strengthen description and discussion of the findings. Chapter 4 has undergone the most major restructuring, but also other parts have also been substantially improved. Detailed descriptions of the various restructurings are given in our answers to individual comments of both reviewers.

2.1) Figure 5, 6, and 7 are not necessary. Move it to the supplement and simply give a short description. Instead it is very important in my opinion to include WRF model output in figure 9, 10 and 11. In this way the reader can appreciate if there is any significant difference between PALM and WRF. Moreover, WRF simulation results should be included both for the location of the PALM profile and the location of the measured profile. In this way the reader can also evaluate how meaningful is to include the measured vertical profiles (that are for a location outside PALM coarse domain) in the comparison.

Thank you for this suggestion (compare also RC1 p.4)). To address this issue, we supplemented the graphs with the WRF profiles for both locations, the sounding location as well as the location of the modelling domain. Moreover, we unified the different sets of comparison graphs - sounding vs. WRF profiles (Fig. 6, 7) and sounding vs. PALM profiles (Fig. 9, 10, S13, S14, S15) - into one series of graphs (FIg. 5, 6, and S15, S16, S17 in the revised manuscript) which provides complex information about vertical profiles; we hope they give the reader valuable and clearly arranged information. We updated the text description accordingly.

2.2) The peaks in the vertical walls temperature, figure 13 (winter), figure 14(winter) should be discussed and it should also be discussed why the peak does not appear in figure 15. Are these peaks related to the radiative transfer model and its interaction with the wall at a particular sun angle under clear sky conditions? This may be an explanation of these peaks appearing at the same time of the day, and also the reason that they do not appear in summer. I noticed that in figure 18 a similar peak appears also in the observation (location 06_4_V). Did the specific wall geometry in relation to sun angle generate the peak? However, it is my opinion that these spikes in temperature and heat fluxes are very local surface effects with little or no influence on the overall heat exchange of the walls and even less on the atmospheric flow (see also point 3 below).

We added an analysis and discussion of the peaks in a new section 5.1.5 “Rapid changes of surface temperature”. The analysis examines the situation at location 11-1_V where these peaks are very strong and are not properly reflected in the observations but the discussion is easily applicable to any other location. The peaks are in fact the real reactions of the surface skin layer temperature to the strong “binary” changes of the direct radiation forcing due to changing of the shading of the sun rays during the course of the day. There are two reasons why these changes are more visible during the winter episode. Firstly, the radiation forcing by the direct incoming radiation to the vertical walls can be as strong or stronger in the winter as during the summer in the right conditions, and the remainder of the incoming radiative energy is lower which makes the corresponding changes of the surface temperature more “visible”. Next, the lower sun elevation angles causes the effects of the shading by other buildings or terrain to occur more frequently. Moreover, this effect is more pronounced on walls than on the ground as the thermal conductivity of the walls is usually lower than the conductivity of ground surfaces which means the ground heat flux is able to dampen the changes of the skin temperature according to the energy balance equation. The magnitude of the changes of the surface temperature at location 11-1_V are similar in the model as in the observations according to Fig. 20 while the precise shape slightly differs due to geometrical imperfections in the digital elevation model used, discretization effects, etc. More details are given in the discussion in the manuscript. A brief mention of this effect and a reference to this analysis were added to the general description of the surface temperature results (section 5.1.1).

2.3) Fig 11. The discrepancy in wind direction should be discussed in more details, and the WRF results should also be included both for wind speed and direction. I think that adding WRF wind direction will help in interpreting the results. Also, please make the shorter arrows longer (or simply make all the arrows of the same length since the magnitude is already reported in the lower panel). Please, do the same in the supplement.

As in the original version we considered the wind observation on FSv building as only supporting information and not part of the main focus of the study, we did not discuss it in detail (the focus of the study is the street canyon level comparison). To attempt to address this issue, we have dived deeper into the problem. At first, we tried to decide whether this effect (which appears only during winter episodes) is a technical problem or some microscale dynamic effect. We went through the entire process of measurement and data collection with the technicians, and we checked all our postprocessing tools. According to the technicians, the placement of the sensor and the settings of the equipment were identical for summer and winter episodes. However, we found no photo documentation of the sensor for the winter episode as exists in the summer photos. We thus were not able to conclusively check and prove the actual real orientation of the sensor in the winter study. We also tried to compare wind direction data from nearby synoptic stations as well as from WRF and PALM 3D fields. Despite all this effort, we came to no solid conclusions besides some speculations. As we cannot exclude the possibility of a technical issue (the wrong orientation of the sensor) during the winter episode, which would affect the wind direction, we decided to exclude the wind direction from the presented FSv wind observation and we updated the description. The potential missorientation of the sensor would not influence the observed wind speed so we consider this approach as the correct one. We consider this issue a very interesting and important problem and in new projects we are starting, we intend to repeat observations of similar type using an improved design.
To improve the presentation of the wind on FSv and its discussion, we first reworked the wind graphs themselves; the wind magnitude graphs are taller and the wind values from WRF and nearby synoptic station Ruzyne were added. We added statistics for this observation and we also improved the description of this part of the study to express clearly that we do not consider these observations as the observations of the free flow but as the observations of a flow influenced by surface effects, but at a higher level than the street canyon observations. To

emphasize this, we moved this part from section 4.2 to section 5.3.3 “Wind speed on the roof” in the revised version.

2.4) figure 13, 14, 15, 16, 17, 22, 23, 24. I think that the WRF surface temperature should be included in the comparison for the “horizontal“ surface. This should be just a single extra line for each location and allows to appreciate the difference/similarity with PALM, and this difference/similarity should also be discussed.

Thank you for this suggestion. We added WRF values to all horizontal surface graphs and we updated the corresponding text and discussion.

2.5) Why not using the heat flux measurement locations (Sinkule and Zelena) also in the discussion in section 4.3 (i.e. as typical urban locations). This may allow discarding some of the figures.

This is a natural idea. We do not use them as they do not cover the same time period and/or locations as the surface temperature observations: the heat flux observations at Sinkule started later, at the beginning of the second summer episode, due to delayed delivery of the instrument; observations in Zelena followed two weeks after that, and IR camera measurements were not taken at this location. Moreover, the HF observations require there to be no direct sunlight on the wall while the “interesting” cases of the surface temperature examination occur in interactions of the wall with the solar radiation of all kinds, including the direct radiation. For that reason, we decided to examine and present the surface temperature observations and the heat flux observations separately.
Nevertheless, we restructured all the chapter 4 and the “typical urban locations" are not presented anymore. Instead, location 11-1_V is used as the only complete example, and it is also utilized later in the discussion of the peaks.

3) The authors discuss the error induced by discretization (section 4.3.5) and also its possible solution (section 5.2. lines 873-884). I think that these discussions are partially misleading and must be modified.
I think that step-like discretization errors at this resolution can create local discrepancies (local in space and/or in time, see point 2.2 above) of limited overall significance; they seem significant only for the very local surface heat flux and temperature. Moreover, by increasing the resolution (e.g. 1m or less) the discretization errors become of the same order of the neglected SGS wall features. Therefore, there is no point in avoiding these errors if the building is not represented in all its many details.
Furthermore, it seems to me that the error, being mainly related to the radiative transfer model, can be fixed irrespective of the representation of the building in the fluid dynamic solver (i.e. I think that there is no need to have a more sophisticated immersed boundary method in the fluid solver to fix the behavior of the radiative transfer model since for the latter it is enough to keep track of the local actual surface angle).
Finally, I think that the influence of the current discretization method on the flow is

quite limited and local at the considered resolution, and become quickly negligible (and likely comparable with the neglected SGS wall elements) if further increasing the grid resolution.

We would like to thank the reviewer for raising this question which is indeed a tricky problem. We agree with the reviewer that the effect of step-like surfaces on the flow is mainly via the surface heat flux and thus in the radiation and the surface energy balance, while the direct effect on the flow is probably only of local nature; we added this to the text. However, to our knowledge there is no study available which directly compares the influence of step-like vs. sloped-surface representation on the flow. Due to step-like representation of the surface the surface area is artificially increased, meaning more surface friction, which may in turn also affect the mean flow above the urban boundary layer. But frankly speaking, we do not know for sure.
Also, we agree with the reviewer that the effects of the discretization on the radiation are most strongly manifested locally and their effects on larger scales partially mutually compensate. This fact is in our opinion clearly stated in the text, mainly in the last paragraph of the original section 4.3.5 (5.1.7 in the revised manuscript). As this study focuses mainly on the very local "point" comparison of the model and observations (one of its purposes was validation of the newly developed and improved modules RTM, USM, LSM), it is necessary to keep these effects in mind all the time, otherwise the conclusions could be wrong for point-to-point comparisons. The discussion of these effects inside this paper thus seems to be reasonable and it can be helpful for everybody who will attempt to use modelling results for assessment of local quantities.
Moreover, we are convinced that the global effects of the discretization cannot be neglected in some cases as the step-wise representation of the surface causes e.g. significant change of the surface area, roughness length, normal angles, mutual visibility, etc. To roughly estimate the level of the global effects of the discretization of a wall, we performed an idealized experiment which utilizes PALM's ability for grid rotation. We ran two simulations of the same idealized urban area with one west-east oriented street canyon. Both simulations modelled the same real geometry and properties of the street canyon, they had the same configurations with the exception that the first simulation was configured with no grid rotation while the other simulation had the grid rotated by 45°. We chose the configuration of the grid in a way that the effective width of the discretized street canyon stayed unchanged. The results for the south-facing wall were averaged over the central one third of the street canyon to avoid potential near boundary effects. The results show that the difference in the wall surface temperature can reach above 3 °C, over 100 Wm^{-2} in case of shortwave irradiance and about 80 Wm^{-2} in case of net radiation. These differences in the radiation fluxes and surface temperature consequently alter ground heat flux and turbulent sensible heat flux which can afterwards (in suitable conditions) alter the pattern of the flow inside the street canyon as we experienced in many of our simulations (one example is mentioned in our earlier GMD paper (https://doi.org/10.5194/gmd-10-3635-2017, chapter 3.4.1, fig. 16). We are convinced that this issue needs further research and better quantification.
To address this comment, we restructured section 4.3.5 (5.1.7 in the revised manuscript) as well as the corresponding part of section 6.2. We decreased the number of examples and we condensed the text. We tried to strengthen the formulations and reasoning to avoid any possible misinterpretations. We also added the estimate of the averaged effects of the discretization on

the wall radiation balance and surface temperature to the end of the section 4.3.5 (5.1.7 currently) together with supporting material in supplements. Please, reconsider this revised version.

4) line 845-847. The authors correctly point out that the PALM results are often very similar to the WRF results.
This is true for the close to surface air temperature, where WRF seems to have a better behavior in winter and a similar performance in summer compared to PALM. This should be discussed, and some statistical indices should be included comparing WRF and PALM performances.
On the contrary the wind speed is generally significantly different, and this is obviously because the buildings are simulated explicitly. The explicit building representation also allows a better agreement between PALM and the observation for wind speed with respect to WRF (within the urban canopy). This should also be discussed, and some statistical indices should be included comparing WRF and PALM performances.
In my opinion this behavior demonstrate that the role of a very fine scale heat surface description is minor compared to having the obstacle represented and can perhaps be not so important. For the correct modeling of the street level urban atmospheric flow It seems sufficient to fairly reproduce the overall integrated heat exchange of the building wall. Very local temperature and heat flux effects (those investigate here with point wise sensors) seems quite negligible for the simulation of the mean flow and mean pollutant dispersion at street level.

The behaviour of the WRF model simulations can be expected to be in accordance to e.g. Halenka et al (2019, https://www.inderscience.com/info/inarticle.php?artid=101840), in which it is shown that even mesoscale model with no urban parameterization, only what is referred to as “bulk representation” (only changing properties like albedo, emissivity, roughness, etc. in the surface parameterization), can simulate average temperatures with sufficient accuracy. For wind or PBL height it shows quite a poor performance. We added this statement and reference to the text.
However, in our study the focus is on the very local street canyon level features, not the mean flow or average temperatures. The purpose of these modeling studies is also to create a modeling system capable of providing very local information that can be used in urban studies such as UHI and AQ mitigation scenarios. Also, as we show in the companion paper (Belda et al. (2020), https://doi.org/10.5194/gmd-2020-126), testing sensitivity to parameter settings in the PALM model, the very fine scale heat surface description is essential as the local features can have unexpected results (e.g. increased albedo can lead to increase in surface temperature due to reflections from opposite surfaces). The example of the influence of the distribution of the surface heat flow in the street canyon is shown also in our earlier paper (Resler et.al. 2017, https://doi.org/10.5194/gmd-10-3635-2017) in Fig. 16 and 17.

5) The author should improve the reproducibility of their simulations. This is a GMD *manuscript and I think that all the necessary input files for performing the simulations should be provided for download, not just a generic reference to the open source model code. If I am not wrong for PALM these should be, the namelist file e.g. "prague_p3d", form both grid levels, the dynamic input file, e.g. "prague_dynamic", the static input file for both grid levels, e.g. "prague_static". The author should also include the file for the shortwave and long wave radiation that are obtained from WRF and used in the simulation, and briefly explain how they are generated.*

The complete configurations and input data of all PALM simulations were published in the ASEP repository (http://hdl.handle.net/11104/0315416, for information about the repository see https://asep-portal.lib.cas.cz/basic-information); this reference was added to the text and to the list of references in the manuscript.

5.1) The authors write that the details for obtaining the WRF (interpolated) wind field on the PALM grid goes beyond the scope of this manuscript (lines 3675-376). I think this detail should be well explained, also because they did not refer to any previously published full validation of this WRF-PALM coupling.

The 3-D fields from WRF outputs (T, Q, U/V/W) were horizontally and vertically interpolated (in that order) to the PALM model grid. Because the PALM model used a higher-resolution terrain that would differ from the coarse terrain in WRF by as much as tens of meters, the vertical interpolation had to include stretching of the atmospheric columns.

At the bottom, the atmospheric columns were shifted to match the PALM terrain, therefore there were no missing data below the original terrain and the surface effects from WRF were preserved. However, at higher altitudes, the atmospheric columns could not be shifted by the same amount, as that would introduce unrealistic horizontal gradients mimicking the terrain shift below. In order to avoid this, the atmospheric columns were stretched heterogeneously. The WRF model uses either sigma or hybrid vertical coordinates, our simulations use the hybrid option where the lowest level is terrain-following and the highest level is isobaric. For each column, the geopotential height of each level in the WRF data was recalculated using the same formula and parameters used in WRF for calculating the heights of the hybrid levels, however with the surface pressure altered to match the PALM terrain. The recalculated level heights were then used for linear vertical interpolation into the PALM Cartesian vertical coordinate system.

The interpolated 3-D fields were used as initial conditions for the first timestep and their top and lateral boundaries were used as boundary conditions for all timesteps. For the velocity fields, the total volumetric flux disbalance was calculated for each timestep as a sum of the volumetric inflow minus outflow for all boundaries. This residual volumetric flux was then divided by the total area of the five boundaries and subtracted from the respective inwards-directed velocity component for each boundary in order to make the inflow and outflow perfectly balanced, as is required by the incompressible equations used in PALM.

The Python code used for processing the WRF and CAMx data into the PALM dynamic driver file has been included into the official PALM distribution and published in the PALM SVN repository in the directory UTIL/WRF_interface since revision 4766.

To address the reviewer comment, we added a brief description of the transformation process and the reference to the transformation source code in the text of the chapter 3.3. of the manuscript. We also added a more detailed description of the transformation of WRF and CAMx data to PALM dynamic driver to Sect. S5 of the supplements with reference in the manuscript.

5.2) The coupling between WRF and PALM includes the specification of the horizontal pressure gradients in the two orthogonal directions at any level. The typical profiles applied for the simulations in summer and winter should be included in the supplement to the manuscript.

In the WRF-PALM coupling we do not consider pressure gradients explicitly. In the mesoscale nesting approach boundary values of the velocity components, potential temperature and mixing ratio are provided at discrete points in time, no horizontal pressure gradients are prescribed in this approach. This is because the horizontal pressure gradients are already implicitly considered in the solution of the Poisson equation for the perturbation pressure, which in turn includes the lateral boundary values for u and v. In other words, the pressure solver (to maintain incompressibility of the flow) creates the large-scale pressure gradient itself.
After several internal discussions within the PALM development group, we decided to completely omit large-scale pressure gradients in the mesoscale nesting approach, even though tests showed that it does not make a significant difference if it is considered or not. At this point we would like to refer to Kadasch et al. (2020, GMD discussion paper, https://doi.org/10.5194/gmd-2020-285) where the mesoscale nesting approach is presented in detail, including a discussion about the large-scale pressure forcing at the end of section 2.2.4.

5.3) It seems that buildings are not resolved explicitly in the outer PALM grid. What local parametrizations have been used to describe the urban canopy?

The buildings in the parent domain are explicitly resolved and they follow quite well the general structure of the buildings in the domain. The input data for the parent domain were prepared from the same DEM source (the Prague 3D model based on photogrammetric aerial mapping) as the child domain. The resolution of the parent domain is 10 m and thus all buildings with height higher than 5 m are explicitly resolved. This represents the absolute majority of the buildings in the domain. We thus did not utilize any additional parameterization except the standard roughness length utilized in LSM and USM via roughness length of individual surfaces. To address this comment, we provide two additional figures of the buildings in the parent domain (3D view of the domain and the grid footprint with the resolved buildings) in the supplements (S11 and S12) and a brief remark about explicit resolution of the buildings in the parent domain in Sect. 3.1.

Other comments

Page 3, I suggest removing lines 76-83 they seem redundant.

The text was not formulated well, but it gives important information about the focus of the study which influences its design. It was reformulated and condensed in the revised version.

Page 4, line 116. What does it mean (+-20m), about 20m?

Yes. This formulation should mean "about". The sentence was reformulated.

Figure 12. There is no color bar showing relations between temperature and colors.

The figure already contained the color bar in the right bottom corner but it was too small and poorly visible. We changed the format of this color bar (size, font size, shadowing of the fonts) to make it more readable.

Figure 32, 33, what does it mean "locations_V" or "location_Z"? I could not find the definition for the nomenclature "_V" "_Z".

We considered it only as part of the location indicator and we did not assign it to any nomenclature in the case of the locations of vehicle meteorological stations. In fact, this naming was assigned by the crew of the observation vehicles and it originates from Východ/Západ (East/West in Czech language). In order to make labels of the graphs more intuitive, we changed it to the full description (e.g. "Bubeneč house East").

Figure 34. The light green band is almost invisible.

The spread of the 10-minutes values is small for observations of the concentrations in many parts of the campaign and the area of this spread is almost hidden by the value line (in contrast with e.g. graphs of the wind). This also gives information in our opinion. To improve the visibility of this spread, we changed the colors of the graphs 32, 33, and 34 (26, 27, and 29 in revised manuscript). Moreover, we restructured these graphs in the way that graphs are wider to make lines better visible and we reduced graphs in the manuscript to episodes summer e1, summer e2, and winter e3; the complete graphs for all episodes and variables were moved to supplements to section S5 "Street canyon quantities".

Figure S13, S14, S15, add the vertical axis label

Corrected

Figure S16, caption. What is Fig supp13? should it be Figure S10?

Yes, thank you for noticing it. Fixed.