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Geoscientific Model Development Discussions

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.

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.

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.

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.

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).

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.

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.

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.

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.

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.

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.

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.

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.

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?

Other comments

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

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

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

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

Figure 34. The light green band is almost invisible.

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

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

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