Interactive comment on "A new urban surface model integrated in the large-eddy simulation model PALM" by Jaroslav Resler et al.
Anonymous Referee #2
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General comments

The authors present an addition to the well-known PALM Large Eddy Simulation (LES) model that addresses the effect of urban areas on local urban climate with a clear focus on the radiative temperatures of urban surfaces. In my opinion the approach that the authors seek is quite interesting and can be a valuable addition to detailed modeling efforts of radiative and turbulent transfer of heat and other quantities within the urban canopy. Unfortunately, the evaluation of the model against the available observations is quite poorly done or written down, and lacks a proper discussion of the strengths and weaknesses of the modeling approach. Furthermore, in my view a proper identification of mechanism that explain differences between modeled and observed temperatures is lacking. Also, I have the impression that the observational data set that the authors use to evaluate their model is too limited to fully appreciate the strength and weakness of the USM addition to the PALM model. Unfortunately, my advice to the editor will thus be to reject the paper in its current form.

We would like to thank the reviewer for the evaluation of our manuscript and useful comments that considerably helped to improve it. Both reviewers raised important points that led to extensive revision and also to the need to re-run the presented simulations which are therefore different from the previously submitted version. We have addressed all comments and our responses are stated below. Reviewer comments are in italics and authors responses are in blue standard font. Together with this response we uploaded a revised manuscript and supplements. We opted not to highlight changes as the changes are so extensive and whole sections are completely rewritten that it does not add any value and make document poorly readable.

Major points

Introduction

The section 1.2 lacks clarity in a number of places. Different processes with respect to urban climate are in my view not properly explained. Also, section 1.2 of the introduction lacks a comprehensive review of current approaches to calculate the effects of urban climate. It mentioned different approaches, but it remains unclear to the reader how this fits into the method that the authors develop. Why did the authors for instance choose to develop their own urban scheme and not to adapt a current scheme for standalone models for use in PALM?

The introduction was reworked to be more clear and comprehensive. We also tried briefly to indicate the reasons which lead as to the decision to develop a new model of urban canopy for PALM. As far as current approaches are concerned, we only mention the processes and techniques that are relevant to the present study and we are aware that this does not constitute a thorough review. However, thanks to the fact that a number of recent comprehensive reviews is available (e.g. Mirzaei&Haghighat, 2010 and Mirzaei, 2015, that we cite), therefore we wouldn't like to substitute them and we would rather concentrate on description of PALM-USM.

Section 2

The authors use the parameterization the PALM-LSM formulation for horizontal surfaces. Monin-Obukhov Similarity Theory (MOST) generally applies to the surface layer which bottom is typically about 3 times the height of the roughness elements. Could the authors justify why MOST is still applicable when grids are used with grid lengths that are in the order of few meters.

Traditionally, MOST is applied over flat horizontal surfaces. For non-building resolving simulations, the grid spacing is coarse so that the requirement that the first grid point is within the surface layer (where MOST holds) is usually fulfilled. In building-resolving simulations, where we can resolve the buildings themselves by a large number of grid points and where we have to deal with both horizontal and vertical surface elements, we need an approach to describe the turbulent exchange of all these surface elements with the atmosphere. There is no theoretical framework available for this application, especially for vertical walls. Here, the use of a stability function is questionable. The established and widely-spread approach is to apply MOST locally for all surface elements (horizontal and vertical). Previous attempts to do this with PALM have been successfully validated e.g. against wind tunnel measurements (Letzel et al. 2008, Kanda et al. 2013, Park & Baik 2013). MOST is used here, despite it is known that some assumptions of the theory are violated. There is no physical justification for this, but the results indicate that this approach gives very reliable results. MOST is hence used in all building-resolving microscale models and is the basis of surface schemes such as SUEWS and TUF-3D. We added a more rigorous justification for using MOST and the relevant citations to the mentioned validation papers to the manuscript (P6, L5-7).

On page 6, from line 4 up to line 10, the authors outline a number of processes which they did not consider. I think that the authors should indicate why these simplifications are justified and what the impact is of these simplifications. For instance, I am not sure why the absorption of longwave radiation of the plant canopy can be neglected. Also, it is well known that the temperatures of plant canopies can differ considerably of the temperature of adjacent air layers.

The description of the omitted processes was completely rewritten. We also mention the purpose of this first version to model UHI situations during the summer heat wave episodes which affected the choice of the implemented processes in this version. We added a new Sect. 4 Discussion where we discuss the limitation of the model and the setup of experiment and we try to assess the potential impact to our results. Finally, we complemented the conclusion with the short description of the current and future development of the PALM-USM.

In the line 5 on page 7, it is stated that the RCSF "represents the proportion of the radiative flux carried by the ray at its origin", while I interpret the first term within brackets on the right *hand side of equation (8) as the effect of the attenuation of the original array by plant canopies in grid boxes that the ray crosses while going from A to C. Please clarify.*

The attenuation by grid box C has to be multiplied by the complement of sum of attenuations before reaching C on the path from A. This way we get the proportion against the original radiative flux and not just against the radiative flux that reaches box C. The text has been revised for clarification.

Page 9, line 11 to 13. It is not clear to me why the plant canopy has zero thermal capacity and why it should be applied to the grid box's air volume. In my view the absorbed radiation is used to heat up to plant canopy which on its turn exchanges sensible and latent heat with the surrounding air. Please, clarify

For our level of detail of plant canopy modelling, we decided to use a simplification where the absorbed heat flux is exchanged with the air instantaneously, just like it is done in some other radiative transfer models and in accordance with the current implementation of non-urban plant canopy model in PALM. Discussion of this simplification has been added to Section 4.

Section 2.4, page 10 line 4 to 11 form partly a repeating of the limitations already addressed in on page 6, line 5 to 10. Unfortunately, also here no justification has been provided.

We reorganized the text about limitations to avoid repeating. We state the general limitations at the beginning of the Sect. 2 (P5, L13-16) and radiation related processes in the Sect. 2.2.1 (P7, L5-7). We added the whole discussion section (Sect. 4), where applicability and the possible impact on the presented results of these limitations is discussed.

Section 3
Page 10, line 14 to 28: no general description of the observation site is given. For instance the description lacks a reference to an exact location (lat/lon) or a general description of the morphological and urban structure characteristics at the observational site. Though there is some description of the sensors provided, important parameters such as view angle are lacking.

Whole section 3.1 was extended and reorganized. All locations are given lat/lon coordinates, general description of the morphological and urban structure characteristics was added. Description of sensors was added/extended.

Page 11, page 5 to 13 It is not clear to me what the authors mean by "an indicative measurement of air temperature". Both the measurement technique and the location are poorly described and as such, too little detail is given to fully appreciate the values in this measurement in validating the USM in PALM.

The whole section 3.1 about observation location and measurements techniques was rewritten and extended to better describe the campaign. An explanation of what is meant by “indicative measurement” was added (P13, L14-18).

Page 12, line 19: As the latent heat flux is usually small in urban areas, atmospheric boundary layers are usually deeper over urban areas than over the surrounding rural areas. Is it verified that the a height of 2364 m is high enough to ensure that the top of the ABL is below this value?

The reviewer is right. The domain height was too low, indeed. In the course of the revision we also revised the model domain height which is now about 3.5 km, while the boundary layer during daytime is about 2 km, so that the new domain height can be considered to be high enough. See also the vertical profiles shown in Figure 5. The results indicate that the boundary layer in the city centre (as simulated by the USM) is indeed higher than what was measured using the radiosonde in suburban area. In that sense, the results appear to reproduce the expected behavior (see Figures 15).

Page 13, line 6 to 13 I think that assigning appropriate values for model parameters is at least equally important that including the correct physical processes within a model. In my view this justifies a comprehensive description of the methods that have been used to calculate the input parameters, which is currently quite short.

The section 3.2.4 about input surface and material parameters was extended. We also added two tables to supplements (Table S2 and Table S3) with parameter values that were used in our case study.

Page 13/14, line 26 to 34. The line of argumentation by the authors is quite hard to follow. I agree with the authors that fig. 3 shows a lot of detail, but I do not see the lateral variations of simulated temperatures due to urban characteristics. I can see that the lower parts of the south facing facades are considerably cooler because of shadowing. Also, it is not clear to me how I can derive from the figure that local shading effects by subgrid sized faces are not captured by the model.

We consider this view as an illustrative one. We added a view to the east facing wall in two morning hours (Fig. 19) which better illustrates the effect of material properties. It is true that the local shading effects by subgrid sized faces cannot be directly observed from the figure 3. We moved the discussion elsewhere in the text (P24, L10-14). The description of the figure was rewritten accordingly.

On page 15, line 1 to 8, the authors claim that the PALM USM does a good job at calculating the temperature evaluation at location 1 and in comparison with the Klementinum stations. This contrasts with the simulations with the WRF model which show a large bias. In my opinion, evaluating models on just one day is quite limited, and might lead to biased conclusions. Also, I am a bit surprised about the large bias in the WRF model. We know that *it has a tendency to produce too low temperatures, but a difference between 12 C and 18 C is quite large. I am wondering whether it is an error in the model or whether the grid cell used to define the 2 m temperature has a 'rural land use' rather than an 'urban' land use. To gain more insight into this, it might be an idee to include a line in figure 6 giving the simulated and observed temperatures at an appropriate rural reference stations.*

The urban parameterisation in WRF was intentionally not enabled in order to avoid double counting of the urban canopy effect which is treated by the PALM-USM model. This comment was added to the section 3.2.3 about large-scale forcing (P16, L6-7). WRF values in original Fig. 6 were 3-8 degC lower than presented measurements. However, WRF values are not directly comparable to displayed indicative and Prague, Klementinum measurements as WRF should represent background effect in the WRF-PALM-USM system, while both measurements correspond to temperatures inside urban canopy. Thus we opted to omit the WRF from Fig. 6 (Fig. 9 in the current version) and based also on the suggestion of the other reviewer, we extended the section 3.2.3 with WRF comparison to relevant ground and sounding weather stations. WRF show bias of roughly 1-2 degC . Despite the slight cold bias of the WRF simulation, we take the WRF-derived values as the best inputs available.

Page 15, line 14 to 20: the line of argumentation of the authors. The authors claim that in figure 7, there are differences in observed temperatures because of the differences in insulation properties. At the same time, the authors claim that modelled temperatures at points 3 and 4 because of 'almost identical surface and material parameters' The model temperature line for point 4 in figure 7 is quite hard to distinguish, but it is clear to the temperature line at point 5 deviates from the model temperature line at point 3, but also from the temperature line at point 6 and 7. In contrast, for the other sites, differences in model nighttime temperature are remarkably similar.

Whole Sect. 3 was completely rewritten. The description of the comparison of modelled and observed values was significantly extended and clarified. We added new Sect. 4 Discussion.

Page 17, line 4 to 9 and page 18, line 1 to 4: the authors identify some discrepancies between modelled and simulated temperatures, providing some speculation on which processes cause these discrepancies. I think that the paper would be much stronger when the authors include a sensitivity analysis substantiating the different processes that cause differences modeled and observed temperatures.

We performed tests of sensitivities to model setup parameters which could give some insight into one of the possible sources of differences. We also computed two idealized simulation. The first one illustrates the influence of the USM to the air flow in the street canyon and the other assesses the possible influence of the domain extent to the results. The sensitivity test are summarised in Sect. 3.4. We moved the discussion of the results and performed tests into Sect. 4 where also other issues of the model and setup are discussed.

Conclusion

In my view the conclusion section is much too general. At least the most important aspects of the model evaluation should be addressed and summarized.

We rewrote the section Conclusions to better describe the achievements and to reflect the new and enhanced results. We also complemented the short summary of the limitations of the model as well as the short description of the current and future development of the model.

Minor comments

Though parts of the paper are remarkably well-written (section 4), many other sections are quite hard to read, even for an experienced reader. I suggest that when the authors re-submit their paper, they go through their manuscript with a sharp pencil.

The manuscript is considerably rewritten, we believe that the text is more comprehensible now.

Also, the manuscript contains many typographic errors and misspellings.

The manuscript went through language corrections.

A number of figures, for instance figures 7 to 11 hard to interpret.

We increased the plot size together with the font size and reorganized the colour scheme. To make the plots easier to interpret we added some details (shading of the night time and solar noon line). We also extended the figure description.

Maps, such as figures 4 and 5, 11, 12 and 13 lack lat/lon coordinates.

Coordinates of the crossroads, which is in the middle of the case study area, was added to the text. Observation locations are shown in Fig. 3 which contains url to web map, where these locations are displayed. We also added coordinates too all graphs with model/observation surface temperatures comparison.

References

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