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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,

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Discussion paper

my advice to the editor will thus be to reject the paper in its current form.

Major points Introduction âĂć 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. âĂć 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? Section 2 âĂć 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. âĂć 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 long wave 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. âĂć 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 f the original array by plant canopies in grid boxes that the ray crosses while going from A to C. Please clarify. âĂć 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 âĂć 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. Section 3 âĂć 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. âĂć 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. âĂć 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? âĂć 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. âĂć 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. âĂć 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. âĂć 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. âĂć 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.

Conclusion âĂć 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.

Minor comments

âĂć 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. âĂć Also, the manuscript contains many typographic errors and misspellings. âĂć A number of figures, for instance figures 7 to 11 hard to interpret. âĂć Maps, such as figures 4 and 5, 11, 12 and 13 lack lat/lon coordinates.

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