

Interactive comment on “A new urban surface model integrated in the large-eddy simulation model PALM” by Jaroslav Resler et al.

Anonymous Referee #1

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General comments

The manuscript *A new urban surface model in the large-eddy simulation model PALM* by Resler et al. describes the addition of new radiation and energy model (USM) to the existing large eddy simulation model PALM. This is an important addition to PALM, which before did not account for radiation transfer in complex street canyons well. This is important for several urban processes and thus the new model clearly is a welcomed addition to the capabilities of PALM. There are however some challenges, what comes to representativeness of the new module and presentation of the methods and results (see below), in the manuscript that needs to be addressed. Also the language of the manuscript needs further improvements. There are several parts that need revision and in the minor comments I've tried to point out some of them, but I suggest the authors to go through the language once again before resubmitting the manuscript.

Thus I suggest major revisions to the paper before it can be accepted for publication to Geoscientific Model Development.

Major comments

Representativeness of the new module

To my understanding the anthropogenic heat emissions only from traffic are accounted for. This might not be an issue in summer at the site of evaluation as there are no heat emissions from buildings, but what if the model evaluation would have been made in winter or in other city with huge need for air conditioning? This is a clear lack in the USM as the authors could have implemented e.g. a simple temperature related anthropogenic heat emission model following some activity profile similar to the traffic heat emissions. Also, USM neglects latent heat flux component from the surface energy balance, which can be important in neighbourhoods with more vegetation. At the same time I understand why in the first step of the model development only the some (most crucial?) points of the complex energy system are included, but the authors should still comment in more detail about the limitations of the model. Some limitation is currently described on P10, but the representativeness and limitations of current USM model version should be described in detail either in the results/conclusions or in a separate section after the results.

On P6, L3-10, the authors list radiation-related processes that were omitted from the radiation model. Could the authors add what is the level of impact these omitted processes might have on the model performance?

Surface properties in USM

Please add somewhere to Section 2.3 that the needed surface properties to run USM are given also in the Supplementary material. How is the clearness index (P5, L26) given to the model?

For the evaluation part, it would be important to know what surface property values were used for the different observational points presented in Figs. 7-11. Maybe a table

to the main paper or supplementary material would work? Also in the results the effect of the different properties could be extended. What was the anthropogenic profile you used for traffic emission? It would be good to plot this together with traffic rates and meteorology for the case study period (see comment below). The obtained traffic heat emissions (P13, L19-20) are rather large during peak traffic hours. To me they seem unrealistic so could the authors comment how they compare with other studies.

Model runs

The vertical domain height is high when compared to the horizontal scale of the simulated area. At the same time the authors say that outside domain area has minor impact on the processes within the modelling domain, but such a high vertical domain makes me doubt this. This must be affected by some further away surface not with similar characteristics as the study area. Could the authors comment this?

It is not explained clearly why did the authors use WRF data to provide forcing for the run. This is shortly described in the results (P15, L4-5, 7-8) but the explanation should be given already in section Model setup. How did the model forcing data look relative to the Karlow station data? Air temperature data is given in Figure 8, but how did wind look like? suggesti that new figure where meteorological variables from WRF and observations (Tair, wind, solar radiation) and traffic rates for the simulated period would be plotted.

Results

The model evaluation section is currently quite poorly written and needs revisions. Text on P15, L13-19 is unclearly written and jumps between differences in the observation points, comparison between model output and observations and furthermore locations. Also, is this part referring only to location 1 as its not clear from the text. If yes then the general conclusion that modelled wall temperature drops faster after sunset is not valid as only on half of the points this is the case and in half not. Rather this pace of cooling could be related to thermal properties of the different points. Please rewrite.

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On P17, L4, the overestimation takes place only in daytime. Please add this information. The authors mention here that the daytime overestimation could be due to heat capacity of the wall. This could be the case indeed as it seems that the surface is not storing enough energy in daytime and release it enough in night-time. This should be discussed more properly in the results section.

In generally more text about the surface properties and their impact to the model performance should have been added. I'm missing some sensitivity tests about the impact of surface properties to the model performance. For example the authors could choose some location point from Figs. 10 and 11 where the surface properties would be slightly changed and improve relative to the observations. This particularly in the case of Figure 11, where the surface temperatures seem to be completely off.

The authors could add more analysis on section 3.5 about the differences between PALM without and with USM. How great impact does the addition of USM have on the turbulent mixing. Could maybe some spatial means at different heights be calculated to really see how mixing is improved? Or maybe showing vertical profiles from certain points on the main streets? Due to missing measurements, I guess the authors cannot really comment is the representation of turbulent mixing improved or not.

The problems related to model/observation comparisons are not mentioned in the conclusions. Possible needs to improvements should be added there.

Minor comments

P1, L1: “a direct effect” -> “direct effects”

P1, L2: “This implies the need for a reliable tool for climatology studies that supports urban planning and development strategies” -> “This implies that reliable tools for local urban climate studies supporting sustainable urban planning are needed”

P1, L4-5: “...a new Urban Surface Model (USM) describing the surface energy processes for urban environments was developed. . .”

P1, L7: In the model the authors neglect latent heat flux and thus are not calculating

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the total energy balance for impervious surfaces. Please reword here.

P1, L9: Please open what MPI means.

P1, L19: I would remove the first sentence: it is said in the abstract already.

P1, L20: Add “in future” after increasing, add “local urban” in front of climate.

P2, L2-3: I would reword this difficult sentence as e.g. it is not clear what is meant with “sound scientific background”. I guess the authors mean rather tools?

P2, L4: Should be “. . .phenomenon related to. . .”. I would change the UHI reference to the original paper by Oke.

P2, L6: “. . .retention energy of urban surfaces and increased heat emissions from human activities.”

P2, L7: Not only building shadows create cool islands but also tree shadows and increased evaporation.

P2, L7-L27: In these lines there is unnecessary repetition and should be restricted. After the cool island should be the whole description how the heat islands are commonly studied and after that what problems these methods meet so that eventually LES modelling is required to understand the issue. Also on some lines the authors talk about urban processes generally and on some lines only on the urban heat island. Also the references on L17 consider only UHI and not e.g. air quality that the authors mention on L2 at the same page.

P2, L33: can be -> is

P3, L3: Remove comma from the front of LES.

P3, L3-4: “Many of the CFD models do not contain appropriate radiative models and to overcome this deficiency, an independent radiative models with the resulting radiation fluxes have been imported into the CFD model. . .”

P3, L9-16: The objectives of the manuscript focus now on the project under which the project is made of, but these should be rephrased to be more general and representative for the actual study. At the same time LES does not require CFD in its front so please remove it.

P3, L18: Abbreviation PALM should be opened in the text here.

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P3, L28: "...obstacles as well as the landform"

P3, L31: Replace next with Secondly; "...radiative exchange at the surface..."

P4, L2: of using -> to use

P4, L7: "...PALM-LES, further extends the surface parameterisations..."

P4, L12 "...plant canopies have not been..."

P4, L16: Again only radiation and direct heat flux is considered: not the whole energy balance

P4, L19: as well as -> and; material -> materials

P4, L24: heat fluxes -> sensible and storage heat fluxes. Also I would add already here at the end of the sentence that heat consumed to evaporation is not accounted for.

P4, L25: "The energy budget in the skin layer...". The reference to PALM-LSM is not needed here again.

P4, L26: Anthropogenic heat flux is missing from the equation. Units are missing from the variable descriptions (an also from later equations). Please add throughout the manuscript. P5, L3: Why here the potential temperature is used whereas in Equation (1) there is air temperature? Shouldn't zero refer to skin surface and not surface?

P5, L4-7: Could the authors add a bit more information about the parameterizations especially as the Maronga and Bosveld paper has only been submitted.

P5, L13: Should be systematically PALM-LSM.

P5, L16: You can replace "Ground heat flux" with "G".

P5, L17: Following equation 1 the layer next to surface should be skin layer?

P5, L22: The title could be "Multi-reflection transfer model" as then it would be systematic with the text on P4, L20-22.

P5, L29-L30: The processes related to shortwave radiation is unclear. It is written that process "Radiation sources from the sun...using the relative position of the sun" is modelled, but from the above text I get the impression that the shortwave radiation on top of the canopy is obtained from the chosen radiation module in PALM. Thus, please be more specific here.

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P6, L12: Here the authors use word irradiance at each surface whereas in the energy balance equation (1) they use net radiation. Please, systematize throughout the manuscript.

P6, L16: Remove “also” from the sentence

P6, L20: The abbreviation for the differential view factor (uppercase d should be give here).

P6, L21: As this is generally the equation used for sky-view factor I would add a reference to the equation.

P6, L23: The separation distance is explained in the previous sentence on the same line and thus abbreviation s can be used after “Under the assumption. . .”

P6, L26: Please explain what A' means. In generally text and equations are not very clear starting from here and ending on P7, L2 and additional information source needs to sought if you are not that familiar with the calculation of view factors. Thus I suggest the authors to add a bit more explanation to this part of the manuscript with proper description of the variables used in the equations.

P6, L8: Same applies to Equation (5). It is not explained that this equation is valid for the case where two canopy grid boxes C and D are between surfaces A and B. First it should be given what is the RCSF for a single grid box C or D.

P10, L16-28: The order of explanation is strange here. The authors first describe the measurement locations before explaining what instruments are used. I would suggest to explain first what is measured and how (surface temperature using infrared camera) and then the actual locations of the measurements. How far was the camera from the surfaces and what was its view in degrees.

P11, L22: What is meant with “slight changes in camera position”?

P11, L4: It would be better to describe here the selected surface cover types and not in the results section.

P12, L5-6: How was air temperature measured?

P12, L7-12: It would be nice to have the meteorological conditions plotted in a Figure from the around 1.5 day measurements campaign (see major comments)

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P12, L29: What is Medard prediction system?

P13, L24-25: It would be nice to have these times in the meteorological figure as lines or as radiation itself.

P13, L26: Add “modelled surface temperatures”

P13, L27: It is better not to use the name of the street when referring but rather use “along the west-east street”

P14, L4: “. . .of the domain to illustrate the effects of tree. . .”

P15, L1: I would remind here what kind of measurement location location 1 was.

P15, L2: What is meant with indicative measurement? The automatic weather station is not mentioned in methods and thus should be added there.

P15, L3: Klementinum complex does not say much to the reader. Is this large area? Is the station part of official meteorological monitoring? This should all be added to the methods. P15, L5: I would simplify the sentence: “The street level air temperature form PALM-USM is in. . .”

P15, L7: “. . .temperatures. . .”

P15, L9-10: “Comparisons. . .are displayed. . .”

P15, L10: “. . .observed temperature patterns. . .”

P15, L26 onwards; I would move Figure 5 to Supplementary material as there are already many figures, and new should be added. The figure is nice looking but not relevant for the actual paper.

P18, L2: plays -> play. Please open here what do you mean by these effects.

P22, L1: had been -> was

P22, L1: “. . .in the range. . .”

P23, L7-8: only sensible heat flux is given.

Figure 2: Add scale also to this aerial image similarly to Fig. 3. You could also draw the area of the observational image to Fig. 3 in a similar fashion as you show the modelling area with green.

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Figure 4: Figure text needs more explanation: It would be good to add the date and that the data shown is modelled.

Figure 5: Scale is missing from the figure. Would it be possible to add this small area to Fig. 3 as a box?

Figure 6: It is difficult to see the lines if printed in black and white. Some of the darker colour lines could be plotted as dashed lines to separate them. I think AGL is not explained in the manuscript. Also as the figure should be able to be looked without references to the text, the authors should add the location 1 above road to the figure text as well as the time (2-3 July 14:00-17:00).

Figures 7-11: The figure texts are very poor currently. Please modify them to include the day, time period, and on the first one also description about the solid and dotted lines. Also in Figure 7 it should be explained what the location 4 is and that within the location 7 points from the IR camera were analysed. Same applies to Figs. 8-11. The authors could add sunset and sunrise to the figures.

Figure 15: The model configuration options should be explained in the figure text. Same applies to Fig. 16.

Figure 17: Please add y-axis to the plot a).

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