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

Anonymous Referee #1

Received and published: 11 April 2017

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.

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 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.

The current version of the model allows to prescribe any anthropogenic heat with fixed diurnal profile of its release while it has no meteorology-dependent anthropogenic heat model. This limitation to heat emission from transportation is the design of presented study, which is justified by the character of modelled area and season. The corresponding formulations were supplemented to the text and present text was clarified. The new section

2.3 was added and the description of settings of anthropogenic heat in the study (section 3.2.5) was extended including the reference to our previous sensitivity study of the influence to heat from transportation. The discussion of limiting the anthropogenic heat to heat from transportation and of the influence to the results of the study was added to the new chapter 4 Discussion. The chapter Conclusions was extended by information about preparation of the coupled building energy model (work in progress).

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.

The reviewer is right, the latent heat flux is very important part of the energy balance in many cases and it was not sufficiently stated in the submitted text. To better cope with this topic, we clarified the intention of the first version of the model to simulate UHI situations which leads to selection of the implemented processes in the first version. We also added one corresponding paragraph to the chapter 4 which justifies this omission in the conditions of our modelled situation including a reference to the literature. We also extended the conclusions by information about the prepared extensions to the USM which will include also the treatment of the latent heat.

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?

The list of omitted radiation-related processes has been moved to Section 4 (Discussion) and extended with descriptions of impact.

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.

Statement that all needed surface and material parameters are listed and described in supplements was added to Sect. 2.4 (P10, L29-30). Two tables (Table S2 and Table S3) showing parameters used in the presented evaluation case study were also added to supplements.

How is the clearness index (P5, L26) given to the model?

Clearness index is a standard measure of atmospheric attenuation of solar radiation. As defined within the referenced article, it is the ratio of global horizontal irradiance (GHI, at

ground level) to extraterrestrial solar irradiance (ETR, at top of atmosphere). GHI is known directly from the model and ETR is essentially the solar constant adapted for orbital eccentricity and multiplied by cosine of solar zenith angle. We feel that these details are only relevant when studying the method in the referenced article, therefore the mention of clearness index has been removed.

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.

We added tables with material properties for all evaluation points to supplements (Tables S2 and S3). We also extended result section with sensitivity analysis on material parameter values (Sect. 3.4.2).

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.

We extended the paragraph about traffic heat rates (section 3.2.5) with more detailed description of traffic heat calculation. We added a diurnal profile of average traffic heat flux to a new figure with meteorological variables (Fig. 4). The presented values referred to heat fluxes right in the traffic lanes while it is common to present the anthropogenic heat rates averaged over larger area - e.g. whole city or over a grid cell of size of hundreds of m2. We added this clarification to the text together with averaged value over the whole model domain to ease a comparison with other studies. The estimates of anthropogenic heat differs considerable depending on the used methodology. Sailor (2011) reviewed different methods for estimating anthropogenic heat in the urban environment. The estimates for average (total) anthropogenic heat ranged from 9 - 150 W/m2, summer values being considerably lower than winter values. Our estimate of 2 W/m2 seems to be reasonable as we are considering only traffic heat in the area with only moderate traffic.

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?

The reviewer is right. The horizontal model domain is too small, which basically imposes some limitations in resolving the largest turbulent eddies. Ideally, these would arrange themselves into hexagonal cellular patterns that scale with the height of the boundary layer. In our case, this height was about 2000 m during daytime. In this context, the horizontal

model domain was way too small. However, this will not affect the available energy in the system. Our recent experience is that the feedback between turbulent eddies and bare soil (which behaves similar to solid walls) is rather small - meaning that incorrect representation does not have to lead to major drawbacks. Nevertheless, the vertical profiles of potential temperature as simulated display untypical unstable stratification during daytime, which appears unrealistic. The test run included in the revised version with a larger horizontal domain clearly shows that this can be removed by extending the model domain which allows a free development of turbulence. The temperature profile then was nearly-neutral as expected under convective conditions. For our results, this limitation leads to too high air temperatures within the canopy, which potentially affects the interaction with the surfaces. We discuss this effect in the revised manuscript. Unfortunately, we did not have sufficient computational resources to perform a run with a sufficiently large domain (say about 5 x 5 km). Our experience showed that the current version of the USM, particularly the calculation of the sky view factors does not scale well with increasing grid points which leads to a memory problems. We are currently working on a solution for this issue to make the application of the USM feasible on large domains.

The reviewer is also questioning whether there might be significant impacts from outside the analysis domain. This is of course very well possible. However, under very low-wind conditions (as in the present study), very local (street-canyon-size) effects dominate the local processes and larger-scale impacts (from regions far away) are of minor importance. Given the good agreement between simulation results and measurement data, we are confident, that the limitation of the horizontal model domain poses no major limitation to this first evaluation.

In the future, we plan to perform larger-scale setups were we will perform sensitivity tests regarding domain size so that this reasoning will be put on a more solid foundation.

We added this comment on domain size to the section 3.2.1 (P14, L13-15).

We also changed the statement about minor impact of the outside domain. The original formulation was inaccurate. The current formulation better corresponds to authors original intention (P15, L4-6).

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? I suggest that new figure where meteorological variables from WRF and observations (Tair, wind, solar radiation) and traffic rates for the simulated period would
be plotted.

To account for the processes occurring on larger scales than modelling domain, but still affecting the processes inside the domain, we employed the large-scale forcing and nudging option of PALM. As no observation data are available in needed vertical structure and time resolution, we used the WRF data to obtain needed forcing values. We extended the large-scale forcing description (section 3.2.3) by this explanation (P15 L16-19). We also added three figures showing the comparison of WRF data against both ground (Figs. 6, 7) and sounding (Fig. 8) measurements. Other meteorological values from Karlov station together with traffic rates are plotted in the new Fig. 4.

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.

Model evaluation section was completely rewritten and changed according to the new model runs. We tried to make it easy-to-read and moved discussion into the relevant new section.

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.

All text was completely reformulated and the relevant discussion was added to Discussion 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.

We added whole new section 3.4.2 - Sensitivity to material parameters. The sensitivities of the surface temperatures to albedo, roughness and thermal conductivity decrease or increase are described in this section.

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.

We performed a sensitivity study where we compare the basic run of PALM-USM model with an run with USM module switched off and fixed heat fluxes prescribed for all surfaces. We compare the resulting flow in the street canyon in the new section 3.4.1. The reviewer is right that we are not able to compare the results to any measurements and to prove the improvement of the representation of turbulent mixing now.

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

We added some discussion about the measurements uncertainty to the Discussion section (P33, L9-12).

Minor comments

All the minor comment which concern some particular formulation were carefully incorporated into the text. Since the text went through a deep revision, many affected parts have gone of it. We thus write the specific answer only to the comments which are relevant in the new text.

P1, L1: "a direct effect" -> "direct effects"

accepted

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"

accepted

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

accepted

P1, L7: In the model the authors neglect latent heat flux and thus are not calculating the total energy balance for impervious surfaces. Please reword here.

This omission is mentioned in the description of the surface energy balance equation and discussed on the Section 4.

P1, L9: Please open what MPI means.

added "the standard Message Passing Interface (MPI)" and the reference to the official web pages

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

accepted

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

accepted

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?

The sentence was reformulated.

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

The sentence has been changed to "One major phenomenon related to the urban climate..." and the reference updated to the original paper Oke 1982.

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

accepted

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

The entire paragraph was rewritten.

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.

This part of the text has been rewritten.

P2, L33: can be -> is

accepted

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

accepted

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. . ."

The paragraph has been rewritten.

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.

The reference to the project was removed and the part rewritten in the more general way.

At the same time LES does not require CFD in its front so please remove it.

Accepted, removed.

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

The authors of the PALM decided to drop the treatment of the name as an abbreviation and they consider it just a name now. (It is not done on the PALM web page yet.) We respect this movement and do not include the older long name in our text.

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

accepted, added to the sentence

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

accepted

P4, L2: of using -> to use

accepted

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

accepted

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

accepted

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

A new paragraph, which briefly describes the limitation of the current version, was added to this part of the text. These limitation are consequently discussed in the following text, mainly in section 4.

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

accepted

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.

Clarification added

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

accepted, the reference to LSM paper removed

P4, L26: Anthropogenic heat flux is missing from the equation.

Anthropogenic heat is not considered in the surface energy balance equation intentionally. As our primary intention was to add the anthropogenic heat from transportation, we model the release of the heat directly into the air. We added a new section 2.3 Anthropogenic heat which describes the implementation of the anthropogenic heat in the model.

Units are missing from the variable descriptions (and also from later equations). Please add throughout the manuscript.

Rather than adding the units to all variable descriptions throughout the manuscript we opted to create a comprehensive table where all variables are listed together with units and its description. This table can be found in supplements as Table S1.

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?

The potential temperature is used in there parameterization of H as it is the prognostic quantity in PALM and also because the use of Monin-Obukhov Similarity theory requires the use of potential temperature to account for the correct buoyancy. In the parameterization of G, however, the actual temperature must be used as there is no potential temperature within the solid material. Actually, in the code, potential temperature is converted into actual temperature using the Exner function in order to solve for the skin surface temperature.

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.

The paper of Maronga & Bosveld is currently in press and available as online version already. It gives an outline of the land surface scheme. As most parts follow the ECMWF scheme, we added a citation to it as well. For the interested reader, the full description is given on the PALM homepage. A manuscript which is purely dedicated to the land surface scheme is currently under preparation.

P5, L13: Should be systematically PALM-LSM.

accepted

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

replaced by "The flux G"

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

accepted, reformulated

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

changed to "Radiative transfer model" to be consistent within text

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.

The section 2.2 was clarified and partly rewritten.

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.

Net radiation is the total radiative budget (i.e. incoming irradiance minus reflected radiosity and emitted radiosity). In this paragraph we wanted to emphasize that following methods are used to model the irradiance, unlike the outgoing radiation which is readily available, therefore we have kept the distinction in the text.

P6, L16: Remove "also" from the sentence

accepted

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

accepted, added

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

Reference added

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. . ."

accepted

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.

A' is the iterator for the sum of all view factors having the same target face. The text has

been revised.

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.

The text has been thoroughly revised. The view factor geometry calculations are all done before accounting for plant canopy and the attenuation by plant canopy is applied separately, as stated near the end of Section 2.2.2.

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.

The whole section Measurements was reordered and explanations extended.

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

The explanation was added in the text. In the original version we wrote "...to correct for slight changes in camera position during measurement". Preposition "during" might have been confusing. Slight changes in camera position were result of the fact that camera was carried from one location to another each hour.

P12, L4: It would be better to describe here the selected surface cover types and not in the results section.

The text about selection of surface cover types (evaluation) points was added (P13, L5-13).

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

We added the description of temperature measurement device and its uncertainties (P11, L19-24).

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)

We included a new figure as Fig. 4 with meteorological conditions.

P12, L29: What is Medard prediction system?

We removed the confusing Medard name, which has no added value to a reader.

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

The nighttime and noon was added to relevant figures.

P13, L26: Add "modelled surface temperatures"

accepted

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

accepted

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

Reformulated as the figure was moved into supplements.

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

Text was rewritten and location better specified.

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

We added a description of all used meteorological stations in section 3.1.1. The meaning of term "indicative measurement" was explained (P13, L14-18).

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.

We added a summary description of all weather stations in section 3.1.1 and changed the description of Klementinum.

P15, L5: I would simplify the sentence: "The street level air temperature form PALM-USM is in. . ."

accepted

P15, L7: ". . .temperatures. . ."

accepted

P15, L9-10: "Comparisons. . .are displayed. . ."

accepted

P15, L10: ". . .observed temperature patterns. . ."

accepted

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.

The figure was moved to supplements.

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

Whole section was reformulated.

P22, L1: had been -> was

accepted

P22, L1: ". . .in the range. . ."

accepted

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

The section Conclusions was completely rewritten.

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.

Scales were added to the figure.

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

The description was enhanced.

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

As the figure was moved to supplements we decided to leave out the box in Fig. 3. We added lon/lat coordinates to figure description instead (Fig. S11).

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).

Figure was replotted to allow for BW print. The date was added and so was the reference to location 1 in figure caption. Explanation of AGL was added to the manuscript.

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.

Figures were modified, time of sunset, sunrise and noon was added.

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

The sentence "The setup of the model corresponds to the setup described in with reduced number of layers to 81." into the description of Fig. 15 (current Fig. 25). The term "Model configuration and…" was added to the description of Fig. 16 (current Fig. 26).

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

The description "Effectiveness of parallelization" was added to y-axis.