Response to Anonymous Referee #1

S. Karttunen, M. Sühring, E. O'Connor, and L. Järvi

Anonymous Referee #1 comment
Authors' response

This study presents the development of a single-layer urban canopy model (UCM) for the PALM large eddy simulation model (PALM-SLUrb). The UCM is intended to be used in the building gray zone that appears when doing self-nested PALM simulations over urban areas that resolve the buildings in one part of the city (PALM-4U) at metric resolution but dont resolve them in the parent nests with resolutions between 5 m and 200 m.

The UCM is newly coded, but following the equations of the version of the Town Energy Balance (TEB) that is included in SURFEX-V8.1. PALM-SLUrb assumes that the city is an infinitely-long streetcanyon and solves the energy balance of the roofs, walls, windows, and road. Shortwave radiation exchanges are calculated with the radiosity method and assuming an infinite number of reflections. For longwave radiation, reflections up to the first order are considered. Prognostic equations for the temperature and humidity of the air in the streetcanyon are solved to support application in LES mode with rapidly changing atmospheric forcing conditions. This approach is different from TEB. Surface resistances of horizontal surfaces are calculated using Monin-Obukhov similarity theory, for vertical surfaces the DOE-2, Rowley and Algren (1937), and Krayenhoff and Voogt (2007) formulations can be taken.

The PALM-SLUrb is tested in coupled mode for spring-time clear-sky conditions in Central Europe with one at-a-time modifications of meteorological forcing, urban form, or urban material parameters. Furthermore, the results for the surface fluxes when using the building-resolving version and the single-layer urban canopy model are compared for patches of LCZ2 and LCZ5 at horizontal resolutions of 2, 4, 8, and 16 m. Very similar daily cycles of the simulated surface fluxes and canyon air temperature and relative humidity are found for the resolved and parameterised urban canopy. A quite strong resolution-dependency of the simulated momentum flux is found, which could be due to the limitations of the Monin-Obukhov similarity theory at high resolution. The work presented in this study is sound, rigorous, the article is well written and the results are plausible. However, there are three main issues with this work

We thank the reviewer for the time the reviewer has invested in reviewing our manuscript as well as for the comments and suggestions given. We give our responses to the specific and technical comments below.

- A single-layer urban canopy model like the Town Energy Balance (TEB) is newly coded, more than 25 years after the initial TEB. Furthermore, there exist numerous publications (often published between 2000 and 2010) of similar single-layer urban canopy models. I understand that for technical reasons, the PALM-SLUrb is newly coded instead of using an existing single-layer UCM. However, I do not consider the submitted work to be a sufficiently novel model development worth a scientific publication.

PALM-SLUrb is indeed based on pre-existing concepts used in modelling the urban canopy layer. However, we think that the work brings novel value to the research community by integrating these concepts with a highly performant large-eddy simulation model system. Particularly, this new integration will allow for large-eddy simulations of the urban boundary layer across a range of resolutions where these weren't previously feasible, but which would be very useful in many studies of the atmospheric boundary layer. For instance, for city-wide simulations it is often not practical to resolve the entire city with a high grid spacing but instead apply the grid-nesting technique (Hellsten et al., 2021) for some limited areas. However, this implies that the rest of the city needs to be resolved at a coarser grid spacing where buildings, street canyons, etc., are not sufficiently resolved any more, possibly altering the urban morphology and biasing urban radiation and flow features. This, in turn, may then lead to biased surface fluxes. In order to overcome this shortcoming of building resolution and to better represent these coarse resolution model domains in terms of the surface energy balance, PALM-SLUrb can be applied, being a grid independent method to represent the urban atmosphere-surface exchange. Thus, the novel part of this manuscript is not primarily the TEB-approach but the combination of a TEB-approach with an LES model.

Beyond this, adapting the single-layer UCM concepts of TEB for a high-resolution LES resulted in a model that shares the same conceptual formulation as TEB, but differs drastically in its numerical and computational approach. For example, we needed to introduce additional prognostic equations for the street canyon air temperature and specific humidity, as the assumptions of TEB's equilibrium-approach are not valid within the short time scales of LES. We are not aware of any similar works to adapt UCMs for high-resolution simulations in the literature.

We firmly believe that developing new scientific software and model code is worthy of publication, especially in the cases where it opens new possibilities to perform studies not possible previously. Following the guidelines of GMD, this is a model description paper, for which the main objective requested by the journal is to describe and document the model and model development for future reference. We see great value in publishing such model descriptions, as these ensure that users have both access to information regarding the model and can cite its usage accordingly in their own works.

To address the reviewer's concerns regarding the publication worthiness, we have made improvements in the Section 1 (Introduction) to better communicate what added value the model brings for the modelling community and why large-eddy simulations with resolutions in the order of tens of metres are well suited for many applications concerning the atmospheric boundary layer.

- The sensitivity studies of the PALM-SLUrb that have been conducted are very simple and rather technical. In the last two decades, there have been many similar studies and also the large urban canopy model intercomparison studies (https://doi.org/10.1175/2010JAMC2354.1, https://doi.org/10.1002/qj.4589). These studies have derived recommendations on how to further develop UCMs. The work presented here is rather a technical test of PALM-SLUrb.

It is true that the sensitivity tests are first and foremost a technical test of PALM-SLUrb. However, we believe that such technical tests are vital in verifying the correctness of the implementation and model integration. The secondary purpose of the tests was to provide to users information on the relative importance of the inputs. The purpose of the sensitivity tests is explained in the beginning of Section 3.

We would also like to point out that the journal emphasises verification in its policies regarding model description papers:
"*Examples of model output should be provided, with evaluation against standard benchmarks, observations, and/or other model output included as appropriate. In this respect, authors are expected to distinguish between verification (checking that the chosen equations are solved correctly) and evaluation (assessing whether the model is a good representation of the real system). Sufficient verification and evaluation must be included to show that the model is fit for purpose and works as expected.*"

From: https://www.geoscientific-model-development.net/about/manuscript_types.html#item1

To make the connection to the journal's policy clearer, we have changed the word "*ensure*" into "*verify*" in a sentence in the beginning of Section 3. The new phrasing is "*The first aim of these tests was to verify that the model's responses to variations in input parameters and boundary conditions are both physically sound and interpretable.*"

- The PALM-SLUrb is intended to be used in the building gray zone. Given the comparison with the resolved and subgrid urban modelling, this will probably give reasonable results for the surface fluxes so it can be useful for nested model domains. However, the presented development does not tackle the main issue of the building gray zone. In fact, the UCMs should only be used at resolutions as fine as 100 m since they strongly simplify the building geometry (here an infinitely-long streetcanyon).

  In the resolutions between 100 m and 2 m, the buildings are not well resolved, but should also not be represented in a completely simplified way. The strategy presented in the submitted study is to just ignore this issue and use the street-canyon geometry anyway. However, this could lead to misleading model results, for example when a building is as large as the grid resolution (e.g. 10 m x 10 m). Then the plan area building density is 1.0 at this grid point, and there is not even a street canyon air volume. Therefore, even though the average fluxes simulated are reasonable, presenting results from simulations at such a resolution could be misleading.

We agree with the reviewer that we have not fully addressed the issue of the building gray zone, so this was also not intended and scope of this study. All models are representations of physical systems to varying degrees of simplification and approximation. Our model presents a simplified representation of a real world system that is the urban canopy layer. It is intended for the purpose of providing realistic urban surface forcing and bulk neighbourhood-scale meteorological conditions within the urban canopy layer for boundary-layer scale simulations with the PALM model system. In our opinion, the utilisation of an UCM for this purpose is justifiable, as the underlying model concepts do not depend on grid resolution. We interpret that the reviewer's concern is more towards the issue with the convergence of urban morphological parameters such as the plan area density if computed for too small an area. This is a valid concern which we discuss in Section 2.7.3.

It is true that these do not converge or are not well-defined for small areas. Hence, we state based on prior literature that at least 100 x 100 m^2 tiles are needed to compute these reliably. It is up to users how to implement this for the inputs, one option is to use a larger sliding window around the surface grid tile to compute the value for it, or just use a lower-resolution surface map. In either case, detail in the scales of street canyons and individual buildings is lost, but modelling this detail is not within the scope of this model. These considerations are also communicated to the users as a part of the official model documentation. As the Section 2.7.3 was perhaps not as clear in this respect as it could be, we have added more information on the loss of detail.

With appropriate inputs, PALM-SLUrb models the urban canopy and roughness layers, where the flow is influenced by individual roughness elements, letting LES resolve and model the flow above the height where these effects are blended together. We have modified Section 2.1 to make a better link with the modelling and concepts of UCL and URL as well as improved Section 1 to better communicate the model's purpose.

Minor review points:

- For frontal area index and plan area index, the nomenclature typically used in the urban climate literature should be taken (lambda_f and lambda_p).

Small lambda (λ) is used in TEB papers and prior PALM papers exclusively for material thermal conductivity. For consistency, we consider it to be reserved for this use. Although the use of λ for frontal area index and plan area index is widespread, we wanted to avoid confusion and selected a new symbol (calligraphic A) to be used with all area fractions.

- "dirunal" is used instead of "diurnal" at several instances in the manuscript.

We thank the reviewer for spotting this. We have corrected all occurrences of the incorrect spelling in the revised manuscript.

Response to Anonymous Referee #2

S. Karttunen, M. Sühring, E. O'Connor, and L. Järvi

Anonymous Referee #2 comment
Authors' response

General Comments:

This paper presents a single-layer urban scheme for use in the PALM large-eddy simulation model system (PALM-SLUrb). This approach aims to simulate urban fluxes without the need for explicitly simulating flow and transport processes around individual buildings, for non-building resolving grids. This broadens the capabilities of PALM by allowing modelling across larger urban areas. The new urban scheme is heavily based upon the single-layer urban scheme within the Town Energy Balance (TEB) (Masson, 2000), which has a large existing user base and support available, with generally available model inputs.

The work presented here is a useful development of the PALM model. However, it feels like a demonstration that the PALM-SLUrb implementation produces results that are realistic, rather than a comprehensive evaluation of the model and it's uses. I have several specific comments on the manuscript.

We thank the reviewer for the time the reviewer has invested in reviewing our manuscript as well as for the comments and suggestions given. It is true that the paper is a description of the model and a demonstration that the implementation produces results that are realistic. In this regard, we would like to point out that the manuscript is submitted as a model description paper. The journal policy states the following regarding verification and evaluation:

"*Examples of model output should be provided, with evaluation against standard benchmarks, observations, and/or other model output included as appropriate. In this respect, authors are expected to distinguish between verification (checking that the chosen equations are solved correctly) and evaluation (assessing whether the model is a good representation of the real system). Sufficient verification and evaluation must be included to show that the model is fit for purpose and works as expected. Where evaluation is very extensive, a separate paper focussed solely on this aspect may be submitted.*"

From: https://www.geoscientific-model-development.net/about/manuscript_types.html#item1

We do agree that further comparisons and assessments of performance would be useful, as discussed in a response to a more specific comment below. However, we believe the manuscript fulfills the presented requirements for a model description paper with regard to verification and evaluation, and that we have appropriately demonstrated that the model is fit for purpose and works as expected.

We give our responses to the specific and technical comments below.

Specific Comments:

- It is useful and valid to integrate a well-known and well-supported existing model, such as TEB, into the PALM system. However, the explanation of the model coupling and re-writing feels insufficient to present as a new paper. Technically, there are reasons to newly-code the urban canopy scheme, however I believe some of the details within the paper is unnecessary. Perhaps only the differences in the implementation should be detailed in this paper if technical details are to be included.

We acknowledge that especially Section 2 is very detailed and technical. This results from our interpretation of the journal's policy on the paper type, a model description paper, which includes the following:

"*The main paper should describe both the underlying scientific basis and purpose of the model and overview the numerical solutions employed. The scientific goal is reproducibility: ideally, the description should be sufficiently detailed to in principle allow for the re-implementation of the model by others, so all technical details which could substantially affect the numerical output should be described.*"

From: https://www.geoscientific-model-development.net/about/manuscript_types.html#item1

We agree that for a general reader this amount of detail is not required. However, we find these important when considering the paper as a reference for the implementation, which can be examined and cited in future works.

We did initially try to write the manuscript in a way that only differences in the implementations would be presented. However, we felt the end result was very confusing for the reader, as a change from here and there was presented, but the paper didn't give an overall idea what the model equations are and look like. As many of the TEB's model equations are at least slightly modified, this approach did not even make the manuscript significantly shorter. Hence, we wrote the manuscript as a complete description of the model, and feel that this approach is the clearest for the reader.

- Though TEB can have within-canopy vegetation (including multi-layered vegetation) (Lemonsu et al., 2012; Redon et al., 2017), the authors choose to exclude this from their new scheme for PALM, and keep vegetation through a tile-approach: L180 'The main difference to Lemonsu et al. (2013), in addition to several differences in technical implementation, is that SLUrb version of the radiation model adds windows but omits gardens at this point'. The authors then state that results from their sensitivity tests (Section 3) perform less-well due to their lack of within-canopy vegetation, supported by model comparisons such as Urban-PLUMBER (Lipson et al., 2023). I don't understand why the simulations with vegetation are compared against those without vegetation at all (L695), as the fluxes from the two cases will not be comparable. I believe that as the existing TEB model and code structure contains within-canopy vegetation, this 'initial' version of the PALM-SLUrb model is incomplete and should also contain these processes.

In general, we agree with the reviewer that such an in-canopy vegetation representation would be important to implement in SLUrb. Currently, PALM-SLUrb represent mixed urban-vegetation patches with the mosaic approach, where the vegetation and the artificial surfaces are considered separately.

Firstly, we would like to point out that TEB's vegetation representation depends heavily on which variant of TEB is used. The latest official versions (>4.0.0) of TEB do not integrate with any vegetation model, but rather use a simple Bowen ratio based approach to modify the heat flux partitioning from roads and (green) roofs (Masson et al., 2020). The SURFEX implementation has additional features such as coupling with a full vegetation model ISBA and the street tree parametrisation by Redon et al. (2017). These features are not present in, for example, WRF-TEB, the integration of TEB into WRF (Meyer et al., 2020).

PALM does implement a land surface model inclusive of vegetation representation (PALM-LSM), but a lot of its aspects are hard-coded for representing fixed, non-fractional surface tiles of the resolved PALM topography. Therefore, it would need a technical overhaul of PALM-LSM in order to allow SLUrb (or any other future model component) to use it for representing internal vegetation surfaces in the way that ISBA is used in the SURFEX implementation of TEB. Such overhaul would require its own validation and performance testing. Realistically, the Bowen ratio based approach will be quicker to implement and is already planned for SLUrb. We have elaborated this on L785-794 of the revised manuscript.

We would like to point out that no model is complete, and there are still many other features that would be useful in SLUrb. However, for practical reasons, this work needs to be done in stages, in conjunction with the development process and the release cycle of the rest of the model. We believe that this version we present here is the first useful and applicable version for many scenarios, and thus merits a model description paper.

We do acknowledge that the phrasing on L695 of the initial submission is confusing and misleading. The simulations with SLUrb are not run without vegetation, the vegetation is modelled with PALM-LSM using the mosaic approach (non-integrated vegetation). The fractions of vegetation and artificial surface covers are still exactly the same in SLUrb and resolved urban canopy simulations. We have rephrased the sentence in the revised manuscript (L714-716) and added an additional sentence in the surface setup description (L645-646).

- Though within-canopy vegetation has not been used within the model, the authors do include the representation of building windows, with L232 mentioning that 'The SW flux is allowed to be partially transmitted into subsurface window layers and subsequently indoors'. The paper does not include many details on how this works within the PALM system. What controls are there on the indoor temperatures, and ow does the coupling work here? How are the indoor temperatures set in the model?

The indoor temperatures are given by the user. It is used as a boundary condition in the subsurface energy balance as mentioned in Section 2.6. The transmitted SW radiation does not modify this value. We added the sentence *"The transmitted fraction is recorded as an optional output, it does not modify the indoor temperature used in the model."* on L253-254 of the revised manuscript to clarify this.

- The experiments conducted are very simplistic. It would be more interesting and useful to complete a more thorough evaluation of the model across different and realistic city types, or against similar (perhaps not so well high-resolving schemes), such as the WRF-LES as mentioned within the paper, or other schemes. Alongside this, some of the descriptions of the results are very basic, e.g., relationships between H and LE with increasing fraction. Other simulations are presented even though they are unrealistic, such as using resolutions that are not technically justifiable within PALM-SLUrb for the resolved urban canopy simulations.

When considering the form of evaluation to be presented in this first paper, we wanted to emphasise demonstrating SLUrb's fitness for its purpose. As its purpose within the PALM model system is to produce surface forcing and bulk urban meteorological conditions comparable to the high-resolution resolved canopy, we decided to go with a comparison of the two approaches. We saw this approach beneficial particularly in one aspect: the uncertainties arising from the rest of the model, its schemes, numerics, and boundary conditions would be eliminated. If we would have compared against e.g. measurements or other models, it would have been much more difficult to attribute the observed differences. Based on our experience with LES, these differences arising from other sources would not be insignificant.

We completely agree that it would be interesting and useful to complete such comparisons against e.g. eddy covariance flux measurements, other models and even satellite-based LST observations. We hope that this work could be done in the near future. Running city-scale LES just for the purpose of comparing surface fluxes against measurements or another model would not be efficient utilisation of resources in our opinion, and should be instead integrated into broader research work.

We have extended the introduction of Section 4 in the revised manuscript to better reason our approach with the evaluation.

- I am unsure of why the parameters modifications for the sensitivity tests here (Table 3) were chosen, and in fact in the paper itself it is stated that ‘a 10% change in a parameter may be relatively small compared to its range in the real world whereas for another it may represent its full physical range’. Would it be better to find a realistic range for each of the parameters, even a min-max range?

We ended up utilising the 10% range in order not to give false impressions about realistic physical ranges of the variables. Defining such ranges would need thorough research work on the past, current, and future building materials, which we felt to be excessive for the scope of the work. Even if we would have been able to define min-max ranges, it would still not give an idea of a “typical” range which would be required for comparability. Defining the “typical” ranges would require the knowledge of distribution of these parameters in urban areas globally. Hence, we think it is better to use the same relative variation for all parameters and normalise the results accordingly.

- One limitation of using the TEB model for the new PALM-SLUrb, is that it makes the assumption that the urban form is described using a single-layer infinite street canyon. This will have implications for the radiation balance and airflow, particularly at such high resolutions. How does this morphology type compare to actual urban scenes at this resolution? Is it an accurate assumption? Several newer multi-layer approaches, or approaches where the street-canyon approach is not used have been developed within the community – would it be better to use a more realistic geometry description?

As PALM already implemented a way to represent complex urban surfaces with the resolved urban canopy approach, a great emphasis was put on simplicity when planning the SLUrb model implementation. Of course, simplicity should not compromise the model’s fitness for its purpose, which is to produce surface forcing and bulk urban meteorological conditions comparable to the high-resolution resolved canopy. On the other hand, model complexity does not necessarily lead to increased accuracy. On the contrary, the first phase results of the Urban-PLUMBER intercomparison project (Lipson et al., 2023) suggest that models with a complex three-dimensional geometry representation do not perform as well as the models belonging to simpler categories.

With simplicity comes some other benefits as well. The number and definitions of the input parameters can be kept relatively simple. For example, the urban morphology in SLUrb is defined by only five parameters, all of which are very commonly used parameters in urban studies. Therefore, inputs for SLUrb can be created using look-up tables based on e.g. Local Climate Zones in cases where high-resolution laser scanning data of the urban surface are not available.

In terms of the four urban morphological parameters the model uses; urban fraction, plan area fraction, mean building height, and street canyon aspect ratio; the model matches exactly those values given to it as an input. The internal representation of the urban form is simplified, but nevertheless geometrically matches those given input values.

These aspects are discussed in the Section 1 of the paper, which we have improved in the revised manuscript.

Technical Comments:

- L680 – written 'dirunal' not diurnal. This is also in several other places throughout the manuscript.

We thank the reviewer for spotting this. We have corrected all occurrences of the incorrect spelling in the revised manuscript.

- Table 8 seems quite unnecessary, and could easily just be given in the text instead.

Although the table is a relatively small one, we think listing the same information in the text is not as clear for the reader as the table form. Therefore, we did not end up making the change and left the table as-is.

- L716 – 'SLUrb still compares closer to the 2-metre resolved canopy simulation in terms of total momentum forcing than resolved canopy with any other tested resolution' has a grammatical mistake.

We have corrected this in the revised manuscript, the revised version reads "Nevertheless, SLUrb compares closer to the 2-metre resolved canopy simulation in terms of total momentum forcing than the simulations with the resolved canopy at lower resolutions."

- L692 – I don't feel that it is necessary to explain to the reader the definition of the Bowen Ratio.

We have removed the definition from the revised manuscript.

- In some of the figure panels in Fig 4, the differences shown in the urban fraction are very small. Perhaps a different plot could show the changes better?

It is true that the differences are relatively small. We tried different variations of the figure, including showing absolute as well as the relative difference to the baseline. The former one has the issue that it does not indicate the relative magnitude of the differences, and the figure becomes difficult to interpret. On the other hand, the latter case is not useful with the variables which may take non-positive values. We contemplated different variations of the figure, with each with their pros and cons. After some consideration, we decided the alternatives could be confusing or hard to interpret for the reader, and left the figure unchanged for the revised manuscript.

- Table 6 and 7 – most of the values here are very small. Is there a way to combine all tables and give only the parameters with the largest changes to the results? Perhaps the others could be given in Supplementary Material.

For this purpose, the values are color-coded, so that the user may find the largest changes for a given variable-parameter pair easily. In addition, the values in Tables 5 and 7 are scaled, whereas in Table 6 the changed parameters are not numeric but rather parametrisation switches. Hence, we do not see a clear benefit in combining these tables and then splitting them partially into Supplementary Material.

- Perhaps the section 2.7 is overly detailed, e.g. 2.7.1 has a lot of detail that may not be necessary to a general reader e.g., L338 – 342, and L363 – 372.

Our response to the general comment on the level of detail and amount of technical details presented in the manuscript applies here as well. We agree that these examples here are not perhaps the most interesting to a general reader, but important when considering the paper as a reference for the implementation.

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