

## ***Interactive comment on “Model evaluation of high-resolution urban climate simulations: using WRF ARW/LSM/SLUCM model as a case study” by Zhiqiang Li et al.***

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Response to Reviewer 1 [Cover Letter] Dear Reviewer, We appreciate your precious time in reviewing our paper and valuable comments. It is your valuable and insightful comments that led to possible improvements in the current version. The authors have carefully considered the comments and tried our best to address every one of them. We hope the revisions meet your high standards. The authors welcome further constructive comments if any. We provided the point-by-point response as below. Modifications in the manuscript are highlighted in red.

Sincerely, Bo Huang, PhD bohuang@cuhk.edu.hk Professor, Department of Geogra-

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[General Comment] This study evaluates performance of the WRF model in terms of high-resolution urban climate modelling over an area encompassing two big cities, Shenzhen and Hong Kong. The chosen area of Shenzhen is heavily urbanized but only a small part of Hong Kong is urbanized. Perkins skill score is used as a major evaluation method throughout the evaluation. The authors argue that their study has proposed a methodological framework for evaluating model performance in high resolution urban climate simulation. I think this work is useful and has provided some information about high-resolution urban climate modelling applied to south China. I very much appreciate the authors' efforts to pursue this kind of modelling work. However, I feel that the manuscript in the current form cannot be accepted for publication. At a minimum, I would suggest some necessary revisions to make the paper publishable in the journal. But to engender a stronger paper, I feel that more extensive work might have to be done. I will leave it to the editor to decide whether such extensive work is required.

Response: The article is in pertinent response to the increasing presence of ambiguous or careless modelling practices in urban-scale climatology. It intended to state the necessity of model evaluation in urban-scale climatology modelling, draw attention within the community of urban climate modellers, and be a kick-off in reducing these window-dressing-like modelling practices. Therefore, the purpose of this paper is to remind modellers of the necessity of model evaluation in the urban climate modelling practices rather than helping to improve the model. Moreover, the modeller should conduct a systematic model evaluation to establish the trustworthiness of the new findings from an urban climate modelling since the model cannot be verified or validated. Furthermore, we reminded that the modeller should be cautious to conclude quantitative conclusions because it is impossible to differentiate the natural gap, observation bias, and model bias in the difference between observations and its corresponding modelled results. To sum up, we are confident that this paper is important to the urban climate

modeller community as it points out the pain points, that is, model uncertainties affect the trustworthiness of the new findings and it is impossible to identify the uncertainties of model completely.

[Major Comment 1] The introduction should be reformulated with greater care. The authors should survey the literature more thoroughly. Only a few papers are mentioned in the introductory section. I suggest the authors give a good overview of the existing studies on the topic and point out the limitations of the past studies and challenges/constraints. Identifying a gap or proposing a new method as well as outlining the contributions of the study is also helpful.

Response: We added some new related literatures in Section 1 to emphasize the importance of model evaluation in urban climate modelling and the fact that modellers paid minimal attention in their modelling practices. Moreover, we identified the systematic framework for model evaluation as the research gap in the urban climate modelling community and outlined the values of this paper in Section 1. (Pg2, Ln27-34)

[Major Comment 2] The data and methodology section should be structured in a more logical way. I think the authors could place model description and experiment/model setup before evaluation method. Overall, both section 2 and 3 are a bit confusing. The introduction of the model is lacking. The authors should clearly articulate what has been done and how it has been done. This can aid the readers in understanding the experiment setup/design.

Response: We revised Section 2 to improve clarity and provided more information about model description in Section 2.1 [Pg3, Ln2-23] and set-up in Section S4 of Supplementary Material. Moreover, we will submit another paper to describe all details about the high-resolution urban climate modelling including suggestions for modelling process, the design of the atmospheric model, model set-up, primary data processing, and a framework for quality assurance.

[Major Comment 3] In section 2.1, more details about the new dataset developed by

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the authors should be offered.

Response: We provided more details about the developed land surface dataset in Section S2 of Supplementary Material. Moreover, we will submit another paper to provide all details about this urban land surface dataset later.

The reasons for focusing on the simulations in the year of 2010 should be discussed.

Response: We selected the year of 2010 since it was the latest year that a complete government-initiated land survey was conducted, which provided access to high-quality field-surveyed land cover data that is crucial for the climate simulation. It requires various data sources for the development of the new land surface dataset, high-resolution urban climate simulation and model evaluation, and we have datasets available around the year of 2010. We mentioned it in the revision of the paper [Pg3, Ln11-12].

In section 2.2, more details should be provided as to the four-day segment simulations.

Response: We provided more details about four-day segments in Section S3 of Supplementary Material.

Did the model read in restart files every four days to continue the simulation?

Response: No. Each four-days simulation segment is a separated simulation.

How may a different simulation strategy affect the modelling results?

Response: Different simulation strategies is associated with the different spin-up method, which affect the modelling results. We added a small discussion about it in Section S3 of the Supplementary Material.

In section 2.3, instead of just giving two tables, I think more detailed descriptions of the data should be given. How are the comparisons between model output (grid points) and observations (stations) made?

Response: We already discussed these comparisons in Section 4. Moreover, we

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added some details about the comparison in Subsection 2.3 [Pg4, Ln14-19]. Furthermore, we would like to provide the source codes of the evaluation software packages to the readers for easy replication.

Representativeness of the observations and potential biases should be discussed.

Response: We added more details about the observation datasets in Section S5 of the Supplementary Material.

The authors should also indicate the reasons for choosing evaluation variables.

Response: We added reasons for choosing evaluation variables in Subsection 2.3 [Pg4, Ln9-12].

[Major Comment 4] In section 2.4, no references are cited regarding the Perkins skill score. Is this a suitable method for this study? There should at least be some discussion. Authors should also discuss whether this method is suitable for all the variables evaluated in the study.

Response: We conducted a small discussion about the evaluation tools in Subsection 2.2 [Pg3, Ln24 – Pg4, Ln4].

[Major Comment 5] In section 3, choosing of the parameterization schemes needs discussion.

Response: We conducted a small discussion of the selection of parameterization schemes in Section S4 of Supplementary Material.

[Major Comment 6] I think the authors should tune down many of their arguments throughout the paper to avoid overstating (e.g., P2L25-26). For example, I don't see any strong methodological framework being discussed and described in the text.

Response: We enhanced the description of methodological framework to support our statement. We add a subsection (2.2 A Methodological Framework for Urban Climate Model Evaluation) to include more details about the methodological framework [Pg3,

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Ln24 – Pg4, Ln4].

[Major Comment 7] I have the impression that the authors have been too obsessed with ‘good results’ when evaluating the model’s performance. Discussing ‘good results’ and ‘bad results’ at the same time, in my opinion, is fair. It’s perhaps more important to identify areas for improvements.

Response: This manuscript intended to state the necessity of model evaluation of urban-scale climatology modelling and to provide a methodological framework of model evaluation to help modellers to establish the trustworthiness of modelling results, and accordingly it focused on the modelling performance rather than to help the model developers improving the model. We added an explanation in Section 1 to emphasize the focus of this paper [Pg2, Ln28-35].

[Major Comment 8] The structure and writing are too repetitive in section 4. This is also true for the figures. The number of figures may be reduced.

Response: We did our best to rewrite Section 4. Moreover, we moved some figures to Supplementary Material for reducing the number of figures in the paper.

While the focus of the paper as stated in the paper is on the urban climate simulation, evaluation seems to be applied to also the vast rural regions. The authors should clarify this.

Response: Yes. The methodological framework of model evaluation also can be applied in the local scale climate simulation wherever in urban or non-urban areas. We added an explanation in Section 5 [Pg11, Ln24-25].

I suggest the authors focus on the most important aspects of the urban climate simulation. I would suggest some points (see following) for the authors to consider and they should further develop a better evaluation framework.

Response: Thank you very much for your suggestions. We added a subsection (2.2 The Methodological Framework for Urban Climate Model Evaluation) to describe more

details about the methodological framework, which included a theoretical explanation to the statistic tools applied in model evaluation [Pg3, Ln24 – Pg4, Ln4]. Moreover, we added Subsection 2.4, which included a graphical presentation of the workflow of model evaluation, the guideline for checking the descriptive statistics figures and the grading guidelines for PSS and PDF of the difference [Pg5, Ln2 – Pg6, Ln6].

-Some basic ability of the model such as spatial distribution temperature/precipitation and diurnal cycles of temperature must be assessed.

Response: The difference in the surface temperature between in urban and non-urban areas (spatial distribution of temperature) had be assessed in Figure 9. The difference of precipitation between in urban and non-urban areas is not significant. The diurnal cycles of 2-meters air temperature had be assessed in Figure 3.

- The weather and climate variability in the study area is strongly associated with the monsoon flow. So the investigation of the simulation of precipitation and temperature is rather important. Both the spatial distribution (not found in any of the figures in the paper) and temporal variability should be considered.

Response: We agreed that the climate variability in the study area is strongly associated with the monsoon flow. However, the monsoon flow is a mesoscale meteorological behaviour and so it is not associated with the spatial distribution of precipitation and temperature at the local scale. The spatial distribution of temperature is strongly associated with the local land surface attributes. Therefore, we added some discussions in Subsection 5.3 about the relationship in the spatial distribution between 2-m air temperature and land surface temperature [Pg11, Ln5 – 25]. Moreover, we agree that seasonal variations in temperature and precipitation are associated with monsoon flow, especially precipitation. Therefore, we added some discussions in Subsection 5.3 on the relationship between the monsoon flow and the seasonal variation of precipitation, and the relationship between the monsoon flow and the seasonal variation of 2-m air temperature [Pg11, Ln5 – 25].

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In particular, the authors may identify some strong urbanization impacts on the precipitation (e.g., precipitation maxima) and temperature (e.g., urban heat island). The model's ability to capture these effects is essential.

Response: Observational data before and after the urbanization process are needed to evaluate the urbanization impacts on the precipitation and temperature. We cannot provide these evaluations because we don't have these observation data. However, we added some discussions in Section 5.3 on the relationship between the spatial distribution of the 2-m air temperature and the land surface temperature, and also the relationship between the spatial distribution of precipitation and land surface temperature [Pg11, Ln5 – 25].

In addition, simulation of sea breeze, wind distribution, boundary layer variability, and stability of the atmosphere should be examined.

Response: We agree that the land-sea breeze exists in the coastal city, and sowe provided a discussion about the modelled land-sea breeze in Subsection 5.3 [Pg11, Ln5 – 25]. These modelled meteorological features (boundary layer variability and atmospheric stability) cannot be examined by the observation due to the unavailability of corresponding observation data. Examining modelled meteorological features is meaningless without comparison with observations. Therefore, we didn't provide the examination of these two meteorological features.

The impact of urbanization on the air quality may also be discussed.

Response: This study focused on providing a methodological framework for the evaluation of urban climate models. The impact of urbanization on air quality is another big topic beyond the research scope of this study.

- The evaluation can be done separately for different seasons. The evaluation should focus on the most important aspects of urban climate/weather.

Response: Actually, all figures includes the information of monthly variations in this

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paper, while some also show the seasonal variations. Moreover, we emphasize that the model evaluation should focus on the comparison between the modelled variables with its corresponding observed ones. Furthermore, we added a small discussion about it in Subsection 5.3 [Pg11, Ln5 – 25].

- The scientific value can be enhanced if the authors can demonstrate how the model behaves in simulating the extreme precipitation events or heat wave/cold surge events, and How and to what extent these events may be related to the urbanization.

Response: Thank you very much for your suggestions. However, our study focused on reminding the urban climate modeller of the importance of model evaluation and establishing the trustworthiness of modelling results. We also provided a methodological framework of model evaluation, and so we didn't put too much effort on the modelling performance of simulating the extreme events. In this revision, we added some discussions about the capabilities on the simulations of the extreme events on Sections 5.3 [Pg11, Ln5 – 25].

- The model's performance between different regions in the study area and between rural and urban regions can also be compared.

Response: Thank you very much for your suggestions. We added more figures on the model's performance in urban and non-urban areas in Section S6 of Supplementary Material.

[Major Comment 9] The figures can be better designed and drawn. Captions of the figures should provide more information. The language could also be improved.

Response: Thank you very much for your suggestions. We did our best to improve the language and the figure captions.

[Minor Comment] Minor comments: The authors should check carefully the use of words and sentences throughout the paper. I suggest some serious edits/revisions. I list only some of the examples. P1L15: add 'have' before paid. P1L26-29: Please split

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the long sentence. P1L37: place 'into account' immediately after 'take'.

Response: Thank you very much for your suggestions. We did our best to check the paper, corrected the language errors and rewrote the long sentences to impro

Please also note the supplement to this comment:

<https://www.geosci-model-dev-discuss.net/gmd-2018-220/gmd-2018-220-AC6-supplement.pdf>

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-220>, 2018.

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