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Interactive comment on "Multi-layer coupling between SURFEX-TEB-V9.0 and Meso-NH-v5.3 for modelling the urban climate of high-rise cities" *by* Robert Schoetter et al.

Anonymous Referee #1

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General comments:

The paper 'Multi-layer coupling between SURFEX-TEB-V9.0 and Meso-NH-v5.3 for modelling the urban climate of high-rise cities' describes the implementation of an updated multi-layer SURFEX-TEB land-surface scheme into the Meso-NH model. The multi-layer SURFEX-TEB is evaluated against in-situ observations in the city of Hong Kong and compared to the previous single-layer version of the SURFEX-TEB. The paper highlights the importance of accounting for building drag and horizontal advection within the urban canopy for the correct estimation of temperature, humidity and wind speed in urban areas. Overall, the quality of the paper is good and it investigates a

very important topic. The current model development, presented in this paper, will be a step towards better weather prediction in urban areas. Yet, I still have a series of minor comments/questions (see below).

Specific comments:

Section 2

1.Lines 114-115. Have the authors tested the effect of vertical discretization of the wall surfaces on the temperature and wind within urban canopy in the multi-layer SURFEX-TEB? I can imagine that using a vertical discretization for the wall facet will allow for the calculation of wall heat fluxes that vary with height within the canopy. This might be particularly useful for reproducing accurate atmospheric stability conditions and vertical mixing in urban canyons. Would the benefits of implementing the vertical discretization outweigh the additional computational costs?

2.Lines 168-169. The roughness length for the roof in this study (0.15m) is larger than that used in different urban surface schemes (i.e. WRF-BEP use 0.01m by default). Is there any particular reason for using the 0.15m roughness length and does this have any implication for the exchange of heat and momentum above the roof? Is a similar roughness length (0.15m) used for road surfaces as well?

3. Is the anthropogenic heat flux deposited at the first atmospheric model level or is a different approach used (i.e. uniformly distributed in the canyon etc.)?

Section 3

4. Lines 267-270 The authors decided to use two heatwave periods (1 to 8 September 2009 and 17 to 31 May 2018) to evaluate the performance of the multi-layer SURFEX-TEB scheme. The selection of a heatwave period is certainly justified, as accurate model performance during heat waves is crucial for the estimation of heat stress. However, since the new scheme is to be employed for weather prediction it is essential to know whether the multi-layer scheme (NEW) offers an improvement over the single-

layer scheme (CLASSICAL) during different atmospheric conditions (i.e. rainy, cloudy days) and seasons (i.e. winter). Have the authors compared the performance of the NEW and CLASSICAL model setups under different atmospheric conditions?

5.Lines 317-320 How many of these measurement stations are located within urban canyons? Is there any relation between the location of the measurement stations and the model bias in temperature, wind speed and relative humidity?

Section 4

6. Have the authors tested the differences in the modeled surface energy balance and turbulent heat fluxes between the 3 model setups (CLASSICAL, NEW and SURFFLUX) at any of the measurement stations?

7. Lines 384-386. Why is the in-depth evaluation of the model performance in the KP and HKP measurement stations done for D4, when both stations are located also within D5? I understand that for a consistent bias comparison between all measurement stations (section 4.1.2) D4 is used, as it contains all of them. Yet since KP and HKP are located within D5, I would expect their evaluation to be done at the highest resolution domain. Have the authors tested the model performance at the KP and HKP stations in both D4 and D5? If so, does the analysis in section 4.1.1 lead to similar conclusions if it is done for D5 instead of D4?

8. Lines 393-395. Have the authors verified the use of 0.1 AOD (i.e. using aeronet stations or satellite AOD products) during both periods? During the 2018HW the assumption of 0.1 AOD seems reasonable, but during the 2009HW period there seems to be substantial difference between the observed and modeled incoming shortwave radiation, especially during the later days of the 2009HW (Figure 5).

9. Lines 515-516. The SBL scheme in the CLASSICAL model setup seems to produce extremely high temperature near the surface during noon (14 local time, Figure 11). Considering also the very low wind speed within the canyon, there seems to be insufficient mixing near the surface in the SBL scheme. What mixing length does the SBL scheme use to calculate temperature and wind speed within the urban canopy? Does this have an effect on the vertical mixing? Have the authors tested whether a modification in the mixing length leads to better results for the temperature, wind speed and relative humidity in the CLASSICAL model setup?

Technical corrections:

10. Lines 448-449 The definition of acceptable quality regarding the rmse error for temperature and relative humidity is rather arbitrary and no measure of acceptable quality is proposed for wind speed. I would suggest that the authors remove/replace the terms "acceptable/unacceptable" as they do not add anything significant to the model evaluation. The rmse values are enough to show the improvement in model performance for the new multi-layer scheme.

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