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Interactive comment on "COSMO-BEP-Tree v1.0: a coupled urban climate model with explicit representation of street trees" *by* Gianluca Mussetti et al.

Anonymous Referee #2

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This paper describes and evaluates COSMO-BEP-Tree v1.0: a coupled urban climate model with explicit representation of street trees. The authors assess the model during a heatwave event with observations from flux towers, surface stations and satellites.

The authors have presented a very strong study which is thorough and convincing. The issue of integrating the effects of street trees in urban meteorological models is important because increasing tree cover is "go-to" response of urban planners who wish to reduce urban heat impacts. As the manuscript shows, interacting processes are complex and outcomes not always obvious. The evaluation is clear and thorough, and the technical achievements will be beneficial for future urban studies.

The lack of open availability of the source code is disappointing for a study presented in GMD. The process for accessing code outlined in the study is not timely, and includes mailing physical documents and/or negotiation. I have therefore not reviewed the code. The authors should reconsider publishing the code openly on a persistent public archive.

Detailed comments follow.

Pg 2 ln 12: "Tree transpiration reduces the surface temperature of the foliage by converting part of the solar radiation to latent heat" not only solar radiation.

Pg 2 ln 15: "Modelling studies on the cooling potential of street trees have almost exclusively been performed at the scale of individual street canyons to single neighbourhood... Only very few studies investigate the city-wide impact of street trees, mainly due to the limited availability of models able to represent street trees at the scale of the city..."

It would be useful here to acknowledge the various mesoscale urban meterological models which have incorporated vegetation as low-height gardens within the street canyons (e.g. Thatcher and Hurley, 2012; Lemonsu et al, 2012; Wang et al 2013). Although not "street-trees" (as they do not provide shading on walls or reduce canyon skyview), they do shade the ground and interact directly with canyon radiation/ turbulent fluxes and momentum budgets, alter the Bowen ratio and reduce canyon surface temperatures. This will also give an opportunity to be clearer about the authors definition of "street trees" when the concept is introduced, as readers may assume models with in-canyon low vegetation, or even external vegetation tiles, are doing the same thing. The statement "very few studies investigate the city-wide impact of street trees" could then be more carefully stated, as there have been many studies at city or larger scales which have assessed the impact of urban vegetation, but most have used schemes with low vegetation or used a tiled approach, and hence missed important shading and skyview effect which is the strength of the current study.

* Thatcher M and Hurley P 2012 Simulating Australian urban climate in a mesoscale atmospheric numerical model Boundary-Layer Meteorol 142 149–75

* Lemonsu A, Masson V, Shashua-Bar L, Erell E and Pearlmutter D 2012 Inclusion of vegetation in the Town Energy Balance model for modelling urban green areas Geosci. Model Dev. 5 1377–93

* Wang Z-H, Bou-Zeid E and Smith J A 2013 A coupled energy transport and hydrological model for urban canopies evaluated using a wireless sensor network Quarterly Journal of the Royal Meteorological Society 139 1643–57

pg 6 ln 4: Why are the intensity terms (ΔV_i and r_i) dimensionless? Shouldn't they have units?

pg 8 ln 13 "street trees do not interact with soil moisture content as represented by COSMOS's land surface scheme" More explanation as to why this was the case – technical constraints? Or was it assumed that all soil is constantly irrigated in urban areas? You may wish to mention the drawback of this assumption (i.e. overestimating latent heat) at this time. Otherwise, do the underlying LSM soil and urban scheme exchange any fluxes? How? Figure 1 could be used here to better explain how fluxes of the urban scheme are coupled to the atmosphere and LSM.

pg 10 ln 6: For the last terms of eq 13, why is Ts associated with fnat, and Tg be with furb? I don't understand whether this temperature averaging approach is valid. I can see the equation is taken from Schubert (2013), but it is not explained there either. For example, take the extreme position where fnat=0, furb=1, then the equation simplifies to:

T2m = Ts + r (T1 – Tg)

Why is Tg part of the T2m temperature if there is no fnat? Also, how is the urban surface temperature here defined? Does it, for example, include the roof facets? I was pleased to see the clear definition of surface temperature for the evaluation of the satel-

lite observations (pg 17) which was based on the facets that the satellite sees (roofs streets and natural surfaces rather than walls). But it's not clear that definition is appropriate for 2m temperature out of sight of roofs, and within sight of walls. If the same satellite-based definition is used for the T2m calculation, authors should note that's likely to lead to underestimation of the cooling benefit of street trees for temperatures within the canyon.

pg 11 ln 3: It would be useful to include information here about the average fraction of green cover in Basel, for easy comparison with other urban studies.

p 11 ln 23: There is discussion of atmospheric initial conditions and on soil properties but it is not clear how soil moisture was initialised. Soil moisture has significant impact on the intensity of heatwaves (e.g. see Wang et al 2019), and soil moisture has memory much longer than the 5-day spinup, so this variable needs to be carefully initialised.

* Wang P, Zhang Q, Yang Y and Tang J 2019 The Sensitivity to Initial Soil Moisture for Three Severe Cases of Heat Waves Over Eastern China Front. Environ. Sci. 7

pg 13 Table 1, please provide explanation for name acronyms here for easier reference.

pg 18 ln 4. I didn't find the attribution of the night-time overestimation of air temperature to the inability of COSMO to represent nocturnal stable boundary layer conditions convincing. If it were an issue with COSMO failing to represent a stable boundary layer, then it should occur in the non-urban site (BLER) night-time cooling rate. However, the supplementary Figure 1 indicates the rate of cooling matches observations well (although with a positive bias throughout), which to me indicates the evolution of stability conditions are probably reasonable at the rural site. It therefore seems the overestimation of night-time urban air temperatures could just as easily be from issues within the urban scheme configuration rather than the atmospheric model or stability issues. The supplementary Figure 2 indicates the bulk albedo may be low, and the material properties of facets listed in Table 2 store a lot of heat. I understand those values are based on previous studies, but those values were derived through optimising WRF SLUCM, and so are not necessarily realistic urban parameters, but could simply be accounting for deficiencies in that model. Therefore, the parameters won't necessarily be appropriate for BEP-TREE at Basel. I'm not asking for the simulations to be redone, but the authors shouldn't be so quick to simply attribute night-time errors to the atmospheric model when urban parameters may be the issue (low albedo, high heat storage).

pg 20 ln 27 "are" -> area

pg 26 "Future Work" It was noted earlier that BEP-TREE does not interact with LSM soil moisture - this is a major limitation which should be discussed here.

pg 27 ln 21 "attributed to the inability of the model to reproduce a very stable atmospheric conditions" again, if this argument is used it should be better supported with information from the current study. For example, was the minimum value of turbulent diffusion coefficients (K_i,min) set at 1 m2/s (per Buzzi 2011) in the current experiments? What were the stability conditions at the rural site? Did an inversion form? Why is the rate of cooling at night at the rural site seemingly correct if a stability is incorrectly simulated? Do the observed wind speeds support the conclusion that very stable atmospheric conditions should have formed on those nights? Is there other observational support for the stability or non-stability of rural sites in the area (non-urban flux towers, boundary layer measurements etc)? Finally, I found myself referring to the rural figure often throughout the manuscript, it may be better placed in the main body near figure 6.

pg 28 ln 16 "additionally explained by an underestimation of the night-time temperature" I believe the night-time temperature is overestimated at the BKLI site.

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