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Interactive Comment

Interactive comment on "Development of the Surface Urban Energy and Water balance Scheme (SUEWS) for cold climate cities" *by* L. Järvi et al.

Anonymous Referee #2

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The manuscript describes modifications to an existing surface energy and water balance urban model to include snow processes. Model parameters are optimized and evaluated against data from Helsinki and Montreal and the model is found to perform well against observations.

The manuscript is generally well written and easy to follow. The authors provide a thorough description of the model parameterizations and details of parameter values which I believe would facilitate the reproducibility of the model. The study addresses a research area that still receives little attention in urban modelling and snow hydrology and therefore provides an important contribution to both fields. As such, I believe that the study should be published providing that the authors address the following important points:

1) The $S_{WE,LIM}$ parameter is currently a grey area in the model:

- Section 2.1.7 states that "SWE is assumed to be reduced to $S_{WE,LIM}$" but in Section 2.1.8 it is stated that "human activities redistribute snow. For example large roadside snow piles are created ...". Therefore, Section 2.1.7 suggests that the model does not conserve mass and "removes" SWE, but Section 2.1.8. suggests it is merely "moved" for fs purposes. Which is it? This needs to be consistent in both the model and the manuscript.

- $S_{WE,LIM}$ values are the same in both Montreal and Helsinki. How did you obtain these values and can you justify why they are the same given that, in the text, you note that clearing is "neighbourhood specifc"?

- If mass is not conserved and simply "removed", could $S_{WE,LIM}$ (too low for example?) account for the difference in snow depth between measurements and observations (assuming both depth and mass are lost) discussed in Section 3.4 and Figures 7 and 8?

2) Many of the parameters are site specific and, despite having read the manuscript 3 times, I have failed to track how many of those parameters were optimized with data specific to this study and how many parameters were optimized with data from one town. I appreciate seeing a sensitivity study but I actually find this section hard to read. I think it would be helpful to include scatterplots of range of value tested vs. RMSE; this would be a very efficient way for the reader to instantly see how sensitive the model is to specific parameters. However, as there are many parameters, the authors may prefer providing a table with all the optimized parameters, the range of value tested, the range of RMSE, and the final value decided upon.

Also, it seems that parameter optimization was performed one parameter at a time. In Section 2.3 the authors constrain four parameters against runoff. Given the large number of optimized parameters, it would be interesting to run a multi-parameter sensitivity analysis to assess possibilities of equifinality.

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3) Advection. The authors need to strengthen their argument for not including advection in their model:

(i) p1068, l14-16: Lemonsu et al. (2010) is quoted but the actual reference is Lemonsu et al. (2008) (Lemonsu et al., 2010 only references the 2008 paper) where Lemonsu et al. (2008) don't assume advection to be negligible but actually demonstrate that the footprint is homogeneous enough to omit Q_A from the energy balance. Looking at the surface cover fractions in Table 1, the modelled areas in this study look more heterogeneous than in Lemonsu et al. (2008). Furthermore, p 1087 the authors of the present study attribute advection as a possible source of error in the turbulent and radiative fluxes. Please show that neglecting Q_A as in Lemonsu et al. (2008) is appropriate for this study or discuss the potential errors associated with neglecting these processes more thoroughly.

(ii) p1073 acknowledges that "One of the most important factors controlling the energy balance and snowmelt is the patchiness of snow". However, many studies have shown that this patchiness leads to high advective fluxes (e.g Shook and Gray, 1997, HP; Granger et al., 2006,). Presumably, lacking advection the model only uses fs to weight the energy and water balance in terms of snow-covered and snow-free fractions? The authors may like to rephrase and avoid the word "advection" (fs represents much more than that anyway) and simply state that it represents snow heterogeneity at sub-grid scale. (I would also move section 2.1.8 to 2.1.2 and clarify that fs is used for the fractional weighting of the energy balance described in Section 2.1.2. 2.1.8 is a bit far and, without context, the reader forgets why it is there at all).

Minor comments:

- Last sentence in the abstract and p1090, l5-7 There is absolutely no proof that the model can be nested in large scale atmospheric model nor that it can be used in urban planning. The model is highly calibrated and the manuscript does not demonstrate that its parameters are transferable to other cities. The sentence should be removed altogether from the abstract and re-written in the Conclusion section.

Abstract, l18: What do the authors mean by "accumulation"? Do they mean the rate of accumulation? Given that snow depth is underestimated and that there are no SWE observations to assess mass accumulation, this is confusing.

Equation 2: How is T_R calculated? What is its value?

Eqs. 3 and 4: Why are Lemonsu et al. (2010) referenced? They have used these equations, like many others, but not developed them. Same for Koivusalo and Kokkonen l12; Verseghy (1991) also reset albedo above a certain snowfall threshold and would be a more appropriate reference if one is needed.

p1072, 18: Word missing between "surface" and "r_a"?

p1074 "the Ek et al. (2003) form of the function is used with coefficients derived from Swenson and Lawrence's (2012)". Which coefficients? 1.3? The study Swenson and Lawrence (2010) investigated fs for a climate model and they acknowledge that the shape of fs may not be appropriate for smaller scale studies. I can see no specific mention of the value 1.3 in their paper either. Please clarify.

Section 2.1.8: A figure showing the different fs shapes for given SWE would be welcome.

Eq. 18: Whilst not incorrect, normalizing RMSE by the range is unusual in model evaluation studies. The denominator is more usually the standard deviation of the observed variable (see Taylor, 2001; Moriasi et al., 2007; Glecker et al., 2008). Given that the range is larger than the standard deviation, the nRMSE, looks much smaller when using the range. I admit that I would much rather see nRMSE with standard deviation as denominator or no nRMSE at all as I think that the range just provides extremely small numbers which don't explain much.

p1077 l20: Dates for runoff are not consistent with dates p1076 l18.

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Section 3.1: This section does not describe results, I would move to the previous section as 2.5.

p1080, l19: reference to Eq. 4 in Järvi et al. (2011). Is it the same as Eq. 16 in this manuscript using the b1, b2, c1 and c2 given in l19? If so, maybe reference Eq. 16 instead so the reader does not need to find Järvi et al. (2011).

p1080, l25: Replace Eq. 15 by Eq. 16.

p1083: I think that it would be clearer if 3.3 was something like "Model Evaluation" and 3.3.1. was "Surface Runoff" and so on...

p1085, l5: I can see other possible reasons why snow depth is underestimated; how reliable are the precipitation data? What about $S_{WE,LIM}$ (discussed above)?

p1088, l2: this is the first time Q_F is mentioned in the text. How does it fit in Equation 1? How is it calculated? Are there town-specific parameters involved? Given that it seems to be rather important in Section 3.6, far more context is needed.

Table 3 is not referenced in the text.

Table 4: I think there is too much in this table. I would remove S and I (they are not mentioned in the results section anyway) and maybe nRMSE (see previous comment).

Figs 7 and 8: Why are the Figures starting on 1 January when the end of the spin-up time was before the beginning of the snow season? It would be helpful to know how close to observations the model accumulation is. It may also give a few clues as to why the model underestimates snow depth (it currently doesn't help that there is 200 mm difference between model and obs at the start of Figure 7a).

Fig 10: I find that the symbols are too large so the red symbols mask the black symbols rather a lot.

References: Glecker et al. (2008) Performance metrics for climate models, JGR, 113, D06104, doi:10.1029/2007JD008972

Granger et al. (2006) Boundary-layer growth over snow and soil patches: field observations, Hydrological Processes, 20, 943-951

Moriasi et al. (2007) Model evaluation guidelines for systematic quantification of accuracy in watershed simulations, Transactions of the ASABE, 50, 885-900

Shook K and Gray DM (1997) Snowmelt resulting from advection, Hydrological Processes, 11, 1725-1736

Taylor KE (2001) Summarizing multiple aspects of model performance in a single diagram, JGR, 106, 7183-7192

Interactive comment on Geosci. Model Dev. Discuss., 7, 1063, 2014.