Response to Reviewer's comments for Schoetter et al : Parametrisation of the variety of human behaviour related to building energy consumption in TEB (SURFEX v. 8.2)

Anonymous Referee #1

General Comments

This paper describes a method to improve the simulation of energy consumption within buildings by accounting for an ensemble of human behaviours and building uses within a single model grid point of an urban land surface model. The paper is novel, well organised, thorough and convincing. It will be useful for the urban modelling community and fits within the scope of Geoscientific Model Development. If the issues noted below are addressed, I recommend the paper be accepted for publication.

Specific Comments

Overall this is an impressive contribution, both in the technical implementation and in the reported improvement of model performance. However, some minor amendments and corrections will improve the manuscript. The paper is long, so I do wonder if readability can be improved by moving the whole of Section 2.3 into Appendix B. This section details how different human behaviours are represented in TEB. The basic methodology of this section is dealt with in Sections 2.1 and 2.2. Section 2.3 is an important contribution, but may be better placed in an Appendix to streamline the paper, and because it continuously references tables there. Also, there is no discussion of the effect that compartmentalisation has on the computation time of the various simulations. I would appreciate knowing the computational cost of the additional calculations.

Thank you for taking the time to provide a careful review of our submission. We move the entire Section 2.3 to Appendix A to render the article more readable. The text is now grouped with the related Tables. We also discuss the influence of compartmentalisation on computing time (Section 4.5). The simulation for the MAP experiment (6 compartments) takes a factor of 1.95 more wall clock time than for the DEF experiment (1 compartment).
Please find our responses to the technical corrections below.

Technical Corrections

pg1 ln17: “consist of the net solar and infrared”. Much of solar radiation is in the infrared, so “short and longwave” or “net all-wave radiation” is more appropriate.
Thanks, we rephrased this sentence (Page 1, ln 17).

pg1 eq 1: “R net = Q sen + Q lat + Q sto + Q ant ” This is a non-standard representation of the surface energy balance that causes confusion regarding the sign of terms, and differs from the cited source. For example, the current form of the equation is not consistent with your line 20: “The storage of heat in the construction materials leads to lower negative values of Q sto during the day.” As currently formulated, if Q sen is positive in the day (as stated), Q sto should also be positive in the day. Also, in this form the anthropogenic term will always be negative which is inconsistent with later parts of the paper. Suggest a more common form, for example as in Oke 1982 already cited in the paper (with all daytime terms positive), or conform to the cited source (Christen and

Vogt, 2004).
The discussion of Equation (1) was indeed not very precise. We now reformulate Equation (1) to make it conform to the cited source and modify the discussion of the different terms where necessary (Page 1, ln 20-22). The sensible, latent and storage heat fluxes are now negative during the day, which is consistant with the text. The anthropogenic heat flux is now always positive, which is consistant with the rest of the article.

pg1 ln 20: ". . .which leads to higher (lower) values of Q sen (Q lat)." True in the daytime only, and sign doesn't conform to cited source.
We reformulate this sentence (Page 1, ln 20).

pg1 ln20: "The storage of heat in the construction materials. . ." Other than issues noted above, I find this sentence difficult to understand with the use of lower (higher) negative (positive) etc. Suggest simplifying to say urban Q sto exhibits greater diurnal amplitudes than vegetated areas.
Done (Page 1, ln 21).

pg2 ln18: ". . .greenhouse gas emissions, its is important. . . replace "its" with "it". Consider revising to remove multiple "it" references.
Done (Page 2, ln 19).

pg2 ln24: ". . .lack of detailed information on the diurnal, weekly and annual cycles". Inventory approach might also lack information on spatial variability at appropriate scales.
Thanks, we rephrased this sentence (Page 2, ln 25).

pg2 ln28: "This approach therefore requires eddy flux measurements. . ." change to ". . .requires turbulent, radiant and storage flux measurements."
Done (page 2, line 29).

pg2 ln29: ". . .knowlegde" misspelt
Corrected (page 2, line 30).

pg2 ln33: "However, they rely. . ." replace with "However, they may rely. . ."
Done (page 3, line 1).

pg3 ln10: (and other instances) ". . .in an UCP. . ." replace with ". . .in a UCP" (as "in a you-cee-pee").
Done (e.g. page 3, line 12).

pg3: ln15: "The UCP-BEM require. . ." replace with "A UCP-BEM requires. . ." or "The UCP-BEM approach requires. . ."
Done (page 3, line 17).

pg4 ln7: "UCPs represent the. . ." replace with "These UCPs represent the. . ." because not all UCPs are based on street canyon unit.
Done (page 4, line 9).

pg4 ln26: Add "In reality" before "The behaviour-related parameters. . ." to differentiate from model parameters.
Done (page 4, line 28).

pg5 ln16: “A general overview of our approach to consider for a variety. . .” replace with “A general overview of our approach for considering a variety. . .”
Done (page 5, line 20).

pg 8 eq5: “linearised Stefan-Boltzmann law.” I don’t understand why emissivity is squared in this linearised form of SB law. I can see this comes from Bueno et al. 2012, but it is not referenced or derived there, and it differs from other published forms (e.g. Eq. (7) of “Linear relationships in heat transfer” (Marin 2009)). Please explain or redefine.
The formulation takes into account for infinite reflections and absorptions of longwave radiation between the surfaces. A derivation of the exact formula (without linearisation) can be found for example at http://web.mit.edu/16.unified/www/FALL/thermodynamics/notes/node136.html.
We now explain that the Equation represents infinite reflections and absorptions (page 7, line 19) and also use an ε symbol to represent emissivity in Equation (5).

pg11 ln10: “Mean Absolute Bias” is this the same as mean absolute error (MAE)? Perhaps use MAE, which is more common name. Otherwise define MAB.
Thanks, it is indeed the Mean Absolute Error (MAE). We changed the manuscript (e.g. page 11, line 26) and Figure 6 accordingly.

pg11 ln12: “We find. . .” I appreciate the comparison to test non-linear behaviour of fractional-approach, although Section 2.2 would benefit from subheadings, as this is a mini result.
Thanks for this suggestion. We now use subheadings in Section 2.2.

pg11 ln17: “However, such situations are rare. . .” are they rare in all regions? In all building types? I can imagine certain situations that would be thermally isolated, for example in non-residential buildings. If the fractional approach satisfies your accuracy criteria for all but the heated/unheated cases, can you separate these instances and use the tiled approach there? Otherwise state, “In this study such situations are rare. . .” and justify.
We agree that this statement was too strong. It is based on our analysis of urban tissue in France, but it might not be valid worldwide. For urban areas with a lot of non heated buildings separated from heated buildings a tile approach would indeed be appropriate. We modify the discussion accordingly (page 12, line 1).

pg11 ln23: “opening window,” change to “opening windows,”
Done (page 30, line 25).

pg11 ln24: “humans or their. . .” change to “humans and their. . .”
Done (page 30, line 26).

pg12 ln5: “We consider the design temperature. . .” In this section generally I was not immediately clear whether equations were calculated separately for each compartment or aggregated across compartments. It appears from the code they are separately calculated, which is appropriate. Perhaps reiterate at the beginning of Section 2.3 that each equation is undertaken on each compartment separately.
Thanks for this remark. You got it right, each equation is calculated for each compartment, which is required due to the non-linearity of most processes. However, it was not cleary explained in the manuscript. We now explain this briefly in the beginning of this Section (page 30, line 22).

pg12 ln25: “Since we cannot take. . .” change to “Since we do not take. . .”
Done (page 33, line 7).

pg13 ln20: remove second instance of “with and without shading. . .”
Done (page 35, line 14).

pg15 ln3: “it is possible to use the fractional approach. . .” isn’t the fractional approach already being used here? Or do you mean to make an added distinction from building use and human behaviour compartments?
Indeed, the fractional approach is already used, but not to separate between different ventilation behaviours. Therefore, this formulation meant that we could use the fractional approach to distinguish different ventilation behaviours if necessary. We rephrased this part to be more clear (page 37, line 3).

pg19 ln28: “we only consider for the. . .” change to “we only account for the. . .” or “we only consider the. . .”
Done (page 16, line 7).

pg19 ln29: “In households with high RT [regulation tendency], the design temperature for heating is on average lower...” I was confused here because I would think a household with high tendency to regulate temperature would have a higher heating design temperature (i.e. smaller comfort range – more regular). I see you follow Bourgeois et al. high/low definition, but with a name change to include “regulation”. I suggest an alternative like Efficiency Tendency [ET], clarifying that those in the high category would be more efficient in their energy use, thus allowing a lower heating comfort temperature.
Done (e.g. page 16, line 6).

pg20 ln9: “Bourgeois et al. (2017) also define indicators related to equipment of buildings with electrical appliances (EQ). . .” The cited source is clearer as to what EQ is actually measuring, that is “Ownership of large household appliances”. Suggest the sentence is reworded to define EQ clearly.
Done (page 16, line 19).

pg21 ln32: “The radiative part of the internal heat release is assumed to be 0.1 for all building uses. This might be an oversimplification, but the overall contribution of lighting to the total internal heat release is only 5% to 15%”. A value of 0.1 may well be a reasonable simplification, but the justification using lighting is lacking, as the radiative component of internal heat release should include the longwave radiation emitted by all electrical appliances. For office IT equipment the radiative fraction can be 20-40% (e.g. see Wilkins, C. and Hosni, M. H.: Heat gain from office equipment, ASHRAE Journal; New York, 42(6), 33, 2000). Please make more appropriate justification or recalculate.
Thanks for this very useful remark. We indeed missed that there is also a radiative fraction due to longwave radiation. We therefore modify our discussion (page 18, line 12) and increase the radiative fraction of the internal heat release to 0.2 for the residential buildings and to 0.4 for office and commercial buildings (Table A4). We repeated our simulations with the modified parameters. However, the results are not changed in relevant manner.

pg29 ln13: “In this area, the simulated building energy consumption is larger than the inventory.” You say post-war buildings are large consumers for heating, and that construction period is not taken into account in TEB, so shouldn’t the inventory show larger building energy consumption in this area than TEB? In any case, one could say a current shortcoming of TEB is that building construction period is not accounted for.

It is the opposite way round! The building contruction period is taken into account in TEB via the architectural database described in Tornay et al. (2017), but not in the spatialised inventory on energy consumption. Since post war buildings are large heating energy consumers it is plausible that TEB simulates larger values of anthropogenic heat flux than the inventory in areas with a lot of post war buildings. We rephrase the text to better explain this (page 27, line 1).

pg29 ln16: "The values for the RMSE are quite close to the absolute bias, which is consistent with the well simulated time series" Close values for RMSE and absolute bias don't necessarily mean a well simulated time series, just that the variance of the error is low (e.g. see Willmott and Matsuura: Advantages of the mean absolute error (MAE) over the root mean square error (RMSE) in assessing average model performance, Clim Res, 30(1), 79–82, doi:10.3354/cr030079, 2005.)
Indeed, we remove this statement.

pg31-32 Figure 7 and 8: State in the caption that these results are for the MAP experiments.
Done (pages 28 and 29).

pg36 ln5: "For each day of week" add ". . .the week".
Done (page 32, caption of Table A2).

pg38 Table A3: Define RT so the table stands alone.
Done (page 33, Table A3).

pg39 Table A4: QIN nom units should be [W m −2]. Define EIU.
Done (page 34, Table A4).

pg40 Table A6: Define 'low, medium and high design temperatures' or refer reader to appropriate section.
Done (page 37).

Anonymous Referee #2

The paper by Schoetter et al. describes the parameterisation enhancement of TEB in building energy consumption by accounting for the variety of human behaviours and compared the spatiotemporal variability of building energy consumption produced by different configurations. The paper is very informative with detailed description of the implemented enhancement. However, my major concern is the readability of the paper considering its length. I suggest publication of this paper in a revised form with the following concerns well addressed.

Thank you for taking the time to provide a careful review of our submission. We restructured the article in order to make it more readable by following the suggestion of Referee #1 to shift Section 2.3 to Appendix A and your suggestions to add a nomenclature and modify Figure 2. Please find the responses to your specific comments below.

Specific comments:

1) A nomenclature is suggested to be added. Although the symbols are mostly explained in place, given the number of symbols used in the manuscript, a nomenclature can be more friendly to the readers.
We added a nomenclature (list of symbols) to our manuscript (Appendix C).

2) Figure 2, a key component of this paper, needs to be redrawn as its current form is a bit misleading. It is clearly stated that NO interaction is implemented across different fractional building uses. However, such assumption can hardly be interpreted from this figure: it looks to the reviewer that energy exchanges are existing between mass 1 and mass 2. In addition, mass 2 should have interaction with roof while such connection is currently missing. I would suggest a two-part figure as follows, with one part showing the separate fractional building uses and the other denoting the exchanges of energy and mass between the indoor and outdoor environments of a single building use.
Thanks for this very valuable suggestion. We have restructured Figure 2 accordingly (page 8).

3) Figure 3: determination of dominant building types should be clarified. If fractions of two building types are comparable in one grid, which would be the dominant type?
Figure 3a deals with the dominant building use at grid point scale. Building use is taken for each individual building from the French administrative dataset IGN-BDTOPO. The dominant building use at grid point scale is then defined as the building use with the largest floor area at this grid point. We add a sentence to explain this (page 15, line 4).

4) Section 3.5: determination of the sub-grid fractions of building uses needs to be clarified: the current description is a bit convoluted. A flow diagram can be used to aid such description.
We agree that Section 3.5 is difficult to understand and we therefore added a table to better explain which subgrid uses and behaviours are considered for which dominant building use (Table 1; page 19). We also restructured Section 3.5 to be more clear.

Technical corrections:

1) Equation 1: the formulation is very unusual. I would suggest putting R "#$ and Q &"$ at LHS so these two become the income budgets while other three form the consumption/dissipation budgets.
We perfectly agree with this point and follow the advice of Referee #1 to reformulate Equation 1 to be conform to the cited source (Christen and Vogt, 2004).

2) Equation 5: the current form is very misleading. First, it is better to use a symbol to explicitly represent emissivity (e.g. ε). In addition, it is not clear to the reviewer why the emissivity is squared in this calculation.
The formulation takes into account for infinite reflections and absorptions between the surfaces. A derivation of the exact formula (without linearisation) can be found for example at http://web.mit.edu/16.unified/www/FALL/thermodynamics/notes/node136.html.
We now explain that the Equation represents infinite reflections and absorptions and use a symbol to represent emissivity (page 7, line 22).

Anonymous Referee #3

This paper describes a detailed parameterisation of the building energy demand as represented in urban canopy models such as TEB. The building energy demand model is coupled to TEB to predict the anthropogenic heat flux for Toulouse based on various levels of detail on building use and parameter values. I found the paper to be clearly written and has a number of important applications for studying urban climate and urban design. Although I believe that this paper should be published, there are some minor changes that may improve it.
Thank you for taking the time to provide a careful revision of our submission. Please find the responses to your review points below.

Comments:

1) I think some clarification on how ventilation is neglected during colder months could be helpful. Is this simply triggered when the air temperature during the day drops below some threshold? Or is this set to occur for certain months of the year? In particular I am thinking about how easy it is to apply this model to other locations on the globe (as mentioned by the authors in the conclusion).
First, the current formulation of TEB structurally excludes that there is ventilation in the same time as heating is made. Secondly, ventilation during cold periods is neglected by the way Equation A9 is formulated, especially due to the choice of the value of TV_{th} (25 °C). With this formulation the fraction of windows opened will tend towards zero if the indoor air temperature is much below 25 °C, which is the case during the heating season. Concerning the applicability of TEB, we believe that it is not necessarily the geographical location, but rather the type of building which restricts the applicability. Indeed for relatively old buildings lacking insulation, the relative influence of wintertime ventilation on heating energy demand is lower than for modern buildings, respecting very rigourous thermal regulation standards. For such buildings, the ventilation during the cold season is responsible for a larger fraction of the heating energy demand than for the relatively old buildings dominant in the center of Toulouse, where our evaluation took place. Future work on (fractional) ventilation during the cold season might therefore be required. We now discuss the issues related to wintertime ventilation in more detail and more precisely (page 36, line 19).

2) Although the discussion of the tile-approach and fractional-approach is sufficient (p11), I wonder if the tiled-approach was explored in more detail. For example, are there technical obstacles to completing one of the experiments using the tiles (i.e., substantial changes would be required to the offline code)? Or is it simply a computational issue (i.e, takes too long to run the simulation)? Was there any attempt to try the tiled-approach with the experiments described in section 4.2.3?
It would not be an issue of computation time to use the tile approach for test purposes. It would probably increase nearly linearly with the number of tiles, but still remain small compared to a three dimensional simulation with a mesoscale atmospheric model. However, we cannot use the tile approach for the simulations in Section 4.2.3 since this would require considerable developments in TEB, especially related to how the different tiles are initialised and also related to the postprocessing of the model output. These developments would be a bit similar to Section 3 of this manuscript since the variety of the urban tissue (building type, use, construction period, behaviours) would have to be partitioned to the different tiles in a sensible way. No changes in the manuscript.

3) Are the authors considering investigating the sensitivity of some of their parameter choices in future work? For example, which parameters are most crucial to reproducing the anthropogenic flux predictions? In particular I wonder about the parameterisation of infiltration and I imagine some of the (reasonable) choices regarding changing infiltration at night or during colder months could lead to noticeable changes in the experiment results. Or are differences in the infiltration model relatively minor compared to the differences demonstrated between the DOM and SIX experiments in section 4.2.3.
Thanks for the suggestion concerning infiltration. We repeat the MAP simulation by hardcoding the infiltration rate to 0, in order to test the influence of infiltration on the heating energy demand. For this very extreme case, we show here the time series of building energy demand corresponding to Figure 6. During the coldest periods of the year, the infiltration is responsible for about one third of the heating energy demand, which is a reasonable value. We therefore believe that, although there are many uncertainties related to infiltration, TEB simulates this processes sufficiently well to not cause substantial errors in the results. A very important result of the "no infiltration" experiment is that even without infiltration, there is still a tendency that TEB overestimates the sensitivity of the heating energy demand to air temperature. This shows that the bad results for this sensitivity for the

DEF and DOM model configuration cannot be explained by a too strong dependency of infiltration on air temperature. No changes in the manuscript.

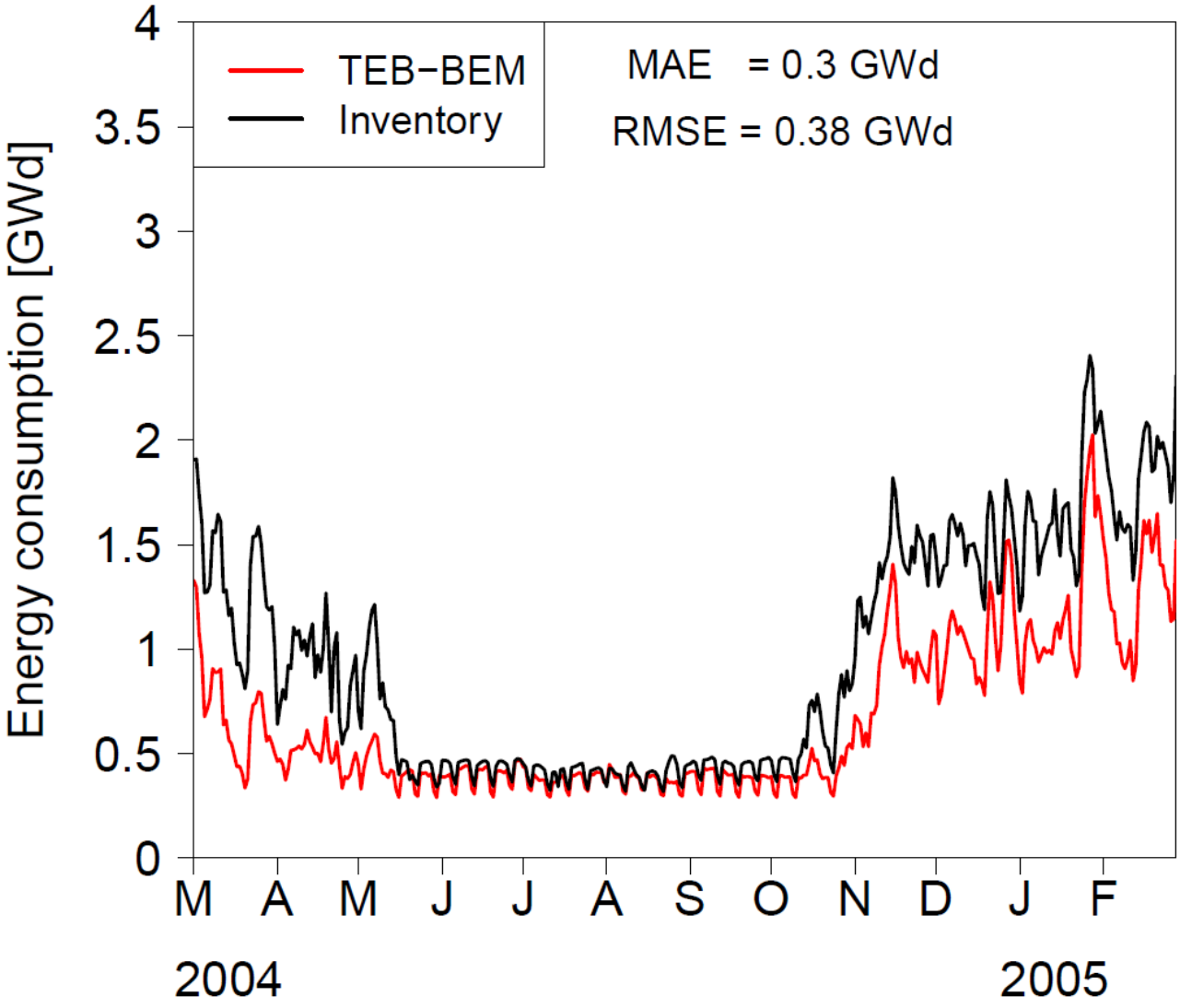

Figure 1 : Same as Figure 6d in the manuscript, but with an air exchange rate due to infiltration set to 0.