

Author's response to referee # 1

Our responses are in blue in between the original comments.

General comments

The paper addresses the complex question of vegetation-atmosphere fluxes of heat and water in the special case of planted roofs. Such roofs are being promoted as an important means of ameliorating the urban climate, as well as leading to energy conservation in buildings. The contribution this study aims to make is to describe planted roofs in better detail than other modeling schemes used at the city scale, which assume that such roofs behave in a similar manner to vegetation on the ground. The primary differences between roofs and the ground are: a) Planted roofs have a shallow substrate with atypical hydraulic properties; and b) they are typically well-drained, allowing much greater out-flow of incoming precipitation than would be expected in ground level soils with similar hydraulic properties. The paper seeks to describe these special features by combining the ISBA-DF model to describe the natural layers of the planted roof, coupled with the TEB model to describe the heat exchange with the building. The models are coupled at the interface between the lower surface of the hydraulic drainage layer and the upper surface of the structural roof (which includes thermal insulation and water-proofing). Results of the model simulation are compared with measured data from a full-scale planted roof.

Specific comments

The methodological design of the study is quite clear. The description of model parameters is detailed and meticulous. The statistical analysis of model results is careful and well-presented – both in a table and graphically (the use of the Taylor Diagram is very welcome). The discussion of the results is generally frank, and where results are disappointing, there is a discussion of possible reasons for this. The paper is, in general, well organized, has sufficient references, and is a pleasure to read. However, the proof of the pudding is in the eating, and where the paper falls a little short is in the actual quality of the model results. This is especially the case with regard to the predicted drainage flow, as well as the soil temperature at a depth of 77mm, where the error in some cases exceeds 10K (Fig. 6). Soil water content is the difference between incoming water (rainfall) and outgoing water (drainage and evapotranspiration). The paper describes only the drainage – it is assumed that ISBA-DF accounts properly for ET. However, drainage flows are not modeled with sufficient accuracy (the error over the whole period is nearly 130% - see p. 1147) – yet soil water content is generally modeled fairly well, despite the disappointing NSE scores, which are barely positive (p. 1146). This can most likely mean that ET is not modeled correctly – otherwise the water balance would not add up. However, this part of the model of soil-vegetation matrix is not described in the paper, and there is no report of any measured or calculated ET data to evaluate it. This, too, may be why the calibration procedure for the hydrological parameters of the soil did not yield a higher NSE rating and why, ultimately, it is likely that the roof-atmosphere coupling may not in fact be better than existing models which were not developed specifically for roofs.

Firstly, to emphasize the fact that the model accounts accurately for evapotranspiration, an entire paragraph has been added to the manuscript to present the detailed parameterization built in ISBA-

DF for the estimation of transpiration (paragraph 3.2.1). The magnitude of the latent heat fluxes over the simulation was checked and found coherent with the seasonal forcings. However, the fact that no record of LE is available on the experimental plot does not allow the checking of the green roof water balance (p 22 lines 27-28).

Secondly, the revised manuscript contains a slightly different type of analysis that was suggested by referee # 2: considering that our calibration tests look like an ensemble experiment, we analysed the performance of the ensemble mean and of its extreme elements (paragraphs 4.4 & 4.5) before evaluating the model (paragraph 4.6). The time periods chosen are described from p 17 line 20 to p 18 line 2 for calibration and p 19 line 25 for evaluation.

In the case of the experimental plot chosen for the model calibration & evaluation, the quality of the model-observations scores have to do with the periods chosen for evaluation because of their impacts on the observations themselves - in the case of drainage (with repercussions on water contents and temperatures). As the observations were likely to be biased because of a device with stoppers (partially preventing the water to be drained out of the green roof, p 17 lines 23-30) installed at the green roof base (already mentioned in the initial manuscript), instead of evaluating the model over the entire simulation period and discuss all hypothesis for errors (including stoppers), we chose to calibrate the model over a shorter time period but a period that would be the least impacted by stoppers (from p 17 line 20 to p 18 line 2). This exercise highlighted that the drainage layer behaved more like a soil made of organic matter than one made of clay or sand (paragraph 4.5 p 18-19), which is an important conclusion that is more highlighted than in the initial version of the paper. When considering the entire OM ensemble (Fig. 4 right and Fig. 6 left), the model-observation scores obtained over this time period are slightly better than in the initial paper but looking at the OM subset 2 (on Fig. 4 right), we can see that scores are actually better. This is confirmed by the evaluation exercise, which demonstrates good scores for the drainage (with a bias of 61 %, Table 5), with a good simulation of the dynamics and a slightly less good prediction of peak amplitude (Fig. 7).

Finally, ISBA being a physical (and not a statistical) model allowing the modelling of surface-atmosphere exchanges as well as hydrological and thermal transfers within the soil, it can be coupled to an atmospheric model to study a wide range of impacts, here of green roofs, for any town in the world under various climates (p8 lines 19-27). In this respect, it presents a significant interest compared to other existing green roof models.

Technical comments

p. 1128: The abstract states (lines 7-10) that the “module allows one to describe : : and to model vegetation-atmosphere fluxes of heat, water and momentum”. In fact, the paper itself focuses on hydraulic modeling of the soil. It then states that the model deals with “: : the thermal coupling with the structural building envelope” (lines 11-12) – but it is not clear whether it is full two-way coupling, or whether the roof temperature is used only to force the soil model.

The thermal coupling is two-way and although we thought we made it clear in the initial manuscript, it was re-enforced in the revised version (p 11 lines 19-26 and p 12 lines 9-11 and in the Conclusion p 21 line 22). Otherwise, we find that the paper describes both the thermal (paragraphs 3.2.2 & 3.2.3 & 4.6.2) and hydrological (paragraphs 3.2.3 & 4.6.1) aspects, even if the hydrology is more developed because of the difficulty in characterizing the behaviour of the drainage layer (paragraphs 4.4 & 4.5).

p. 1129: Planted roofs do not have “increased roof solar reflectivity” (lines 14-15): the albedo of vegetation is in fact very low, at about 0.15-0.2. There is a tendency to exaggerate the thermal benefits of green roofs (lines 17-20), but careful analysis shows

these are in fact more modest and nuanced. See for example: Moody & Sailor (2013): Development and application of a building energy performance metric for green roof systems. *Energy and Building* 60:262-269.

We agree. This was removed from the manuscript p 2 line 23.

p. 1130: UHI amplitude (line 26) may not, necessarily, be affected by a reduction in urban air temperatures, if the reduction occurs during daytime due to, e.g. evaporative cooling, while the UHI is driven by radiant cooling at night.

Due to the different energy balance of green roofs compared to artificial surfaces (i.e. the dissipation of daytime energy through evapotranspiration) the storage of heat during day time is reduced, resulting in a reduced releasing of this stored heat at night time. This should logically contribute to reduce night temperatures locally compared to standard roofs, hence affect the UHI amplitude. The question is how much and if this effect will be significant in the streets at the pedestrian level. This is the kind of aspects that we will be able to investigate with our GREENROOF model. We have moderated this conclusion by replacing “should” by “may” (p 3 line 27).

p. 1134: The term ‘spatial’ is used to describe time-related variations (line 8), instead of ‘temporal’?

No, we actually mean “spatial”. The text has been modified p 6 lines 24-30 to better illustrate what was meant.

p. 1143: In the description of the parameters for equation 5 – “cloud temperature is equivalent to that of the surrounding air” (line 8), add: “and cloud emissivity is equal to ‘1’ ”.

OK, thank you. Text added.

p. 1149: In the discussion (lines 9-13) of the temperature trends in Fig. 6, a possible explanation of the change in tendency after about Nov. 1 relates to the rather sharp drop in external air temperature, which is most likely accompanied by a reduced rate of ET: If the model does not account accurately for this process, this too could lead to the results shown in the figure.

As presented in the revised manuscript, the model does account accurately for the evapotranspiration process (paragraph 3.2.1 p 8-9 & p 10 lines 23-29) and the magnitude of LE were checked and found coherent with the seasonal forcings. Moreover, the results for the substrate layer (reservoir layer for plant transpiration) temperature are in good agreement with observations over the time period (after Nov. 1), hence the explanation may lie elsewhere : either with an underestimation of the temperature set in the model for the heating of the building, which was the main hypothesis presented in the initial paper, or an overestimation of the water content of the drainage layer at this time period. Unfortunately, without observations these hypothesis will remain unverified. In the revised manuscript, we finally thought preferable to remove this paragraph because we it was going into too much details for a long enough paper.

p. 1150: The conclusions state that the “GREENROOF module will allow the impacts of green roof implementation on various types of buildings: : :” (lines 12-13): This was not demonstrated in the paper, and in fact it appears that the coupling is one-way, with building temperature forcing the soil module.

Since the thermal coupling in GREENROOF is two-way, it will be possible to evaluate the benefits of green roofs by using it on various “types” of buildings by changing all the building characteristics possible within TEB (building height, fraction of windows, roof and wall surface albedo and emissivity, as well as the number of roof, wall, and floor layers and their respective thermal conductivities and heat capacities, without forgetting their heating/cooling characteristics).

Fig. 2: Please use a larger font in the block diagram – the text is barely legible, especially on the darker background.

OK, done

Fig. 5: If the blue line (upper graph) represents rainfall, please add it to the legend. It would be better if this information were displayed on a separate part of the chart, with a common time-scale but a better vertical scale (say max of 10, instead of 40).

OK, done