

## ***Interactive comment on “The GREENROOF module (v7.3) for modelling green roof hydrological and energetic performances within TEB” by C. S. de Munck et al.***

**Anonymous Referee #2**

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In this article, the authors describe the physical representation of green roofs in the Town Energy Balance (TEB) scheme. Although this is an important research subject, and although I found some aspects of the study quite interesting, this paper is seriously flawed and I cannot recommend it for publication in GMD.

The first problem concerns the lack of focus of the study. Indeed, it is not clear what exactly are the main objectives of this work (they are not stated in the Introduction) and what the authors wish to accomplish. Because of this vagueness in the authors' aim, it becomes quite difficult for the reader to pose any judgement on the results presented.

I understand that GMD mostly focuses on the description and evaluation of numerical

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models used in geosciences. But my opinion is that the approach presented by the authors is not evaluated properly, and that the presented conclusions are in error. My greatest concern is related with the “calibration” aspect of this work. In their quest to obtain the best solution, the authors performed a total of 576 runs based on all possible combinations of physical parameters for the vegetation and soil / substrate layers on the roof. As shown in Fig. 4, these runs are of a widely variable quality when evaluated against one of the chosen variable (here soil moisture). The choice of a single solution, or “calibration” as stated in the text, suggests that these best results could have been obtained by pure chance, and does not demonstrate the quality of the physics that was implemented in TEB. Considering that the results significantly depend on the specification of the parameters, it might be argued that a simpler representation of the physical process could be preferable. The tests performed by the authors look like an ensemble experiment. It would have been more interesting, I think, to examine the performance of all the members (or of the mean), and examine the uncertainty of the simulations as expressed by the spread.

Even if we accept the authors approach, then we are left with a best solution which does represent that well the observations. Results for the soil moisture are reasonably good, but there seems to be some problems for the drainage. And the substrate temperature has serious issues (bias and amplitude of the diurnal cycle), in spite of the authors' claim of success.

Finally, there are no demonstration that this set of parameters could be generalized to the city scale, as stated by the authors in the conclusion.

There are other minor aspects that would have to be corrected if this article is accepted for publication, related to the English language used (the paper is easy to read, but some sentences are awkward) and organization (many paragraphs are way too long and could be split in two, three, or even four shorter ones).