Review of revision of "Modelling hourly evapotranspiration in urban environments with SCOPE using open remote sensing and meteorological data"

For this review, I first focused on the authors' response to the comments from the reviewers together with the tracked changes. After that, I reread the "clean" version of the manuscript to assess the quality. Beforehand, I want to express that the authors have done a great job and the quality of the manuscript has improved considerably. Below, I explain my positive view on the revisions. A few points of concern and others out of curiosity are left. These should not be hard to address.

Response to comments

The authors have taken the comments of the reviewers seriously and have taken the time to respond to all of them. I would like to compliment the authors on the improvement of their figures, which are now easier to read and understand (Figure 6 is especially very insightful). The overview of available models that are able to model urban evapotranspiration is expanded and now in my view includes all major model types completing the introduction. This inclusion also showcases the added value of SCOPE compared to the existing models needing less parameters as input.

I do agree with the authors that interception does not mitigate UHI, droughts or make cities more sustainable. However, in order to create a complete overview of the water balance and specifically the composition of the ET signal observed with EC this is an important process, as shown by the literature cited by the authors. This is thus important for the final claim of the paper at the end of the conclusion: "Therefore, our approach is well-suited to produce ET maps that are highly relevant to urban planning and climate change mitigation.".

The discussion has been shortened and its readability and relevance have improved. The authors successfully frame their findings and limitations. However, I think the discussion could be more to the point. The sections are a nice separation, but the headers are not informative and already show that it are more 6 than 3 sections, as all headers name 2 topics. More descriptive headers and using more, shorter sections should solve the latter issue. The discussion still mentions topics (e.g. EC in urban areas) that are not relevant for this particular manuscript. This change was very successful in the introduction, which is now a pleasure to read.

In the new version, the novelty of the research is clearly argued to lay in the transferability of the model to other cities. In case this is possible, it would indeed be a great addition to the existing knowledge. Theoretically, the model should indeed be more transferable to other cities, given the limited number of parameters needed. From these sites, I find it hard to deduce whether model is indeed transferable. Has this been tested or is this planned? It would be nice to test it on a completely different setting and see how it performs. I am also wondering whether all the open data applied in this method is widely available for cities around the globe.

New manuscript

The new manuscript reads more easily and I is possible to follow the reasoning of the authors over the course of the research. A clear aim is defined and in my opinion attained. This makes the whole research more accessible and showcases the progress this paper presents.

When rereading the manuscript, I wondered about particular effects of the urban area on the vegetation. Does SCOPE in any way account for the clothesline effect (Oke, 2002) and additional

water supply to vegetation next to impervious surfaces? Otherwise, this should result in a relevant bias, since the change in the fluxes of these effects are considerable.

Table 1: The last link does not work (related to vegetation fraction).

L328: It may be convenient to state the definition of rBIAS.

References

Oke, T. R. (2002). *Boundary layer climates*. Routledge.