

# Review of “Modelling hourly evapotranspiration in urban environments with SCOPE using open remote sensing and meteorological data”

## *Summary*

In the manuscript, the authors present a methodology to improve urban evapotranspiration modelling by adapting the SCOPE model for urban environments and adding remotely sensed data. They successively supply different levels of information to the SCOPE model and compare its performance. They conclude the model is capable to predict hourly values for urban evapotranspiration based on the adapted model supplied with open remote sensing and meteorological data.

## *General comments*

The topic of ET in urban areas is, as explained by the authors, a very relevant and interesting one that differs from ET in more homogeneous areas. Predicting urban ET is difficult, especially on the hourly scale, but the authors manage to achieve satisfying results with their adaptation of SCOPE. Remotely sensed data is not widely applied in the modelling of ET in urban environments, while it is shown here to have potential to improve the quality of predictions. I am concerned about the use of the SCOPE model, since there are models that are readily adapted to urban areas, but the authors decide not to use these for unmentioned reasons. In addition, the quality of the manuscript itself is not of the level it should be.

In my opinion, this paper presents very relevant findings, but the manuscript is not clear and readable enough. In addition, a model is chosen that is not the most appropriate. Therefore, the comments below should be addressed to improve the quality of the text and thoroughness of the research before it could be published.

## **Structure of the text**

The introduction is clear in its content and covers most of the relevant literature. It frames the research nicely and puts it in the right perspective. However, there is room for improvement regarding the storyline, which is now mostly missing. Paragraphs miss a clear subject, their connections are not described and their order is not always logical (introducing EC and only later the less suitable alternatives). L36-44 is good example, where I am not sure what the message is: ET is both mass and energy; the composition of terrestrial ET or the different observational techniques. Some of these messages also come back in the next paragraph. In this case, I would reorder the paragraphs to separate the explanation of the ET process and the observations. This is not the only paragraph where this issue occurs. The same can be seen in the abstract, which is a summation of facts that are not actively linked. This leaves it up to the reader to fill in the blanks.

The structure of the manuscript stays a limiting factor in its understanding beyond the introduction. Apart from links between paragraphs, short phrases explaining what will be said in a section have the potential to improve the manuscript. I will comment on the textual side in more detail, once the structure has been improved.

## **Missing literature**

An elaborate overview of models is given (hydrological, surface energy models, L70-87). It feels like the models designed for the cities are overlooked, while they would be appropriate models. Grimmond et al., (2011) evaluated this type of models concluding ET is the most poorly modelled flux

making the analysis of the authors even more interesting. The authors state (L94): “Most ET modelling approaches assume a landscape of homogeneous vegetation without anthropogenic elements.” By which they omit a significant part of the urban climate research field. This is also missing in the discussion, where SVAT and hydrological models are discussed, but no urban climate models.

### **Choice for SCOPE**

This brings me to a related point and my main concern, which is the missing argumentation for the choice for SCOPE. The inclusion of vegetation and soil in the model is interesting, but it is not developed for the city. Other models UT&C (Meili et al., 2020) and SUEWS (Järvi et al., 2011) also include vegetation and soil, but are designed for urban areas already. Given the authors argue that the model is not suitable for urban areas (L229-230), why is SCOPE adapted for urban areas instead of using remote sensed input for a readily adapted model? SCOPE is not the most appropriate model for the study at hand, and the author should consider redoing the analysis with a model designed for urban areas.

### **Clarity of methodology**

While the actions taken reasonably well described, the methodology lacks explanation of the design choices. This raises questions for example regarding why data was left out and altered in a certain way. The effect of the choices should also be discussed. In the minor comments below, I indicate examples of choices for which the reasons are not clear. Also, for some parts references or explanations are missing, which is also indicated below.

### **Correction factor**

Defining the correction factor as the vegetation fraction creates an assumption that is not mentioned in the manuscript. By multiplying the predicted ET by the vegetation fraction it is assumed that this fraction produces all relevant ET. At the same time, literature shows impervious surfaces make a significant contribution to ET (Ramamurthy et al., 2014; Wouters et al., 2015). Therefore, this limits the conditions for which the presented model can be applied. In case of completely impervious area, the correction would lead to a permanently zero ET, which is not realistic.

In the discussion, the authors give a very complete framing to the results. The discussion also includes a lot of repetition and remarks that do not seem to have a purpose. Therefore, I think it can be shortened considerably. Implications of drawbacks and assumptions that are mentioned for the results are often not mentioned. The conclusion is very to the point, but should be more specific regarding amongst others the performance of the model.

The figures properly support the text and show the results clearly. I especially liked Figure 6. Unfortunately, the image quality of the figures is poor. For some, the images are grainy and individual pixels can be seen. They are also not easy to read due to missing legends and titles for panels. The captions are very long with a lot of information on how to read the figures. Legends and subtitles could help to transfer some of this information to the figures themselves. In the minor comments I included remarks per figure.

### *Minor/technical comments*

Apart from these larger concerns regarding the methodology and reflection, I have some smaller points that could help improve the manuscript. Also, I wrote down a list of textual improvements,

which is not meant to be an exhaustive overview. Since I think the writing needs major adjustments, I have not corrected the language in detail. The points are indicated with a line number.

L11-13: It sounds contradictory that most models assume a homogenous vegetated landscape, but at the same time they lack input parameters to describe the land surface.

L22: It is unclear to me what the respectively refers to, is that the different sites?

L24-25: I would like to see a more concrete suggestion for an application, instead of a very general statement that fits under almost all urban climate research. What does this research specifically add?

L30: I think the adverse health effects of the UHI are better described in other papers than Vulova et al., 2020. Either remove or replace this citation, since the other two are appropriate.

L34: In my opinion, it is not substantiated here why ET is so important in urban areas, although I do fully agree with the statement itself.

L63: A practical solution to what problem?

L101: Why is the choice here to use evaporation, while in the whole manuscript evapotranspiration is used?

L104-105: This sentence is not correct English.

L115-118: The use of comma's would improve the readability of long sentences like this (or consider breaking them up).

L123: Omit "(mm)".

Figure 1: The color scale for the impervious fraction is hard to read due to the use of two colors that do not have a different meaning. It would also not be readable in black and white.

The buffers are drawn at 1500 m, but in literature 500 m is used more often (e.g. Coutts et al., 2007 and Hong et al., 2020).

It could be useful to use letters for the different panels to ease referring to parts of the figure.

L136-139: The second sentence repeats much of what is said in the first one (e.g. a sonic anemometer measures orthogonal wind components).

L140: I would say this is part of a site description, not the data.

L146: The authors state: "Negative values (condensation) were set to zero." I think this affects the results, while it is not based on anything physical. What are the effects on the results? Has an analysis without setting all these values to zero been performed.

L146-148: The used prepositions seem odd here.

L151-153: The choice to leave out the 4 hours directly after precipitation will cause a problem, since at this time ET is especially high in cities (Ramamurthy et al., 2014). What is the influence on the conclusions?

L155-157: What methodology was used to calculate the footprints?

L173: This is not the first point the content of Table 1 is discussed.

L179: Why is linear interpolation performed if the differences are minor and irrelevant to the study?

Table 1: This table summarizes a lot of information very nicely. It also feels like a lot of repetition from the text, which provides the opportunity to shorten the methodology.

L188-190: Repetition of L155-157.

L196-197: This method is not clear to me, what pixels are used and which are not?

L198: What was the original resolution of this dataset? And is this one map for the entire year?

Figure 2: Please include a legend indicating the meaning of colors and line style (it is now only in the caption).

L221-223: Given that this is the definition of benchmarking (Best et al., 2015), it would be good to use that term.

L227: This sentence is not correct English.

L229: In my view, the model will not provide but require a lot of input, which in turn can be positive, but also has the potential to limit the usability of a model.

L233: Replace "group" with "groups".

L237: What are these other parameters at the end of the line? The same as the ones mentioned earlier?

Table 2: The unit of air pressure is stated as ppm, but this seems odd to me since this is not the standard unit.

L255: The direction of bias would be good to include.

L257-259: Is the vegetation fraction the correction factor? It seems logical, but it is not stated explicitly.

L259-260: This is stated more elaborately in L263-269.

L263: All available ET values observed before or after the filtering described earlier?

L263-265:

L266: What is the definition of rBias?

L272-273: Repetition of L75-76.

L274-275: The reference is to only one panel of the figure (3a), while results for any combination are stated. What are all the possible combinations and are these all shown?

L277-278: A very interesting result that could be highlighted more given its wider potential for application.

Figure 3: Panel a has a y-label stating DWD data and the others simply state DWD. Are these the closest DWD stations to the flux towers or were multiple combinations tested? On top of that, panel b-c show ET<sub>0</sub>, but in d-f this is replaced by predicted ET. ET<sub>0</sub> is in this manuscript also treated as a prediction. A minor detail is the inconsistency of the capitals in the x-labels.

In addition, panel a does not show the observed ET on the x-axis. Panels b-f tell a story together (evolving precision using different approaches), but panel a is different and as shown now feels as if it should be a separate figure.

This figure may benefit from titles for the subpanels to in one glance see the differences between the plots.

L285: This sentence contradicts itself. If atmospheric conditions are the main driver, why is it mostly dependent on the land surface?

L286-287: Repetition.

L288: Please add panels to the figure reference, since some are corrected for this assumption.

Table 3: The caption states that the highest precision and lowest bias are highlighted in bold, but this is not shown in the table.

The names of the scenarios may cause confusion, since  $ET_0$  is both a model approach and an input scenario. For example, in L303, it is bit clear without checking the values in the table what is the corrected  $ET_0$  prediction.

L317: What is meant with the word “selected” in front of SCOPE?

L320: This suggest SCOPE produces output for specific sources of ET, which would be very interesting. Is this correct?

L324: Panels 4c and d are referenced before a and b.

L330: The observed ET may be influenced by an anthropogenic moisture flux. How relevant is this at the study sites?

L332-333: Is ET prediction in SCOPE linked to the water availability?

Figure 4: A very insightful figure, but for panel a and b a legend is missing.

Figure 5: In order to clarify the link between the error and the precipitation, showing them in the same graph (reducing the panel count to 2) makes comparison easier.

What is the meaning of the dotted lines in panel b?

L360-363: Repetition.

L370: In order to value the comparison of the study at hand and the on done by the Senate Department for Urban Planning and the Environment, some background of that study should be discussed.

L375: I miss a comparison with the results of the work from the authors.

L378-379: To what study is referred here?

L406-408: Repetition.

L409-415: The message of this paragraph is not clear to me.

L417-427: Repetition of the introduction.

L435-441: Since none of these observation methods is used, why is this relevant for the study at hand?

L453-455: I agree with the authors that the interception loss/evaporation will be important in urban areas, but the link to anthropogenic heat fluxes should be explained.

L458-462: To my understanding, removing these 4 hours would decrease the value of the modelling approach for water balance estimation, as the interception evapotranspiration is not (completely) taken into account. As is also stated in the manuscript, but I do not understand why as a consequence these 4 hours are excluded from analysis.

L462: There is always a dew point temperature.

L462-464: I do not see why interception loss is comparable to water that is condensed and evaporates again or what the relevance is of that it is never taken up by the soil or a tree. Additionally, I doubt whether the water that evaporates from the mentioned wet surfaces has been detected by the EC systems, since they are installed high above the surface.

L466-467: To what bias is referred here and what is the source?

L468-469: What are the implications of this difference? Are the models wrong or the observations or is something else going on?

L470-471: If this correction is common practice, why is it not applied?

L473-492: There are a considerable number of interesting and noteworthy statements in here, but I miss the connections.

L494-501: In my opinion, this is a very important outcome of the study. It paves the way for the prediction of ET for cities without highly specialized observation equipment, such as EC. It deserves a spot light in the conclusion.

L506-508: I was surprised by the clear daily cycle in the composition of the footprint. I assume this is related to the daily cycle of the wind direction and strength, but a short background on this cycle would help to interpret the results.

L511-512: Has this been tested for the approach in this study?

L529: What is not comparable in what terms?

L542: I value the inclusion of a future perspective. However, I think in the previous subsections there are already parts of this perspective. It would help to keep them together. Also, part of the section itself are not a future perspective, but an evaluation of this study and how it was adapted to its goals.

L569: Please further specify "land surface".

L544-545: The verbs switch time in this sentence, so it is not clear whether this is a general statement or one on this study.

Figure A1: This figure shows the data for April are missing to a great extent, but more nighttime observations seem to be there. Could this partly explain the poor performance during this month?

L732-733: This link does not work.

## *References*

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