

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2  
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## **Comment on hess-2021-283**

Anonymous Referee #2

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Referee comment on "Modelling hourly evapotranspiration in urban environments with SCOPE using open remote sensing and meteorological data" by Alby Duarte Rocha et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-283-RC2>, 2021

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### **General comments**

The paper estimates ET from two urban neighborhoods in Berlin using a Penman-Monteith equation and the SCOPE model, postulates corrections to make these models work better for urban terrain, and then compares the models to observations from Eddy Covariance towers. For the SCOPE model, 3 levels of sophistication in imposing the input are tested.

The study unfortunately falls short in multiple aspects and would need significant improvements before it can be reconsidered for publication.

### **Specific comments**

1) What is the MAIN goal of the paper? The whole paper reads like an unstructured collection of discussions and results that do not seem to be motivated by any open scientific question or knowledge gap. It is obvious that ET models for natural terrain would not apply in cities, and that different models are needed. So that is not a new finding. There are multiple models that already handle urban ET with much more sophistication than SCOPE+correction, so that also is not a new aspect (listed as a novelty on lines 110-112). So what is novel about this paper?

2) The introduction goes on and on about measuring ET generally, but the paper is about modeling ET in urban terrain. The introduction needs to be completely rewritten to focus on the particularities of the physics of urban ET and highlight the gaps that this paper fills. Also the current intro is very convoluted and the ideas are not ordered coherently.

3) The paper presents as one of its main novelties the correction method for SCOPE and  $ET_0$ , but in fact this method and the resulting factors are never explain. From lines 104-105, it seems to just be the ratio of pervious-vegetated terrain for a given footprint. Is that so?

4) If this is the correction methods, then again what is the novelty. There are a number of models out there now that treat urban hydrology with a much higher degree of sophistication (including shading by building, in and out of canyon vegetation, ET from impervious areas ....). So this paper seems to only be relevant for users of SCOPE.

5) A potentially interest angle is the input to the model, where there are 3 tests with more detailed inputs. This could be an interesting question related to the value of land surface data in modeling urban ET, but it is skimmed over very superficially.

6) Section 2.2.1 is difficult to understand. The corrections done to instantaneous data and averaged data are mixed and explained poorly. It makes it look like the authors used the EC package but did not really understand it. For example

- the authors write "high- and low-frequency spectral corrections using double coordinate rotation" but double coordinate rotation does not correct for missed spectral bands, it corrects for sensor alignment...
- When the authors write "observations six standard deviations (SD) greater than the average (de-spiking)," is that for raw data or averaged ET? De-spiking is a term used for raw data cleaning.
- Why do they need big leaf to measure ET??
- Line 158: how much are "4993 and 5104" in % of data
- Do they remove data when the wind is coming from behind the tower? This is not mentioned

7) Line 510: it seems that the impervious fraction changes during the course of the data. I presume this is imply related to wind angle and the footprint analysis, but the actual impervious surface are not dynamically changing like the LAI of vegetation for example.

8) The later discussion sections are just a long discussion of so many ideas, things one can do, improvements and so on that do not seem directly relevant to the paper.

## Technical corrections

- “Whereas” is used multiple times including in the abstract in a wrong way linguistically.
- Line 36, mm is not a unit of mass and watt is not a unit of energy....
- Table 1 row 4, is it 300 or 333 m as they mentioned earlier?
- Line 328: it seems to be overestimating rather than underestimating.
- Lines 345-346: the authors write “As both models are deterministic, temporal autocorrelation in the residuals is not an issue.” I am not sure what this means.