

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2  
<https://doi.org/10.5194/hess-2020-640-RC2>, 2021  
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## Comment on hess-2020-640

Anonymous Referee #2

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Referee comment on "Quantifying the effects of urban green space on water partitioning and ages using an isotope-based ecohydrological model" by Mikael Gillefalk et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-640-RC2>, 2021

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The manuscript "Quantifying the effects of urban green space on water partitioning and ages using an isotope-based ecohydrological model" written by Gillefalk *et al.* provides a set of insights for water partitioning in a complex urban landscape. They incorporated the use of water stable isotopes in precipitation and soil to verify the model capacity for partitioning water fluxes. Also, they use eddy flux and sap flow data to evaluate the model results. Despite their meticulous work, there are some concerns about data collection and applicability.

### Major Comments

- Authors mentioned in lines 147-150 the use of another urban flux tower for a portion of the sampling period. It is important to highlight the fact that despite their similarities as "Urban Environments", the proportion of green spaces/buildings can affect considerably the model outputs. Also, the authors did not show a consistency analysis or a comparison for the period June – November in which both towers could be operating. This is fundamental to consider the fluxes as similar, equal, or different. Fluxes such as outgoing longwave and shortwave radiation, as well as water vapor can be affected and give different proportions. As an example, the 2-fold overestimation showed in Figure 7 with respect to sap flow data for April and May can be linked to the differences between flux towers without counting on the constant LAI effect.

The authors should ensure that this data set can be used by this other location.

- \* How different are the land covers within the tower's footprints?
- \* Are the fluxes for the period June-November equal/proportional/different?
- \* Do both towers have the same setup in terms of instrumentation?

- The manuscript is based on the application of a model which requires a calibration period. The authors mention the application of this procedure (Section 2.4). Despite the detailed description of the calibration procedure, two main questions remain unanswered:

\* Which data (period and source) was used for the calibration?

\* Did the authors apply a spin-up procedure (how long) or not?

This issue is important to assess possible trends or initial effects in the flux initial values.

- During the modeling procedure, the authors used a constant Leaf Area Index per cover. This can be true for the grassland depending on the species but the effects in trees and shrubs fluxes can be important. The application of this assumption triggered important consequences for the model results which end up with the overestimation of transpiration fluxes during the first part of the year (Figure 7 – April and May). However, the lack of sap flow data in shrubs affects the reliability of the fluxes from this cover.
- Along with the discussion, the authors mention the term “urban karst” given by Bonneau et al. (2017) which affects the water fluxes and redistribution by the preferential flow. Taking into account the heterogeneity of the subsurface on the sampling area (Lines 134-135) where the “subsurface is heavily impacted by human activities, and in places has an added layer of up to 50–180 cm of debris”, how does the preferential path flow form by these debris affects or potentially affects the soil water age estimations?
- How does the “tree with run on water” compare against the transpiration of the tree(s) sap flow measured in those pixels? This will support the affirmation given by the authors about the model performance and side effects of nearby impermeable land covers (e.g, buildings, pathways).

## Minor Comments

- Does the soil water content measurements were calibrated with soil samples along the sampling period?
- The authors mention the use of a German Weather Station that “records essentially the same rainfall” (Line 156). Can the authors provide the values?
- What are the urban tree species sampled for this manuscript (Lines 159-161)? Can the authors provide more information about the individual trees sampled (e.g, diameter, species, height, etc)?
- The paragraph between lines 167 to 175 describes the results obtained from the data collection described in the previous methodological sections. Consequently, this should be in Results and not in Methods and Material.
- The authors mention the use of Nash-Sutcliffe efficiency (NSE) as objective functions (Line 216). However, across the manuscript, there is only one reference to NSE in a broader context (Line 234) with no reference to the results of this analysis and neither in the supplemental material. The authors only mention Kling-Gupta Efficiency in detail (e.g, Line 272, 282). What happened with the NSE analysis?
- - The authors should follow the recommendations given by Knoben *et al.* (2019) when

using Kling-Gupta Efficiency analysis in models.

- - The authors should add more information about the results using the NSE. Also, adding the respective equations as for KGE.
- - It is necessary to add more information about the sampling processing (precipitation and soil samples). The following questions must be answered:

- \* How were soil samples collected?

- \* When collected, how long after a rain event the soil samples were taken?

- \* Which soil water extraction procedure was applied?

- \* What method/equipment/laboratory performed the stable water isotope analysis?

### **Recommendations**

- The authors can use boxplots in "Figure 10: Soil layers" instead of bars. In this way, the reader can have a better idea of the data distribution for each layer/cover.
- The authors can add the transpiration envelop in Figure 7. This will allow the readers to have a better notion of the temporal flux variability.

### **References**

- Bonneau, J., Fletcher, T. D., Costelloe, J. F. and Burns, M. J.: Stormwater infiltration and the 'urban karst' – A review, *J.Hydrol.*, 552, 141–150, 10.1016/j.jhydrol.2017.06.043, 2017.
- Knoben, W. J. M., Freer, J. E., and Woods, R. A.: Technical note: Inherent benchmark or not? Comparing Nash–Sutcliffe and Kling–Gupta efficiency scores, *Hydrol. Earth Syst. Sci.*, 23, 4323–4331, <https://doi.org/10.5194/hess-23-4323-2019>, 2019.