Comment on hess-2020-640
Anonymous Referee #1

Referee comment with author replies 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-RC1, 2021

Author replies in red

Overall comment

This paper presents the results of an urban ecohydrological modelling exercise using the ECH20-iso model. Growing season field measurements, including soil water content were used to calibrate the model. Qualitative validation was carried out by comparing measured and simulated water isotopes, surface temperature, and simulated transpiration to measured sap flow data. Overall, the paper was well organized and enjoyable to read. The clarity and simplicity of the figures was appreciated. I think there is room to improve the clarity of the methods with some additional details (see suggestions in specific comments below). As well, I recommend the authors try to make the model validation more quantitative (see suggestions in specific comments below). Lastly, I think the authors could place some additional focus on the urban design/management implications of the grass results, which suggest they are as important as trees for regulating green water fluxes.

We thank Reviewer 1 for her/his supportive comments and suggestions. In retrospect, we see that we could have been clearer in describing some of the methods used and gone further in validating the model. In addition, we agree that some of the management implications could have been better developed in the discussion. We are confident that we can address these issues on revision.

Detailed comments

48-9: Reverse order of this sentence, i.e., … by increasing infiltration and groundwater recharge, and thereby reducing stormwater runoff. OK

55: Replace 'removing' with 'to remove'. OK

57: Replace 'gives recreational benefits' with 'provides recreational opportunities'. OK

57-8: Remove '…to actually…' OK

63: Should be '… quantities of water that are partitioned…' OK

70-1: The point being made here is unclear. I recommend revising the wording for greater clarity. Will be revised

76: 'setting' should be plural. OK

81: Grammatical issue in this sentence – isotopes is plural, but sentence doesn't reflect that. Will be revised

96-7: Could the aggregated percentage of green and blue space be broken down for both? Will look in to this

Figure 1: Add a scale bar and north arrow to the zoomed in map of the observatory site. Will be added

144-5: Do the authors think that the drought conditions preceding the study could have impacted the results in any way? Yes, in fact we mention this in the discussion, lines 381-383: "This may be a "memory effect" in the moisture regime of soil-vegetation systems from the severe drought of 2018 and low rainfall over the winter of 2018–19. Circumstantial evidence suggests that transpiration in trees and shrubs could have been suppressed during the study period." We will emphasise this point further in the revision.

151: Missing words in this sentence - '… installed at the top of the flux towers…' Will be revised

150-7: What is the fetch (or footprint) of each tower? The tower on-site appr. 500-600 meters, the tower 6 km north of site appr. 700-800 m.
What proportion of the study period is each tower measuring convective fluxes that are representative of the observatory site? This will depend on wind direction of course. The tower on site should be measuring representative conditions at all times, regardless of wind direction. This therefore true for the calibration period, (as the tower on site went into operation the preceding summer).

Section 2.2.: Throughout this section it would be helpful to know the make/model of all the sensors that were used. Will be added.

161-4: How was soil sampled? How was water extracted? How was isotope analysis performed?
166: I recommend providing a brief overview of methods and then referencing Kuhlemann et al 2020b for more detail. We will add more details in the supplementary material to make our paper more standalone. But all details are available with Open Access in Kuhlemann et al. (2021), *HESS* (in the submitted manuscript this was referred as Kuhlemann et al. (2020b), since then the preprint has been accepted and published).

188-9: Is available moisture at the surface not used as well for partitioning available energy into convective fluxes? Yes, we will be clearer here.

198: How is the vegetation rooting parameter obtained? Is it based on field measurements of rooting depth? It is a calibration parameter, see Table 1, assuming an exponential root profile.

204: The calibration period is the period over which data were obtained from the site. Validation is qualitative. Is there 2020 data that can be used to validate? Thanks for this suggestion. Yes, there is SWC data but no soil isotope data. We have taken a look at this as per suggestion and results are the same as 2019, except for a poorer fit for Grass layer 1, where SWC generally is underestimated. We will include this validation evidence in the revision.

206: The soil division in the model is unclear to me. Were soil surveys used to characterize the spatial distribution of soil types? If not, was there any verification of the assumed soil characteristics? Many soil characteristics were used as calibration parameters and we made sure to stay within reasonable parameter ranges. But this was also based on previous soil surveys at the site. We will clarify this in the revision.

Figure 3b: Is the local drainage network storm sewers? Is there any channelized flow through the site? The local drainage network is topographically defined and shows the direction that water would flow in the model in case of overland flow. In our study, the local drainage network is really only relevant for the exploratory study described in sections 2.5, 3.6 and 4.4. For the green spaces that are the focus of the study, there is no overland flow.

239: I think you could use 'ground-to-atmosphere' instead of 'upwelling'. We will clarify on revision.

247-9: How common are "infiltration hotspots" at sharp interfaces between impermeable and permeable surfaces? In my experience, these areas are usually quite compacted, thereby preventing infiltration. This is a good point and we will acknowledge this issue on the revision. However, in Berlin the sandy nature of the soil means that compacted areas are limited to areas with high footfall. That is not the case at the study sites, but we will acknowledge this limitation.

267-8: Were surface cracks visible to the field team? No, they weren't. Another interpretation, based on subsequent data collected at shallower depths, is that water did not reach down to the sensors at 10 cm, while in the model the top layer starts at the surface and therefore registers any infiltration into the soil. This will be added in the paragraph.

274-5: Are the authors referring to the measured or modelled SMC here? Layer 3 is not shown in Figure 2. It's not clear which data is being discussed. It is mentioned in the following sentence that we are referring to the measured SMC. We will rearrange the two sentences to make this clear.

299-300: I think the authors should try to provide some interpretation of the poor fit in late June, late Aug and early Sep. Thanks for this suggestion. Including the sap flow measurements was not meant to be a quantification of transpiration, what we wanted was a qualitative comparison of the variability. We have now also updated Figure 7 as per request of reviewer #2 (see below), adding an envelope showing the range of the measured sap flow for the individual monitored trees.

307-8: What do the authors mean by 'This compares with the 352 mm of precipitation during the calibration period.'? All the ET values are larger than P. Will be reformulated. "This can be compared to the 352 mm…"

318-20: Is there some way to estimate E from these surfaces to see if they account for the missing amount? Yes, with some assumptions about surface storage on buildings/sealed surfaces and evaporation rates, this would be possible but we would prefer to keep the focus of this paper on urban green space.

324-7: In layer 1, SWC is generally higher at the beginning and end of the study period with fluctuations (but lower baseline) in between. Why is the water age relatively high in April/May and Nov? Maybe the April soil water is old from the previous winter, but November receives a fair amount of precip. Some explanation of the distribution of ages at the beginning and end of the study period would be helpful. In fall/winter much less water leaves the compartments, less transpiration, less evaporation, and there is mixing. You do see that when there is rainfall, the ages go down, but not as extreme as in summer when old water continuously leaves the compartments. We will make this clearer in the revision.

362-5: Would the authors recommend more, denser SWC measurements in urban soils? That is one option if resources are available. Another idea would be to integrate the measurements over the soil profile and create virtual layers from that within the model domain.

391: Spelling error – trees. OK.

396: It's unclear what '… generally capturing processes adequately…' means. What were the criteria for evaluating this? Visual comparison.
On line 397, 'generally good reproduction' is used. Can the authors be more quantitative,

e.g., measured and modelled isotope values were within x-x % of one another? We have presented KGE values for layer 1, but did not for layers 2 and 3 as there are lower dynamics. In the revision we will look to use RMSE or ME as an alternative.
It's unclear what is meant by '… this simulation required…'. Will reformulate

399: Similar to the last comment, '… somewhat less successful…' seems too vague. Can the fit be assessed quantitatively? (As above)

399-400: I recommend being more explicit about the number of wetting fronts that the isotopic results reflected. We will clarify that in revision.

Figure 10: This is a nice conceptual figure. Thank you.

Section 4.4: It might be worth mentioning that sometimes preferential flow pathways can form along impermeable-permeable surface boundaries. This would move water away from the area and potentially make it unavailable for green or blue water fluxes. Something to explore in future work perhaps. [I see the authors make this point later on line 454-6… excellent].

Section 4.5: Could the authors comment further on the role of grass in promoting green water fluxes? Many sustainability-focused landscape designers seem to be moving away from grass (or lawns), but perhaps the findings of this study are an argument in their favour. We do mention the study by Gómez-Navarro et al. 2021, which stated that a combination of turfgrass and trees can be beneficial in combating UHI. But we can definitely elaborate further on this topic, with respect to, for example, differences in shading and irrigation needs between trees and grass.

References

Kuhlemann, L.-M., Tetzlaff, D., Smith, A., Kleinschmit, B., and Soulsby, C.: Using soil water isotopes to infer the influence of contrasting urban green space on ecohydrological partitioning, Hydrol. Earth Syst. Sci., 25, 927–943, https://doi.org/10.5194/hess-25-927-2021, 2021.

Suggestion for new version of figure 7: