

Interactive comment on “Using soil water isotopes to infer the influence of contrasting urban green space on ecohydrological partitioning” by Lena-Marie Kuhlemann et al.

Anonymous Referee #2

Received and published: 13 November 2020

I largely enjoyed reading this paper by Kuhlemann et al. In fact, I thought it got better as I progressed through the manuscript. The paper describes hydrometric measurements, stable isotope variation and isotope-based estimates of young water fractions and transit times in three plots designed to represent different green spaces in the urban environment. I think that interest in aspects of urban hydrology is growing and that studies like this are needed to continually improve our understanding of processes in heavily human-impacted systems.

There are a few issues with the paper, but in total, I do not think these issues are dire enough to prevent the paper’s eventual publication. My suggestion is along the lines of

[Printer-friendly version](#)

[Discussion paper](#)



a “major revision”.

From an experimental design standpoint, it is unfortunate that replication is somewhat minimal. There is some within-plot pseudo-replication of certain measurements, but I remain a little hung-up on trying to assess whether the grass plot, shrub plot and tree plot are indeed representative of what one might come across in an urban space and whether the measurements and findings really do represent the breadth of variability that exists naturally. That said, there is a rich dataset here and I do believe that the authors can mostly move forward with what is written. I would suggest that the authority of the writing should however be tempered to match the lack of replication (and therefore the lack of understanding of heterogeneity or representativeness) and at some point, be more explicit in the paper about how replication with the necessary investment into these types of plot studies is not always feasible – and finally, how the lack of replication leads to some unknown uncertainty.

I found the general premise of the writing of the introduction and parts of the discussion/implications to be not as directly related to the work as I would hope. There is significant context given to climate change and irrigation, but I would suggest that the experiment does not hit squarely on either all that well. Irrigation in most temperate urban spaces is not much of an issue. Even if it is in the future, I’m not sure this work is directly transferrable to answering much about that. For climate change, the work does fit well with a drought scenario, but there’s no real “change” that is within the design of the study. Perhaps the more direct way forward is to couch the paper more about soil water dynamics in some typical urban greenspace areas under conditions affected by recent drought. This is mostly what is already here and thus, wouldn’t be too much of a pivot. The use of stable isotopes, especially through time, is quite novel, and the title of the paper does fit well with a somewhat differently focused introduction. It would not be too much a stretch to wait until later in the discussion to make the fuller climate change and future urban water management. Setting the scene this way up front just does not quite represent the work effectively in my opinion.

[Printer-friendly version](#)

[Discussion paper](#)



The results section is described in a pretty dense way, especially in its first half. It would benefit in readability if the authors tried to synthesize more toward the principal observations and let the figures partly speak for themselves, at least a little bit.

Finally, this is quite squarely an ecohydrological study, but the ecological part of that is somewhat lacking in the discussion. Are all grass-covered, shrub-covered and tree-covered soils in urban systems really expected to act according to the study's observations? What feedbacks might we expect in a changing climate for different vegetation covers? Do we know much about how species composition/community composition might impact on the observations from the study?

Some more specific comments:
 • Line 2: Is maintaining the water supply for green infrastructure really a particularly serious issue in temperate climate cities? In many circumstances, the purpose of green infrastructure is largely to help control too much water on the surface of the landscape.
 • Line 6: "effects" of? Vegetation type?
 • Line 25: is climate "breakdown" really a term in common use?
 • Line 25: abstractions, or extractions?
 • Line 117: The study description could use a bit more (couple of sentences) in terms of fundamental experimental design explanation.
 • Line 141: What silicon? This sentence could use some editing. Are you trying to explain that there was some sort of silicon septum on the bag?
 • Line 226: How relevant are stream water and groundwater in the context of this study exactly? The "experiment" is more a plot-scale experiment, so this seems to be a brief, but unfocused part of the study.
 • Line 271: This first discussion paragraph is a pretty long paragraph without a good central theme, but rather quite a few disparate points being made. Could it be broken up a bit to focus the main points better?
 • Section 5.2: I found this section quite interesting and well explained. It hits me that it would be nice to have an idea of field capacity in order to situate this a bit more closely with deeper percolation and eventually groundwater recharge. This might not be possible, but could it be inferred/estimated from the soil moisture time series maybe?
 • Line 378-381: For context to some of my earlier comments, I found this sentence to best situate the study

[Printer-friendly version](#)

[Discussion paper](#)



into a discussion of climate change. – Line 381-382: Something is oddly explained here given summer and spring are indeed 50% of the year. – Descriptions related to the word “depth” such as at line 412, but I believe maybe elsewhere: it is better not to use higher/lower in relation to depth. Deeper/shallower is easier to understand. – Figures and Tables: I think these are nicely done. I have a few comments. – Figure 3: it took me a minute to realize that u-norm is only relevant to the treed plot. I would suggest that the caption needs some editing to more clearly describe what is being regressed against what. Also, given that most of the p-values in the associated Table 5 are not statistically significant, is it actually meaningful to include linear lines of best fit for the insignificant relationships? – Figure 4: Though clear in the paper, the labelling of this figure could be more specific about the measurements being soil pore water in grassland, shrub, trees - or at least make clear in the caption. Otherwise, one risks quick readers thinking this is isotope information in water within grass, shrub or trees. – Figure 5 and 6: I think the heatmap is an interesting way of doing this, but I honestly would prefer to see the evolution of the soil profile through time, which I think is exactly what is shown in figure 6. I would like the authors to consider if figure 5 and 6 are too closely representing the same information and whether this should be collapsed into just one figure. Could the heatmap part of figure 5 not just be replaced with the entirety of figure 6 (keeping the top part of figure 5 still)?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-425>, 2020.

Printer-friendly version

Discussion paper

