

Authors' responses to the comments of Ryan Stewart

We appreciate your review and comments on our manuscript, "Detecting the occurrence of preferential flow in soils with stable water isotopes". Your feedback is valuable to us, and we will make the recommended revisions accordingly. We provide detailed responses to each of your comments below.

General comments:

This manuscript describes the results of using stable water isotopes extracted from different soil depths to detect incidence of preferential flow, using data collected seasonally from four catchments in Europe. The manuscript is well-written save for some editorial suggestions listed below, and the approach is an interesting contribution. The manuscript focuses a bit more on the validation and assumptions of the approach than on the scientific contribution itself, so having a better balance with the actually scientific contribution of the work would be something to consider during the revision process.

In terms of results and interpretations, the approach appears to work best in areas with distinct seasonality in the isotopic signature of precipitation (e.g., the Alpine catchment) and operates under the assumption that water moving via preferential flow has the same isotopic signature as the topsoil. This assumption does not cover all instances of preferential flow, as discussed starting around Line 310.

The approach also relies on the assumption that there is isotopic exchange/equilibrium between mobile and bulk matrix water (Lines 321-328). Another possibility is that mobile water could still be present (but not equilibrated) within the soil at the time of sampling. Water content at the time of sampling could be a way to distinguish between these possibilities, and it could be generally instructive to present the water content data if those are available.

Thank you for this suggestion. We will further discuss this in the revision, however we do not have water content data.

Another assumption (which I don't think was discussed) is that the reference profiles were not affected by preferential flow. The authors constrain what they consider to be reference profiles based on presumed seasonality in the isotopic signature of input precipitation, but it might also be possible to use a simple plug flow calculation or something similar to perform a back-of-the-envelope verification of the approximate depths those seasonal inputs would move given an assumption of effective porosity and rainfall depth (the latter perhaps minus ET).

Unfortunately, we lack the precipitation data of the year previous to drilling at those locations. Our approach aimed at filling this data gap with the reference profiles. We will make this clearer in the revision, that the reference profile creation was due to lacking isotope values in the rainfall.

The manuscript also does not discuss situations in which the water at depth has a depleted isotopic signature relative to the reference profiles, even though such scenarios 1) exist in the dataset, and 2) may reveal interesting behaviors about subsurface flow.

Yes, those instances are sometimes found in the data, however this was a really minor case. It might be that “older” winter water was transported there, but since we are not able to assess the origin of the signature (as it is not within the topsoil reference profile), we are unable to say which flow process transported it there.

With some revision the paper is a good candidate for publication in HESS.

Specific comments:

Line 2-16: Some minor grammatical issues here: 1) “may be quickly activated” sounds like it is referring to the soil matrix, 2) “and enhance infiltration or interflow”; 3) “profiles of stable water isotopes”; 4) “and selected those that matched...” 5) “heterogenous soils, many profiles”; 6) “flow pathways and highlight the...”; 7) “hillslope and catchment scales”.

We will fix that

Line 19: “water flows through... rather than the surrounding...”

We will change this

Line 22: This range of preferential flow (16-27%) seems very specific. Either provide the context or consider not putting these numbers.

It was stated in the publication but is indeed very specific, therefore we will remove the numbers in the revision

Line 25: “understanding” may not be needed twice.

We will rephrase

Line 38: “soil-layer, which”.

We will change that

Line 46: “formed”.

We will update the sentence

Line 47: Check your references that “Glenn V.” is needed for Wilson.

We will revise the citation to leave out the surname.

Line 53: 190 m is another number that seems very specific and context-specific. Can you at least express on a relative basis or something? Otherwise it doesn’t seem transferrable.

It was also stated in that publication but is also very specific, therefore we will remove the number in the revision

Line 58: Why is SSF only limited to 1/3 of events? Another specific rather than universal number.

We will also remove this number

Line 60: Velocities have already been discussed in L27-28.

We will remove this in the revision

Line 61-65: This section could be written more concisely.

We will try to make this more concise.

Line 85: GPR can also be used in a timelapse manner to visualize changes in water content during discrete flow events.

Thank you, we will add that

Line 102: no dash is needed between continent and effect.

We will remove this

Line 111: “making them an ideal”.

We will update the wording

Line 130: “and reach deeper”.

We will change this

Line 132: “the signatures shift”.

We will revise this

Line 140: This could be a place to elaborate on the validity/assumptions of deriving reference profiles from the dataset itself.

We will include a few sentences in the revisions where we elaborate on the validity

Line 163: add “for $\delta^{18}O$ ” after 1.7 per mille. Same for Line 166.

Yes, we missed that and will add it

Line 216: It seems you could solve this equation explicitly for f , which may not be that important but would better illustrate the analysis.

True, we will change it so it gets clearer what we are actually solving

Line 272: Interesting interpretation/context.

Thank you!

Figure 4: The shading for SD is faint (didn't show up in the printed copy I made) and would benefit from lines as in Figure 6.

We will change it to the same style as in Figure 6

Line 285: Might indicate that 65 and 35% are approximate values.

Yes, we will add that

Line 284: Suggest calling the variable "aspect", not "Aspectcos", and it indicates the direction (not just northerly).

We also calculated this for Aspect (Range 0-360 degree) which discerns between east and west facing slopes, where we found no effect, and used the cos of aspect to gain a metric to discern between north and south, therefore we will keep the name but also add some explanation on this

Line 305: Seems like these two sentences could be combined. Somewhat redundant currently.

We will write this more concisely

Line 317: "in both cases."

We will change the wording

Figure 9: Did you consider/try fitting logistic models to the individual sites? The response appears to be driven primarily by the Alps site, which makes sense as the one showing the most preferential flow, but also it is hard to see the different catchments with the points overlaid on each other.

Yes, we tried and still saw the effect, but stronger in the alps. We will add that to the revision

Line 318: "thus".

We will change this

Line 334-336: The comparison of the two stable isotopes is an important point, but one that wasn't emphasized until here near the end of the paper. It would be good to make this point early on.

We will shift this further up

Line 354-360: This paragraph is choppy in its structure and logic. I suggest rewriting.

We will rewrite it ensure more structure and logic.