EGUSPHERE-2025-3025

**Continuous *in situ* monitoring of a labeled-water pulse through a boreal Scots pine forest: vertical and horizontal fluxes**

Review Response

Reviewer 1:

Review of Marshall et al., for HESS

This manuscript describes a study in which a deuterium tracer was added to soil during drought, allowing for the opportunity to see the timing and spatial pattern of trees using that added water. Continuous isotopic monitoring capabilities were crucial to observing the breakthrough of that tracer in a set of trees. Their differential distances from the location where the tracer was added demonstrated a) the relative timing of later transport from through roots to each tree stem and b) the likelihood that numerous trees' roots were taking up water from the 1m2 location of tracer addition.

The results are simple and straightforward (with a few caveats described later), but the ability to conduct an experiment like this is a complex challenge. Consequently, it is a rare to have insights into a process such as this, and so I think this study will represent a valuable contribution to the field. While I find the discussion of the results to be framed in a way more appropriate to an ecophysiology audience than a hydrology audience, this work still fits within the scope of HESS. I enjoyed the writing style and I especially appreciate the blunt description of the challenges and mysteries; this sets a positive example that I would like to see more scientists follow.

**Thanks for appreciating the value and difficulty of performing this experiment.**

I think the greatest area for improvement would be to follow the suggestions of the first posted review, regarding more in-depth consideration of how to address mass conservation issues. I do not have specific suggestions beyond those.

**We have addressed the mass balance issues in the response to the other reviewer.**

While I know the borehole method has been tested and described in other papers, my concerns with the method pertain to the hole implying a severing of the xylem transport pathway. Is it capturing water in transit up the tree, or some signal that has diffused from active xylem into this less active wood storage (after being drilled)? By focusing on the tracer arrival timing, interpretations seem robust to this issue. However, it could explain the mass conservation failure. Can you elaborate on this further based on your experience in experimenting with this method?

**The question about hydraulic contact with flowing water in the xylem will be addressed in the discussion. In short, the xylem transport pathway is disturbed, but this disturbance is rather small in relationship to the entire sapwood area of the tree. The trees do respond to the wounding, but we have found in our experiments that this does not hinder the isotopic equilibration of the air passing through the borehole with the xylem water isotope composition of the tree. As you mention, the disturbance is theoretically worrisome, but it seems to work. This question also arises, in a smaller way, with other in situ tree xylem probes (e.g., Volkmann et al. 2016) and sap flux measurements, but they also seem to work. We now address this in the discussion (L493-496).**

71-72 The need for quantifying overlap is mentioned here. I'd like to see elaboration later in the discussion, within the context of this ownership hypothesis.

**We now discuss this on L97-98 and 421-440; however, we have modified the text to describe territoriality rather than ownership, noting that this may be in some sense a continuous parameter (see below).**

Figure 1 might benefit from having the scales in meters rather than degrees lat / lon.

**Agreed. Have modified.**

376-378 Inclusion of tree metrics would benefit the contextualization of findings. What are the crown sizes? Perhaps this could be shown in Figure 1.

**Unfortunately, we did not measure the diameters or crown widths of trees labelled in this experiment. We have been forced to rely on measurements of the stand characteristics made on plots nearby (L436-L440).**

423 Can you clearly state the "ownership hypothesis". If it's simply that there exists "a continuum between complete domination of uptake by a single tree, analogous to owned and defended property, vs. complete sharing of the benefit", I do not see how this is a useful hypothesis. It seems obvious that this continuum would exist and it is unclear how this hypothesis would be tested. I would hope to see a hypothesis rooted in what would drive the owning-to-sharing variation.

**Fair point. We have dropped the term ownership and replaced it with territoriality, which implies a weaker control. Rather than a yes-or-no hypothesis test, we now present it as a continuum between the extremes described above. We have also specified that we view our labelled tree counts as a measure of territoriality (L63-72), where the labelling of a single tree on a plot would be considered evidence in favor and the labelling of multiple trees would be considered evidence against. We have**

**also described reasons why sharing might be disfavored (water conservation and/or hydraulic limits, L424-426) on the one hand and why extensive sharing might be favored (mycorrhizal networks, L57-60) on the other hand. But even extensive sharing would presumably be limited by distance, where the cost of growing and maintaining roots would exceed their benefits at some point.**

429-439 (including Figure 7): More elaboration would be useful here. I struggled with precisely understanding the point.

**This has now been broken out into a separate paragraph (L442-450), where we argue here that our consistent N-S borehole orientation modified signal strength because of unequal tangential label distribution in the xylem. However, this does not seem to have influenced our labelled tree counts as our values are generally consistent with a study that used boreholes facing toward the labelled plot. Nonetheless, future work should use boreholes facing the labelled plot. We have reworked this text to make it clearer.**

Reviewer 2

This manuscript presents results of a field experiment to identify root water uptake using isotopically labelled irrigation. The measurement techniques are state of the art, which is to say that they are difficult to implement and interpret. The results are satisfyingly clear in terms of temporal behavior of isotopic composition of soil water and sap, and the experiment is thus a rare opportunity to quantify processes that are difficult to observe. The strength of the results lies in the spatial observations of tree water uptake, and the weakness is the mass conservation estimates. Some improvements in mass conservation estimation might reduce some of the considerable uncertainty that derives from recovering less than 5% of the tracer.

**Thanks for these supportive comments. We address the mass-balance questions below.**

L162 “UD” does not appear in Eq 1, and is later defined as something else (L167). Please move L168 to L162.

**Correct, we have fixed these problems on L160-175. Equation 1 estimates total water uptake and Eq. 2 estimates mean depth of uptake (UD).**

L171 what is the exact scope of this non-evaporation assumption? For example, do the berry bushes count in the reported LAI or not? Also, mosses are a nontrivial pathway for E (e.g., Heijmans et al. Global Biogeochem Cycles 2004). Finally, Fig 4b, 5a pre-labeling lce<0 and vertical profiles of 2H both suggest soil evaporation (though there are also other possible explanations for these conditions). If the flux tower data and pre-labeling soil isotopic profiles are not sufficient to resolve the soil/moss/understory(?) E flux component, some rough, literature-based or isotope-based estimates would be helpful for estimating biases introduced by assuming it to be zero.

**Thanks for this important comment. We have emphasized that the assumption at line 171 refers to our estimate of water losses at our uppermost VWC sensor, which was located 10 cm below the mineral soil surface. At this depth, there were roots of understory Vaccinium shrubs. The mosses and lichens have no roots so it is unlikely that they were drawing water up. To avoid raising concern, we have removed this sentence. Evaporation of rainfall directly from the mosses and lichens is another topic. Please recall that we injected the label below the moss layer and the bilberry canopy at the surface of the mineral soil. The injection was chosen over surface sprinkling precisely to minimize the evaporation. We return to the evaporation issue later in the discussion (L467-471) and when we discuss "recovery" in the supplement.**

What is the likely magnitude of downward unsaturated flux—i.e., to a zone below the soil moisture sensors? If unaccounted for, this flux would give upward bias to estimates of water consumption in upper layers—more bias the shallower the layer.

**We installed sensors down to 100 cm, but did not detect a change in VWC there (Figs. 3a and 4b). Likewise, the label was detected as deep as 70 cm at the end of the experiment, but was never detected at 100 cm. The transport to 70 cm may have been by either water vapor diffusion or unsaturated flow. However, the label intensities at 50 and 70 cm were so low relative to the upper horizons that we think that unsaturated downward transport to this depth was a minor proportion of the total. However, the clearest response to this question is that we never detected the label at 100 cm.**

Fig 1 shows 8 trees within 4 m of the injection. This is a tree-dense part of the forest. What does this mean for extrapolation of the results to the stand scale?

**Good point. 8 trees in 50 $m^2$ yields a density of approximately 1600 trees/hectare. The measured stand density on random plots was 1010 (sd =125) trees/hectare (Lim et al., 2015). So it is true that the labelling was placed in a tree-dense microsite. This was done to be able to reach several trees with the tubes that led to the analyser at plot center. We now recognize that the plot was not representative of stand density, but note that the observed root overlap was similar to estimates using $^{2}H^{1}HO$ on replicated, randomly placed plots elsewhere within the stand** (Henriksson *et al.*, 2021)**.**

L198 the whole-tree-bore equilibration method causes samples to equilibrate with all water along the hole, whereas the tree water might be dominated by recent uptake only at the outer few mm of xylem. Just as Marshall et al (2000) addressed this, it would be helpful to add some discussion of the likely magnitude of this effect in this experiment.

**Agreed. We have now reminded the reader of this, but have also pointed out that the outer rings have the largest cross-sectional area and support the highest flow per area.(L496-500)**

I think the Label Recovery section of the discussion more properly belongs in the results. Also, what are the consequences for the soil depth uptake calculations of accepting some alternative hypotheses about the fate of the unrecovered 95%+ of tracer?

**The co-authors have discussed the issue of label "recovery" and discovered that isotope hydrologists generally avoid this term (and this analysis) unless they can account for all significant fluxes. As we did not measure the evaporation flux nor water content or isotopes in the uppermost centimeters of the soil, we lack perhaps the major flow path. We have therefore decided to downplay the tracer recovery part and have moved it to the Supplement. We have described the calculation in more detail there.**

**The uptake depth was estimated from VWC only, not from the isotopic data. As explained above, we do not think that there was much evaporation from the 10 cm depth and do not see evidence of unsaturated flow in the deep soil layers. Nonetheless we have added some text describing these possibilities into the discussion.**

Fig 6 and its caption could be improved to explain the meaning of "days after labelling"—day of first observation? Also, isn't it more logical to swap the axes?

**We have changed the x-axis label to "Days until label detection." The axes were placed this way because we were interested in the slope of this line, which we interpreted as lateral transport rate.**

Are there no measurements of the monitored trees available? I expect differences in rooting and uptake might be related to things like relative crown size and sapwood area.

**Unfortunately, we lack estimates of sapwood or crown area of the labelled trees. We provide measurements of the stand characteristics made nearby. Fortunately, the stand is quite uniform.**