

Reviewer 3:

The manuscript "Beyond two water worlds: dynamic transpiration sourcing in a mixed-species boreal forest" presents a unique dataset of in situ measured xylem water isotopic signatures for two boreal tree species spanning two contrasting growing seasons.

The manuscript is well written, clearly structured and pleasantly concise. Overall, I would recommend this paper for publication after some minor revisions.

My main point of critique for the current manuscript would be the incidental treatment of the soil water isotopic signatures. Clearly, the isotopic signatures of soil water are of paramount importance to explain the observed xylem water isotopic signatures, yet, the authors did not report any soil isotopic signatures for the first year and only measurements from 5 and 15 cm soil depth for the second growing season. It is safe to assume, that both tree species' root systems are likely to exceed these depths.

We agree that more soil depths and from both growing seasons would have been valuable. We did not treat them as of paramount importance because we originally focused the study on comparing the precipitation to the tree stem and stream water. As noted above, we have now added a section describing in more detail the passage of the rainfall pulse through the system after the drought was broken in 2018. In that analysis, we emphasize that the soil isotopic data approached, but did not reach the values from the precipitation and xylem water. Evaporative modification along the path from precipitation to the upper soil horizons would more likely have enriched the isotopic signal, leading to higher values in the soil rather than lower. The soil data are more consistent with the notion that only about half the soil water was replaced by piston flow (see R2 above). We will present these issues in a new paragraph in the discussion. The new analysis integrates the soil data more effectively than before, but recognizes that there are limits to what we can do with the data in hand.

Furthermore, I am a bit sceptical about the use of BGDL 300 soil gas lances for the measurement of soil water isotopic signatures. Are there any previous publications that have used and validated these devices for the measurements of stable water isotopes? If yes, I would like to see a reference for that, if no, the manuscript should include some more details on the setup such as the material and dimensions of the BGDL 300. Did you also use heating cable for these lines? Did you do any comparisons to more established ways of measuring stable water isotopes of soil water?

This is true. We know of no previous publications using this device. However there are multiple publications describing similar soil equilibration methods. In any case, we agree that more detail is required and will provide it in the revision.

Looking at the results in Figs. 2 and 3, I see that after the drought ending precipitation event with -8.1‰ $\delta^{18}\text{O}$, which shifted the xylem isotopic signature of spruce xylem water towards values around -8‰ $\delta^{18}\text{O}$, somehow both of your observed soil depths did not exceed -9‰ $\delta^{18}\text{O}$. This would imply that spruce had to source nearly all of its water from above 5 cm of soil depths and that even 85mm of rain were not enough to flush the soil water isotopic signatures to a depth below 5 cm. Or could it be that your measurements of soil water isotopic signatures are somewhat biased?

Yes, they could be biased. In the additional text describing the breaking of the drought, we note that the probes gave unsteady results during and immediately after the heavy rains, but appeared to stabilize after this time. We speculate that liquid water may have flowed down along the probe surface. This question about piston flow in the presence of a litter layer and canopy interception has come in all the reviews. We will add a rough analysis of it, recognizing that the soil data are not limited. We emphasize again that the primary focus on the manuscript was on the xylem water and streamflow, as noted just below.

I am fully aware that the focus of this study was on xylem water isotopic signatures and the results indicate an impressive successful long term application of the borehole technique for natural abundances of ^{18}O . However, I think the soil water isotopic signatures within this study deserve and require a more detailed discussion. Do you trust the results? Can you recommend the use of BGD 300 soil gas lances for soil isotopic measurements?

R2 made some similar points. We will add more description of the soil results and of the device.

Apart from that, I have the following minor comments:

line 92: Please also specify the inner diameter and wall thickness of your PFA lines.

OK, we will add this info.

line 95: If possible, please specify the type and manufacturer of the solenoid valves.

OK, we will add this info.

lines 92-103: The description of the setup is a bit unclear to me: did you just draw atmospheric air from one end of the borehole through the tree and into the analyzer, or was there an additional dry air supply connected to the entry side of the borehole (as depicted in Fig.2 of Marshall et al. (2020))?

We will clarify in the text that we simply drew ambient air into the borehole trusting that it would equilibrate with the xylem water as modeled and empirically demonstrated in Marshall et al. (2020).

lines 103: You say you "focus on $\delta^{18}\text{O}$ because it becomes less biased during extractions than $\delta^2\text{H}$ ", but these effects are likely to be limited to cryogenic extractions, which are not part of your study. Volkmann et al. (2016) reported a mismatch between in situ $\delta^{18}\text{O}$ measurements with a CRDS compared to IRMS measurements of destructive samples, while $\delta^2\text{H}$ showed no such mismatch. Kinzinger et al. (2024, <https://doi.org/10.1093/treephys/tpad144>), using the same in situ methodology as Volkmann et al. (2016), focused their analyses to $\delta^2\text{H}$ because $\delta^{18}\text{O}$ showed a lower accuracy and higher drift. It would be interesting to know how the results for the two isotopes compare in your study. I would love to see a dual isotope plot of your in situ measurements (maybe as a supplement figure), just to get an idea on the capabilities of the borehole technique.

The potential problems with isotopic analysis occur not only with cryogenic distillation, but also with any sample that contains sufficient organic interferences. We have added a statement to this effect, speculating that this was what happened to our $\delta^2\text{H}$ data. We are aware that Herbstritt et al. (2024) observed no such interference in on-line measurements of garden vegetables and deciduous trees, but a conifer tree may be quite another thing. In any case, we have added dual isotope plots into the supplementary materials to show that the precipitation data look fine but the borehole data have their intercept shifted, consistent with the organic interference hypothesis.

Fig.3: As you mentioned that there were three repetitions for the depth of 5 cm - what is the grey line showing? The mean of all three repetitions? Could you also indicate the range of the three repetitions, or just show each of the measured time series with a separate line?

Yes, the grey line shows the mean of the three probes. Examination of the individual time series showed that one of the probes was the source of much of the noise. The noise occurs primarily on days with heavy rains, consistent with the idea that the probes might provide a preferred pathway for infiltration if installed incorrectly. We have dropped this noisy probe from the main analysis. However, we present all of the probes, including the noisy one, in a figure in the Supplementary Materials.

line 191: Is -0.02 actually the average streamwater SOI? In Fig. 4 it looks like the streamwater SOI is positive most of the time - or did you use a mass weighted average?

Yes, we used a weighted average and we now say so.

line 199: In my opinion, the dichotomy between "transpiration" and "infiltration" seems misplaced, since transpiration is also likely to be sourced from water that infiltrated into the soil. Maybe "(deep) percolation" or "ground water recharge" would be better terms than "infiltration".

Agreed.

lines 245-247: Speaking of models: Could you provide (or at least archive) the data presented in Figs.1-3 as well as additional site information such as a more detailed description of the soil profile and stand properties (basal area, leaf area index, any known information on root density distributions) and (if by any chance available) sapflow or dendrometer data in such a manner, that future modellers could use them in a quantitative way? Currently, the manuscript is missing a data availability statement.

Yes. We hope that these new data will be used by modellers and others in future work. Therefore we will provide and archive them in an open access depository. Previous work at this site has provided considerable data on soil and stand descriptions, which we will summarize in a table in the supplementary materials. We will also provide a data availability statement.

Herbstritt B, Wengeler L, Orlowski N. 2024. Coping with spectral interferences when measuring water stable isotopes of vegetables. *Rapid Communications in Mass Spectrometry* **38**: e9907.