

RESPONSE TO THE REVIEWER COMMENT OF JOHN MARSHALL REGARDING THE MANUSCRIPT “TEMPORAL DYNAMICS OF TREE XYLEM WATER ISOTOPES: IN-SITU MONITORING AND MODELLING”

We would like to thank John Marshall for the time he has taken to read our manuscript and his helpful comments to improve it. In the following section we are going to repeat the points brought up (in grey italic letters) and subsequently respond to them:

The model describes only one pathway from the root to a point in the stem. But the probe is 5 cm long, so there are a tremendous range of fluxes along that radial distance. Generally the flow rates decline whenever the cells become smaller in diameter (latewood of each tree ring) and as the wood ages, i.e., as one moves inward radially. I do not ask for a model that describes all of this, but I think it should be mentioned as a source of variation that the model cannot address.

We do agree that our simple conceptual model is not suited to explicitly account for the mentioned effects. This is reflected by its inability to properly transform δ_{RWU} into δ_{XYL} based on assumptions about the macroscopically determined flow path length distributions (as described by equations 6 – 9 in section 2.1.2). However, the alternative parametric equation (22) yields a remarkably good fit to the observations of δ_{XYL} . Even though the fitted shape and scale parameters do not have any obvious relation to physically observable properties of the system, we would say that the resulting apparent flow path length distribution implicitly accounts for macroscopic as well as microscopic effects.

Sapflux measurements are usually adjusted for the radial trends described in point 1 above. This is important here because so much is made of the comparison of rates. By the way, the depth of sapflux probes was not specified. Perhaps this would help explain some of the discrepancy. At least some recognition of this radial decline issue should be given.

The utilized probes measure at three depths (5, 17.5 and 30 mm) but our measurements did not show any clear decline of sap flux velocities along those three depths. We will add these details to the revised version of the manuscript. However, we do not directly see how this could explain the observed discrepancy between sap flux and tracer velocities – as the actually shallower penetration depth of the sap flux sensors could only be an explanation for the reverse case of sap flux velocities that exceed the tracer velocities.

In addition, the heat pulse method is one of the sapflux methods that was found to underestimate gravimetric sapflux in Steppe et al. (2010), who also used Fagus, by the way. The underestimate was not five-fold, but the sensor was not the same brand as this one either. In any case, the issue of calibration should be discussed when the methods are compared.

This is a really interesting literature suggestion, we have not yet been aware of. We will include it into our discussion. Indeed, Steppe et al. (2010) show a big discrepancy for heat pulse dissipation probes as used in our experiment, especially without wound correction (as in our case), where actual velocities were 3.7 times higher than the probe based estimates.

I have always wondered about whether the air leaving these probes is saturated. I was reminded of this question around line 313. I presume that the dry air addition is in part an attempt to prevent saturation, and the condensation that might result. Is that true? In any case, if the interior airspace is not saturated, then it does not seem sufficient to use the equilibrium fractionation to calculate liquid water isotope ratio from that of water vapour. If there is a temperature sensor in the probes, then it may be sufficient to calculate the saturation vapour pressure at the temperature and determine whether the water vapour concentration in the air coming out of the probe is at that concentration. If not, I wonder if a more complex model, accounting for kinetic effects isn't needed to infer the liquid values? I am not convinced by the text on lines 313-315.

Due to the comparably big surface area of the porous probe head and the small flow rates, the air within the probe head itself is indeed saturated as long as the probe is embedded into a "moist" medium. Volkmann and Weiler (2014) have tested this during summer time in the same region.

The dilution of the sample happens in the mixing chamber (B), not in the probe head (C), and consequently has no effect on fractionation. We revised Fig. 3 to highlight the physical separation of mixing chamber and probe head (see Fig. R1) and will make sure to highlight this distinctive feature of the probe design in the description in Sec. 2.2.3.

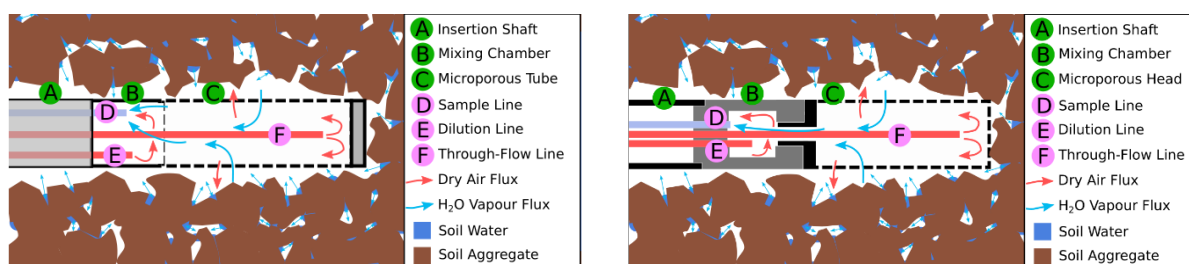


Fig R1: Left side: original version of the proe sketch. Right Side: revised version of the probe sketch with an improved depiction of the probe's specific components.

Table 1: Three cases of flow rate partitioning and the resulting effects on the humidity within the mixing chamber (where the sample is taken) and the probe head (where the equilibration happens). The first row represents a flush period, the second row represents a measurement and the third row has been experimentally verified but has no practical use case.

Flow rates [mL/min]			Humidity within probe compartment	
Sample	Dilution	Through-Flow	Mixing chamber	Probe head
-35	35	0	dry	saturated
-35	15	20	unsaturated	saturated
-35	0	35	saturated	saturated

Since we can assume equilibrium fractionation within our probe heads (see Table 1) and apply the same dilution rate to all our tree-, soil- and standard probes we are confident, that the H₂O concentrations measured by the CRDS can be used to calibrate the field measurements. As shown in Fig. A2, we are getting a fairly consistent relationship between the measured water vapor concentrations and the measured isotopic signatures of our standards. The biggest outliers are found right at the beginning of the experiment (light blue triangles), where we had some initial moisture problems within the tubing of the whole setup and for ¹⁸O between Jul-03 and Jul-11 (red triangles), where we had problems with the gas supply.

We acknowledge, that our calibration procedure has some space for improvements (see manuscript lines 537 - 542), but for the time being, we would abstain from including a more

complex calibration methodology which would certainly require precise and representative temperature measurements for each single probe head at different depths. This would most certainly be needed to make full use of the sub-daily measurement frequency, but regarding the points treated within this study, a more sophisticated calibration procedure would very likely end up with a rather unfavorable cost-benefit ratio.

Line 92: these were not logs, but trees cut from their root systems. They drew water up through the stems under tension derived from transpiration, as trees normally do.

We will correct that in the revised version of the manuscript.

Line 113: I don't think Jarvis did this with isotopes, did he? Clarify. Also, I think you have a "source strength," not a sink strength.

We will rewrite Sec 2.1.1 in order to clarify that the computation of δ_{RWU} is not a part of the Jarvis model and replace "sink strengths" with "source strengths".

Line 128: a soil layer's – will be corrected

Line 234: built – will be corrected

Line 386: any idea why a biofilm would change the equilibration?

We could think of a potential clogging of the membrane head's pores, which might affect the equilibration between the air within and the water outside of the probe head. Additionally, the biofilms might emit volatile organic compounds, which have been shown to influence CRDS-isotope measurements (West et al. 2010, Chang et al. 2016). Unfortunately, the CRDS that we used during that experiment was not logging the respective indicator parameters that could have helped to identify such organic influences on the isotope measurements.

Line 430: delete "of" – will be corrected

Lines 499 to 500: No, the model described in Marshall et al. determined that the water equilibrated within a couple of mm as it passed through the borehole. The issue, if there is one, is the opposite: the borehole vapour represents a thin layer of the sapwood near the outer edge, rather than the whole thing. In that sense, the probes described here may be a better integration of a greater depth. Suggest you say that.

You may have computed that the passage through 5 mm of borehole is enough to reach an equilibrium with the xylem water, but this does not mean that there is no further exchange with xylem water further along the borehole. According to Marshall et al. (2020), the borehole equilibration technique so far has only been applied to stems with diameters between 8 and 12 cm. At such small diameters, the ratio of conducting to non-conducting parts of the xylem can be expected to be quite different compared to more mature trees. Therefore, we would leave our statement as it is.

Line 512-518: it seems fair to add that the biofilms may have been favoured by the closed system. There was no such problem with the open boreholes, at least not that we knew about.

We will include this thought into our discussion (Sec 4.1).

Lines 569-574: this needs to be reworded. The last sentence is very important, but it means rather little as written. I think what you want to say is that the scaling error allows you to detect trends, but the values are not accurate. Then again, as noted above, I wouldn't place too much faith in the sapflux sensors, so I don't think you know whether they are accurate or not.

We will revise the whole section of the discussion and hope that it will gain in clarity.

Line 617-619: I disagree. If the labelled irrigation event were big enough to allow the xylem to come to steady-state for a longish time, then it should be possible to match d_{RWU} against d_{xyl} if there were no other problems with the method. The fact that you couldn't do it doesn't mean it's impossible.

In the next paragraph (line 620) we already mentioned that under the condition of little temporal variability of the RWU composition δ_{RWU} and δ_{xyl} , are similar enough to be treated as equal. And actually this condition seems to be fulfilled most of the time: Fig. 6 shows that there are only three periods (labeled A, B, and C) where δ_{xyl} seems to deviate from δ_{RWU} due to temporal dynamics. All of these periods follow sudden changes of soil water signatures or soil water availability.

We rewrote that part of the conclusion and hope that now it is clear, that we do not generally deny a relation between δ_{RWU} and δ_{xyl} , but we want to raise awareness for the possible discrepancy between the two signatures.

References:

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