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Comment on bg-2021-35

John Marshall (Referee)

Referee comment on "Temporal dynamics of tree xylem water isotopes: in situ monitoring and modeling" by Stefan Seeger and Markus Weiler, Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-35-RC1, 2021

Review of: ” Temporal dynamics of tree xylem water isotopes: in-situ monitoring and modelling...”

This manuscript describes one of the first attempts to use continuous on-line sampling of xylem water isotopic composition in three mature Fagus sylvatica (Beech) trees over the course of a summer. The soil around the trees was labelled with enriched water early in the experiment and the passage of the label through the soils and trees was monitored as new rains replaced the labelled water. All of this was modelled by a residence-time model that calculated flow rates along pathways of varying radial length and upward past three heights in the main stem. The paper is generally well-written and the model, in particular, is very clearly described.

I found several possible points for improvement or clarification:

- 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.
- 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.
- 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.

- 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.

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.

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.

Line 128: a soil layer's

Line 234: built

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

Line 430: delete "of"

Line 486: unattended operation is important! Well done!

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.

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.

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.

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.

Steppe, K., De Pauw, D. J. W., Doody, T. M., & Teskey, R. O. (2010). A comparison of sap flux density using thermal dissipation, heat pulse velocity and heat field deformation methods. *Agricultural and Forest Meteorology*, *150*(7–8), 1046–1056. https://doi.org/10.1016/j.agrformet.2010.04.004