

## **Author responses to Reviewer #2**

We thank the reviewer for his insightful comments that helped to refine the manuscript. Below, we respond to each of the specific comments (orange).

Line 24: I'm not sure what the authors mean that 'diurnal variations largely offset each other at the daily scale'? Does this indicate the daily mean doesn't change much over time? Relaxing to a well-mixed background isn't necessarily indicative of an offsetting flux. Just think about whether this idea could be clarified.

We rephrased this sentence.

Line 95: Authors give the outer diameter (OD) as ¼ inch. Please provide the inner diameter (ID) if possible as it can vary.

Unfortunately, this specification is not available for the tubing as it was not measured independently.

Line 126: "As no significant variation was observed..." This can read as no mixing ratio-dependent isotopic variation was observed, but I think the authors mean that no temporal variation in the mixing ratio-dependent isotopic variation was observed. Please consider editing for clarity.

We rewrote the sentence to make this clearer.

Line 133: Eqn 1 can be described as a hyperbolic-linear function to help the reader picture the shape of the mixing ratio dependence.

We added this information.

Line 138: Are there any statistics to share from the quality control?

We added: "The standard deviation of the calibrated quality control standard over the experimental period (n =41) was 0.01 ‰, 0.02 ‰, 0.1 ‰, 5 per meg and 0.1 ‰ for  $\delta^{17}\text{O}$ ,  $\delta^{18}\text{O}$ ,  $\delta^2\text{H}$ ,  $^{17}\text{O}$ -excess, and d-excess, respectively."

Line 226: Eqn 6. I urge the authors to consider the variables they use here. They are making a distinction between their method and that of Salamalikis et al. (2015) which used residence time weighting, and the authors use fractional contributions. The tau symbol is the commonly used to denote residence times, so using it as a fraction is likely to cause confusion for readers not paying close attention. Consider using a different symbol for the fraction ('f' perhaps).

We thank the reviewer for noting this and changed the symbol as suggested.

Line 248: "The turbulence coefficient has been adjusted to 0.33..." Do the authors mean it was fixed as a constant value to match observed d-excess?

Yes.

Line 275: awkward grammar "stomatal conductance in oaks cope with"

The phrase was rewritten.

Figure 2: panel (a) labels are difficult to read in my print version.

The resolution of the figure has been increased.

Section 3.4: This paragraph feels lacking some context for the reader. I realize the authors discuss these results later in the paper, but in this brief section, key elements of Figure 5 are not introduced, e.g. cloud and evaporation dominated. Without adding too much, foreshadowing the usefulness of this diagram would help readers unfamiliar with the Graf et al., 2019 framework.

The framework of Graf et al (2019) is now explained.

Line 385: dominated -> dominant

Changed.

Line 547: The discussion of comparison with Baldini et al. 2008 feels like it flips from earlier to here. Earlier it contrasted, here it supports. Maybe I'm not following.

You are right. We rephrased the sentence to clarify this.

Line 570: I personally think 'strong potential' may be an overstatement given the evidence in this paper.

We removed the 'strong'.

Table A2: Authors provide statistics on the parameter fits, but not statistics on the fits themselves. How well does these fits match observations? They could consider sharing statistics of the model fit residuals.

We give now the coefficient of determination and the root mean square error of the functions.