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Interactive comment on "Leaf-scale quantification of the effect of photosynthetic gas exchange on $\Delta^{17}O$ of atmospheric CO_2" *by* Getachew Agmuas Adnew et al.

Anonymous Referee #1

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As far as I understand the justification for the great effort required in measuring 17O and its "access" (or anomaly), is the discovery of significant mass independent oxygen isotope effects in the stratosphere that is conserved to some extent in the troposphere (seems to be true both for atmospheric O2 and CO2). The extent to which this anomaly is conserved in the troposphere depends on the CO2 (or O2) cycling through the biosphere, which erases it by exchange with water. Thus, if the stratospheric production of the anomaly is known and it is relatively constant, the residual signal in the troposphere should reflect the biosphere productivity (GPP). This is exiting application considering the uncertainty around GPP.

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However, ALL the processes associated with the Biosphere, including leaf gas exchange studied here, seems to be mass dependent and are FULLY covered by the conventional 18O studies. The only exception may be the small variations observed in the lambda factor that define the expected ratio of 18O to 17O mass dependent discrimination ($\sim$0.5), which is not studied here. And so, while the present paper goes through an impressive exercise of gas exchange and isotopic measurements and calculations, I fail to see the purpose and merit of this exercise, beyond a test that verifies that indeed the 17O measurements are consistent with the 18O studies. The occlusions as much as I can see are already fairly well-known form 18O studies and, in fact, much of the calculations here still depends on the 18O measurements.

For example, the key results indicated in the Abstract are: "Our results demonstrate that two key factors determine the effect of gas exchange on the D17O of atmospheric CO2. The relative difference between D17O of the CO2 entering the leaf and the CO2 in equilibrium with leaf water, and the back-diffusion flux of CO2 from the leaf to the atmosphere, which can be quantified by the Cm/Ca ratio". Isn't it that these 'basic principles' of leaf gas exchange are already fairly well known from previous CO2 and the 18O studies?

It seems also that the notion of "discrimination against D17O of atmospheric CO2" is not clear. If this is confused with D in leaf photosynthesis as for D18, then again 17O is predictable and has no clear additional information (other than perhaps the reflection of the possible variations in the lambda factor). The final estimate of global 17O discrimination anomaly is back of the envelope calculation based on these known principles and literature values. I am not sure what new insights are provided.

And so, while the experimental setup, measurements, and going through the isotopic theory are impressive and seems to be well done on first look, I think the authors have to re-think the presentation and provide a better justification of what in these measurements takes advantage of any mass independent effects (as declared), and in what ways this goes beyond a sophisticated confirmatory report.

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