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Discussions

Interactive comment on "The ^{18}O ecohydrology of a grassland ecosystem – predictions and observations" *by* Regina T. Hirl et al.

Anonymous Referee #2

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Hirl and coauthors present an impressive data set of seven years of isotopic observations in a grassland and an equally impressive modelling effort of the data.

The interpretation of the data is regrettably only discussing the isotopes and gives very little insight into the water fluxes of the ecosystem.

For example, if main water uptake is always at 7 cm depth even when this layer falls dry, then ecosystem transpires probably less than possible during this times because it would have access to more water in deeper soil. How is the ecosystem reacting? Is it shutting down the stomata? Is it changing its carboxylation capacity and stomata close thence? Or both? And why would a grassland do this? I guess it is well established in trees that they would harvest deeper soil water.

Are any of the other variables telling me something about the ecophysiology of the plants or the ecohydrology of the ecosystem? Are leaf water isotopes telling me something? They tell me at least that there is nighttime conductance. Is there also nighttime transpiration? Anything else?

I think, therefore, that the claim in the conclusions that the "work highlights the usefulness of mechanistic 18O-enabled modelling for explorations and quantitative analyses of the ecohydrology of ecosystems." is premature because only point (2) of the three points, i.e. root water uptake is actually ecohydrology of the ecosystem. The other points are about 18O ecohydrology, as the title of the paper suggests.

I have to admit that I had problems with the sensitivity analysis. Firstly, the mean difference is not a good measure. Differences can cancel out even when the model reacts strongly to a change. Most people use variance, standard deviation or root mean square error to avoid this. I guess that would be something like the error bars in Fig. 6. Secondly, one can of course use "arbitrary" ranges of model parameters to look at the output range, but then one cannot compare anymore the output ranges between the different parameters as done in Fig. 6. One wants to disturb each parameter similarly. So a derivative would probably be a good idea, or an elasticity.

Lastly, the authors suggest that there is no Péclet effect but rather a second unenriched water pool. While the data seem to support this, I would have expected a much better discussion.

I cannot find any mentioning of the 2D formulation of Farquhar and Gan (2003) while this should probably be the correct model. For example, what would be the effect if the leaf followed exactly this 2D model but the leaves were sampled only partly, not sampling the least enriched part ?

The very small discussion starts with the possibility of xylem (or associated tissues) water and non-steady state but then only talks about the latter. I would have loved to see insights about grass blade anatomy, especially from this group who knows it that well.

I also do not follow the argument that there is no non-steady-state effect in the missing correlation with transpiration because the model includes non-steady state. The model yes, the data no. Margaret Barbour's group also claimed to see no Péclet effect but if they plotted their data against the isotopic composition of transpiration rather than xylem, the Péclet effect re-emerged.

The data sampled 7 species while the model describes one mean species. What is the effect of this? Could an averaging of different leaf dynamics not lead to the observed missing correlation with transpiration?

Overall I compliment the authors on this very nice data set and the very careful modelling, and wish to see the paper published soon.

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