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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.

Matthias Beyer (Referee)

matthias.beyer@bgr.de

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Thank you for letting me review the manuscript 'The 18O ecohydrology of a grassland ecosystem –predictions and observations'. I enjoyed reading. In their work, the authors apply an 18O-enabled soil-plant-atmosphere transfer model in order to predict the dynamics of δ18O in soil water, the depth of water uptake, and the effects of soil and atmospheric moisture on 18O-enrichment of leaf water in a grassland in southern Germany. In particular, they investigate the propagation of the δ18O signal of rainwater through soil water pools, root water uptake and 18O enrichment of leaf water by tracing, predicting and validating δ18Osoil, δ18Ostem and Δ18Oleaf. Finally, the authors test two models for describing Δ18Oleaf at the canopy scale (the two-pool model or the Péclet model) and evaluate their performance.

Without doubt, this manuscript is well-prepared and written. The structure is clear, research questions are stated concisely, and the introduction provides a thorough overview on the topic. The graphics are suitable and well illustrated. I also agree to the authors that the model results are promising. The applied model MuSICA definitely seems capable of simulating ecohydrological processes including water isotopes. In my opinion, the hydrological and ecological community definitely needs a more integrated approach in modeling and investigating, and MuSICA seems a promising approach to that. I do not have major criticism on the manuscript, but a number of questions and comments that should be addressed in a revised version.

In summary those are: - In general, I find that the discussion of the results needs to be more critical. Yes, the results are good for an uncalibrated model. BUT: Grass is (sorry for saying that) probably the simplest plant to model (homogeneous and short roots). Looking at the isotope results, the 20cm depth and also under dry circumstances does not really fit well – see R^2. Hence, I would appreciate a more critical discussion, you have to highlight also the weaknesses that certainly still exist. Also, a total water balance is always a good means of validation and would be nice to have.

- The results section contains a lot of discussion (see detailed comments) - Why was model not calibrated?

- Why was 2H not used? How was fractionation evaluated without 2H - did the authors simply use the offset of 18O from the LMWL? Is the model capable of modeling 2H as well? The dual-isotope space enables a more comprehensive understanding of processes. Also, it is more sensitive compared to 18O and since the authors did a sensitivity study, perhaps very useful. I don't say I expect that in a revised version, but I am interested on the authors opinion on that.

Having that said, I suggest minor revision. I am looking forward to see the manuscript published in HESS.

Detailed comments:

Abstract l.20: grazing pressure, but how about rooting depth? Grasses are shallow-rooted so any other uptake is not expected?!

l.20: respond to atmospheric moisture. . ..does that mean leaves take up moisture from the atmosphere? (foliar uptake???)

l.21: two non-mixing pools. . ..is that realistic or justified?

l.26: the second sentence is not well written/unconcise

l.29: explain better or provide citation – explain why do leaves fractionate

p. 2 l.14: 'source water' for plants would be soil or groundwater, but not xylem water as it is plant water already

p. 2 l.15/16: 'summer' and 'winter' should be related to the particular study area, these statements are not true for the whole earth. . ..

p. 2 l.29: 'enrichment above . . ..' I know what you mean but this is written ambiguous – stem water can also be subject to fractionation under certain conditions. It should be more clearly expressed what is meant with this sentence.

p. 2 l.31: 'many authors' – could you provide some citations, please?

p. 3 ll.2-14: this is well written!

p. 3 l.15: is this relevant for grasslands only?

p.4.l.5: please review this sentence and provide more information. . .which species, which soil depths, what exactly is meant with 'growing season'

p.5.l8: though you cite a paper on the cryogenic system you use, it would be nice to specify temperature and extraction time here

p.6.ll. 1 & 2-7: These information belong together, I'd suggest to either put the first part down or the second up

p.7 l. 33: based on what was the beta distribution assumed (based on previous research or citation) p.10.l 2: Why does the ratio need to remain 1.6?

p.10.ll. 4-6: Perhaps that fits better to 2.4.1 isoforcing

p.10.l21: Was predicted soil water content validated somehow?

p.11. l 29: in the way that (word missing)

p.11: paragraph 3.4 contains a lot of discussion, I suggest reviewing and removing some of the 'judging' (e.g. last sentence or l.29/30)

p.12.l.21: MLR does not appear in the methods/statistics

p.12.l.23: weakly significant? I think this should be rephrased → significant or not

p.12. paragraph 3.5.: the authors mix VPD and relative humidity quite a lot here, which makes this chapter hard to read. I suggest restructuring and rephrasing of this chapter (though the results completely make sense)

p.13l 4-10: Discussion

p.13. l.26-32: This sounds more like a conclusion

p.14. l.5: quite

p.14. l.6-7: suggest rephrasing: 'likely result from sampling effects and analytical error'

p.14. l.12-23: I agree, but also it should be clear that grass with a fairly uniform uptake depth right below surface is probably the easiest of plants to model. This is not a criticism but would be interesting how the model performs for different plant types.

4.2: I am not sure if this deserves an own chapter. I believe that it is true that the grass takes the water mainly from the upper depths but considering the characteristic shape of soil water isotope profiles at the surface (enrichment and subsequent decrease of isotope values towards a constant value), the used resolution of only 2 depths might not reveal true uptake patterns. Also see Rothfuss and Javaux, 2016.

p.15. l.26-27: ‘online transpiration isotope method’ this appears here for the first time?

p.16 l.9-11: I like this chapter, but the last sentence does not make sense – why compare and justify grass species with a study on non-grass-species?

Conclusions: An experienced and known Professor once gave me the advice ‘A good paper doesn’t need a conclusion – the reader draws it him/herself.’ The authors should decide themselves, but I feel emphasizing some key points in the manuscript/abstract a bit more would be sufficient without conclusion.

Fig. 3: As stated above, the model does not work that well for 18O. I think this needs to be discussed thoroughly

Rothfuss, Y., Javaux, M., 2016. Isotopic approaches to quantifying root water uptake and redistribution: a review and comparison of methods. Biogeosciences Discuss. 1–47. https://doi.org/10.5194/bg-2016-410

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