Thank you for letting me review the manuscript hess-2019-329 ‘Coffee and shade trees show complementary use of soil water in a traditional agroforestry ecosystem' by Muñoz-Villers et al.

I enjoyed reading. In their work, the authors investigate water uptake depths of large shade trees and coffee trees during two dry seasons and one wet season using water stable isotopes and a Bayesian mixing model. They find that coffee and shade trees show complementary water use patterns, i.e. preferential water use by coffee and deep-water use by the shade trees. During the wet season, both groups shift to shallower resources.

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. I like the study and the topic is interesting. Also – and this is the main scientific contribution of the paper – it is great to see that the authors integrated priors and used root information and macronutrient distributions for that. However, apart from that, the novelty and innovation of the study is limited. I also have a couple of rather major concerns about the methods used in the paper related to the soil water extraction and mixing model. The former might be answered, but the latter might require some more effort. I elaborate on those below. Minor comments are summarized further down.

All the best,

Matthias Beyer

Major points:

l.249-250: was complete extraction somehow validated? Also note that clay-rich soils need higher extraction temperatures (see recent (Gaj et al., 2017; Orlowski et al., 2016) papers on mineral mediated isotope fractionation). Using a water bath at 100°C might result in an offset in isotope compositions and lead to errors/uncertainty in the mixing model (the reservoir of water that is extracted would not equal the reservoir that is available to plants). The authors state at one point that there was an offset of the values towards more depleted – this is exactly what would happen and was observed in other studies when clay was an issue. This issue should be at least discussed. Another question (but this is more general) related to the cryogenic extraction is why such long extraction times are needed (I know, West et al. 2006 propose that). I think one part of that is related to the relatively low extraction temperature, but still. The extractable water should be leaving the sample side very fast given the low volumes (even under 100 °C) – waiting longer would not evaporate more water from the sample side unless the temperature is increased further.

l. 297-300: These assumptions need to be validated/proven. Why was not the soil water isotope composition of the first 5 cm used directly? I guess in order to account for water that was taken up by the plant before the actual sampling date? How was the classification used for the mixing model decided? Slightly above and below the zero-flux plane, the isotope composition of soils normally changes drastically during dry periods…for clay this is often in the first 15 cm soil depth. The 30 -120 cm depth were isotopically similar? In my understanding, the discretization used in the mixing model should be done after the isotope depth profiles are evaluated and backed up by statistical measures of differences between different depths. After checking the supplementary data, I’m really doubting the discretization used. There are partially huge differences of the isotope values of the soil profiles between 30 and 120 cm. And how about 15-30 cm? – was the isotope information of this depth not used at all? (in that case, the mixing model is missing a source which violates the mixing model requirements). I refer, once again, to the Rothfuss et al. publication, which might help to address these issues.

Minor points:

- Since many different analysis were carried out with the soil and plant samples, this could be summarized in a table nicely.
- It would have been easy and interesting to check the uptake depths of the large trees separately and not lumping them. (but maybe not of interest for the study)
- I suggest strong discussion of the use of informative priors and putting a more general focus on this aspect, as this is the key scientific/methodological novelty in this paper in my opinion.
- (more a comment): It would have been interesting to have water potential measurements in both soils and trees, because those could really constrain the possible uptake depths.

Abstract

l.27: Providing the rainfall amounts in addition to the year would be nice; in addition, it would be nice if the authors could state the type of environment of the study (e.g. semi-arid, tropical,…)

ll.35/36: the percentages are the mean? median? I suggest adding a +/- xx % notation accounting for uncertainty

l.39: short-term wetness status? Do the authors mean that the uptake depth is not influenced by small rain events? This sentence is not easy to understand, I suggest rephrasing

ll.39-41: this sentence needs to be rephrased. The terms near surface vs. much shallower are confusing the reader (5 and 15 cm are both shallow). Perhaps 'upper five centimeter'?

ll.42-43: the spatial segregation mentioned, is it due to the different rooting depths of the studied plants? Was this validated somehow?

l.44: plant-soil water uptake? Confusing phrase. Do the authors mean 'root water uptake patterns/depths'?

I feel like a concluding sentence is missing in the abstract. What are the implications of the study? What novel things were found out?

Is 120 cm the max. rooting depth???

Uptake depth vs. rooting depth? (coffee shallow, others deep)

Introduction

I really like the way the introduction is written (clear and concise). The Bayesian mixing model needs to be addressed though. The word is only mentioned once, and some readers might not know what it even is. At the end of the introduction, sentence is missing highlighting the importance and novelty of this research.

l.55 ~~and~~

l.73: 'soil resources' sounds odd…can the authors specify please?

l.87: ~~However~~,

l. 90-92: please note that mixing models are also frequently criticized, (Rothfuss and Javaux, 2016)

l.92: 'Although rarely implemented' – do the authors have examples where it was implemented? (this is out of interest)

l.143: micrometeorological measurements (which)

l. 146: nice the authors are implementing priors. See related publication where this was suggested (and also MixSIAR was used): (Beyer et al., 2018). You don't have to cite us but maybe it helps for some explanation in the authors manuscript.

l.151/152: The answer to question no. 2 is not reflected in the abstract

Materials/Methods

l. 168: ~~on~~ an; is there no data after 2000 for rainfall? This seems like it's likely to have changed meanwhile

l.214: 'carried out' rather than 'performed'?

l.218-222: how many replicates per individual were taken? (same later for coffee and the soil samples)

l.232-233: 'Auger sampling points were located so that each of the sampled shade trees and coffee plants had a total of three soil sampling points within their 3 m radius.' – If it was sampled at only three different locations (see sentence before), so it means that all the trees had the three sampling points in their 3m radius? That seems odd. Can the authors please check if this phrasing is correct here?

l.247: refrigerated – was any mold developing on the samples? This can affect isotope ratios

l.268/269: What is API – if it is not a common method, it needs to be explained briefly.

l.304/305: It would be very appreciable to the community I believe if the authors explain how the priors were determined and implemented into MixSIAR as this is not something that has been done often.

Results

l.321: I see a point in putting this as result, but this is nothing that belongs to the objectives of the study as such. I suggest including it into the methods chapter. In many hydrologic and soil studies variables such as rainfall and soil moisture are the basis and not highlighted as results.

l. 335: Definition of normal vs. below-average dry season: In fact, both dry seasons sampled were below average, 2014 was about 20% lower (323 mm vs. 389 mm normal) and the 2016/17 one 40%....not sure if I would consider 20% below average a 'normal' year.

l.351-353: it is not surprising that the wet season is wetter the dry season, but it is notable that the wet season is drier than the 2014 dry season! Why is this information omitted?

l.353: the API results don't tell the reader anything without proper explanation

ll.359-360: two digits after comma reported for 18O – more than precision – should be avoided; add 'for' delta 18O, 'for' delta 2H

l.382-384: because of the effect of clay material on extraction? (see comment before) – same for ll. 387-388

l.417: the root biomass cannot be distinguished between species, right? (coffee vs. large trees?)…that means that the created informative prior would be quite biased….

ll.432-436: discussion

Putting the rainfall amounts in the results section is debatable…it sure is something that was done during the study, but it is not directly related to the objectives. As Hydrologist, I personally would've liked to read these numbers earlier to put the words 'dry season', 'less than average' etc. in perspective.

Discussion

Ll.522-525: So in the wet season both trees and coffee use shallow water, because it's abundant. In the dry season, the trees use deep water – because they have deeper roots and water in deeper soil is easier accessible (low matric potential of soils). The coffee uses shallower water in the dry season. What is the reason? – the fact that coffee plants cannot grow deep roots? – or is it because they don't need so much water compared to the trees and don't need deep roots? – or, because the coffee plant has another strategy and its roots can extract water from drier soil compared to tree roots? or……. This is not a criticism; this question is out of interest. I wonder then, if this is really 'complementary' water use as such?

ll.599-600: Which recommendations based on their results would the authors give to coffee producers then? This would be a nice addition.

ll.606-612: this is a bit contradictory, because in the presented example using this additionally information did not affect the results much (both uncertainty and general outcomes). So which variables should be included in the future? Are there others that might be more suitable? Micronutrients? Soil moisture?....

Conclusions

An experienced and well-known researcher a while ago gave me the advice: 'A good paper does not need a conclusion chapter – the reader draws them him/herself.' That stuck to me somehow. I think this is a good paper.

Beyer, M., Hamutoko, J.T., Wanke, H., Gaj, M., Koeniger, P., 2018. Examination of deep root water uptake using anomalies of soil water stable isotopes, depth-controlled isotopic labeling and mixing models. J. Hydrol. (accepted Publ.

Gaj, M., Kaufhold, S., Koeniger, P., Beyer, M., Weiler, M., Himmelsbach, T., 2017. Mineral mediated isotope fractionation of soil water. Rapid Commun. Mass Spectrom. 31, 269–280. https://doi.org/10.1002/rcm.7787

Orlowski, N., Breuer, L., McDonnell, J.J., 2016. Critical issues with cryogenic extraction of soil water for stable isotope analysis. Ecohydrology 9, 1–5. https://doi.org/10.1002/eco.1722

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