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Interactive comment on "Tracing ecosystem water fluxes using hydrogen and oxygen stable isotopes: challenges and opportunities from an interdisciplinary perspective" *by* D. Penna et al.

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I am happy to read that Daniele Penna and others attending the workshop "Isotope-based studies of water partitioning and plant-soil interactions in forested and agricultural environments" share the results of their discussions in the submitted commentary ("Ideas and perspectives" as called in Biogeosciences). They provide an adequate overview of the current state regarding recent developments in the application of stable isotopes as a tracer in biogeosciences and discuss also limitations that we are facing at the moment. I like how they frame these limitations as new perspectives and research opportunities to put a positive spin on these challenges.

I also welcome their aim to promote interdisciplinary research using stable isotopes. The commentary is in the scope of the journal and I am sure it will be of interest to a broad community using stable isotopes. I suggest publication after minor revision and I provide some critical comments below. Please note that the provided references are of course only meant as suggestions and that reference to my own work are mainly provided to underline my arguments given in the general comments.

General comments

I think that the commentary could be stronger if the authors would revisit some parts, as the structure is not very consistent. For example, some parts in Section 2 would fit better to Section 3, as they deal with limitations, while several paragraphs of Section 3 read like a review and would fit better in Section 2. I provide example of these paragraphs in the specific comments.

I would also encourage the authors to acknowledge recent developments that deal with their suggested research agenda, when asking for isotopic variability of groundwater (as done by Scheliga et al., 2017), promoting tests of isotope analysis for vegetation samples (as done by Millar et al., 2018), analysis of spatial variability of soil water stable isotopes (as done by Yang et al., 2016), suggesting dual-labeling studies (as done by Bachmann et al., 2015), incorporating evaporation fractionation in catchment models (as done by Knighton et al., 2017, Smith et al., 2018, and Kuppel et al., 2018). While these studies were partly published recently, I still think it would be worth looking into them and considering including them. This would provide examples of developments that might go into the direction that you are suggesting and help the reader to see what is currently being done and tested.

One comment on the "n water world": The "Two Water Worlds" were suggested based on the findings by Brooks et al. (2010) that soil waters extracted with suction lysimeter or cryogenically have different isotopic compositions. Thus, this definition of a split into two subsurface pools mainly stems from the limitation to not be able to extract water held at different pressure heads. I obviously agree that the subsurface is a continuum of varying pressure heads (P6 L23). However, we are currently lacking the means to sample the isotopic composition along this continuum (water retention curve), but efforts have been made to compare different methods sampling waters of different mobility with different methods (see Figure 4 by Geris et al., 2015 and the work by Orlowski et al. (2016)). The relevance of different pressure heads for the TWW has been discussed earlier by Berry et al. (2017). The point that I want to raise here: Of course, it would be neat to be able to sample a "n water world", but is it really practical? Instead, the TWW is relatively loosely defined into "mobile" and "less mobile or tightly bound" waters. However, from a stable isotope perspective (on which the TWW is based), we can only distinguish between "mobile" and "bulk" soil water given our limitations in the sampling procedure with either suction lysimeter and cryogenic extraction/direct equilibration, respectively. I tried to convey this message with my co-authors in Sprenger et al. (2018b). Important with this regard is that i.) the relative contributions of "mobile" and "more tightly bound water" is temporally variable (Figure 5 in Sprenger et al., 2018b) and that ii.) the "mobile" water does not reflect at all the total plant available water, but plants can access more tightly bound waters than the suction cup lysimeter (Figure 1 in Berry et al., 2017 and Figure 1 in Sprenger et al., 2018b). There is further a lack of clear definition distinguishing the two water pools.

Specific comments

P2 L11: I am not sure if a report (2030 WRG, 2009) sponsored by "The Barilla Group, The Coca-Cola Company, The International Finance Corporation, McKinsey Company, Nestlé S.A., New Holland Agriculture, SABMiller plc, Standard Chartered Bank, and Syngenta AG" is an appropriate reference.

P2 L 29: To my understanding, it is not only that the laser-based instruments are more affordable, but also running it for analysis is cheaper and easier.

P3 L2: I think that the context is missing here. One might ask "which well-mixed conditions"? While I believe to know what you refer to, you aim to reach a wider audience in Biogeosciences and thus, should be clear about the simplifying assumptions.

P3 L25-L29: This reads more like "limitations and challenges" and is an example for what I mean that the structure could be improved.

P4 L17-L19: Again, when you start a sentence with "One limitation. . .", this might fit better in section 3 of the manuscript.

P4 L19: What about carrier gas issues like CO_2, which potentially cause issues for CRDS (Gralher et al., 2018)? Beside consequences for the direct-vapor equilibration method, this would also be relevant for in-situ measurements via the vapor phase.

P4 L23: You mention soils, tree stems and leaf as examples. What about ET-partitioning as done for example by Wang et al. (2010)?

P5 L6: I think that the comparison and review study by Rothfuss and Javaux (2017) is a more appropriate reference here.

P5 L10: Given the methodological issues with the calculations of source waters by Evaristo et al. (2015), as revealed by Javaux et al. (2016), this might not be a good reference.

P5 L18: Consider including Zhao et al. (2016) as reference, who compared also different methods for xylem sampling.

P5 L20: Be more specific: What do you suggest to the community? What do you "aim to analyze" (- depends on your research question, or not)? How is this message different to the section 4.3 in the commentary by Berry et al. (2017)?

P5 L26: I am not aware that McCutcheon et al. (2016) studied the stable isotopic compositions among different pore spaces. Instead, they specifically stated that "We are not able to determine if pore-scale variability can impact isotopic composition of root-absorbed and draining water".

P5 L31: How about evaporation through the bark as studied by Martín-Gómez et al. (2016)?

P6 L7: Do you think numerical simulations can help assessing such ages, as done by Brinkmann et al. (2018) and Sprenger et al. (2018a)?

P6 L14: Do you think relating tree ring to source waters would be beneficial to study long term dynamics (e.g., Singer et al., 2014)?

P6 L25: See general comments regarding the “n Water World”.
P7 L18- P8 L23: These paragraphs read like a review and do not link to “Limitations and challenges”.
P8 L26: Please note the intensive sampling of groundwater stable isotopes by Scheliga et al. (2017).
P8 L29- P9 L10: To me, you miss pointing out the limitations and challenges in these paragraphs.
P9 L21: Regarding the heterogeneity of soil water stable isotopes, please see the data set by Yang et al. (2016) the intense variability across relatively small area.
P10 L23: Consider including the interesting study by Millar et al. (2018).
P10 L14: This ratio between water accessible for plants and water extractable by suction lysimeter is time variant (Sprenger et al., 2018b) – this is usually ignored and cannot be accounted for when sampling twice soil and plant isotopes over a year.
P12 L5: I suggest providing examples, where this has been done (e.g., Knighton et al., 2017, Smith et al., 2018, and Kuppel et al., 2018).
P12 L1: As done by Bachmann et al. (2015).
P12 L12: as reviewed by Rothfuss and Javaux (2017)
P12 L14: Can you provide any studies backing this idea? Is to “introduce further complexity” something positive in this context? This “potential interactions of these tracers with soil, roots and the water itself” would need to be carefully studied and understood for the tracer interpretations, right?
Fig. 1: You’ve added remote sensing to the graph, but do not discuss it in the text.
Fig. 1: Plant uptake and transpiration have scales > 10m in your graph, but much of your discussion deals with processes way smaller where water is taken up by roots (scales « 1m).
Fig. 1: How about potential of tree ring samples (in the category “grab sampling”)?
Fig. 2A: It seems from Fig. 2B that the green shading represents the range of samples and the black line represents an average value. Are these values then representing average and range from replicates (e.g., five soil samples at 0-5 cm soil depth)?

Otherwise, how could you have a range from one sample value (as indicated on the y-axis)? Or does the green shade the range of values out there in nature and the black line represents the individual sample? This is not clear to me.
Fig. 2C: Not sure if I understand how the temporal variability of precipitation and throughfall is higher within events than among events. Is not the variability within the events part of the overall variability among events? Looking at Figure 6 in Freyberg et al. (2017) it seems that variability between events is higher.
Fig. 2C: Why did you not include plant water isotopes?

Technical corrections

Title: As I understand, the manuscript title must start with “Ideas and perspectives:” (https://www.biogeosciences.net/about/manuscript_types.html)
P2 L24: Repetition of “Stable isotopes of hydrogen and oxygen”
P14 L1: Coauthors missing for Bertrand et al. (2014).

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