

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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Review of Penna et. al.:

The submitted paper is a review of recent studies in the use of stable isotopes to understand water cycling within ecosystems. The paper draws together multiple up-to-date papers that, when taken together, present a somewhat concerning view of the field as it currently stands. This I feel is much needed within the community, and publication of an even-handed review of these issues is well suited to Biogeoscience. A few clarifications would be helpful before publication:

C1

Wording: Given the above, it is critically important that the authors of this paper are very careful with their citations and the language used to discuss the conclusions of others. For example, on P5L10, the authors claim that in Brooks 2010, the sampled xylem water doesn't match the signature of potential source waters. I think this is a mis-interpretation of the Brooks 2010 conclusions, which as stated in the abstract of that paper were that: "initial rainfall events after rainless summers is locked into small pores with low matric potential until transpiration empties these pores during following dry summers." So, Brooks 2010 et. al. do hypothesize that the xylem water does match the signature of a potential source, just not that of the well-integrated stream water. I do not take issue with your conclusion that the wrong pools are often sampled, just how this section (and others) is written could be improved for clarity and consistency. Another example is P5L24, where the authors state that soil water isotope ratios vary with water potential (citing Brantly 2017), then introduce the study of McCutcheon (2017) as a singular example that didn't find this to be true. Given that you've got two competing recent phenomenological studies, some readers are likely to decide that this issue is decidedly unknown at this movement. However, reading through the paper as is currently worded, other readers could interpret that Penna and co-authors are definitively claiming that tight or loosely bound waters always have differing isotope ratios (though only one citation for this is given). Again, the wording needs to be clarified and more citations added.

Figures: A bit more work on your figure 2 would be helpful. I found the first two panels very difficult to interpret. I think a lot more thought needs to go into this figure and its description. For instance, your caption discusses multiple measurements made to characterize a heterogeneous domain. However, panel A doesn't depict multiple samples, but only the variation within a sample as its volume or duration increases. In panel B you show what looks like a gaussian distribution, but I fail to see how this represents the presence of both micro-scale and macro-scale heterogeneities. I find panel C the most informative here, but it also seems highly speculative and difficult to compare. Since you've got no x-axis, the location of the mean is meaningless, and all

C2

you're really plotting is differences in standard deviations, so why not have similar set of axis as figure 1, but with variation itself? Also, what justification do you have for any of these standard deviations. This figure seems highly speculative and is only referenced in an off hand manner (P10L14). Finally, is figure 1 adapted from Bowen & Good 2015, it seems quite similar.

Spatial Patterns: On P7L7 the authors claim that less attention has been paid to spatial processes, which is decidedly not the case. Gabe Bowen has spent many years pursuing this topic in particular, published many papers, and even publish a book (Isoscapes) and recent review paper (Bowen and Good 2015). While much work has been focused on precipitation and surface water isotope ratios, many studies have extended this concept to plant and animal tissues.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-286>, 2018.