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Interactive comment on "Reviews and syntheses: Gaining insights into evapotranspiration partitioning with novel isotopic monitoring methods" *by* Youri Rothfuss et al.

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This paper addresses the use of isotopic data to partition evaporative water fluxes from terrestrial ecosystems, specifically focusing on transpiration vs. soil evaporation. It notes that isotopic methods have frequently been labelled "powerful," but have in fact proven difficult to use. The authors describe the barriers and complexities and propose several preferred pathways forward. It seems that these methods may be ready, at long last, to leap over the barriers that have constrained them. This paper prepares us for that leap by focusing attention on emerging best practices.

The topic is deeply relevant to BG because water fluxes control so many biogeochem-

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Discussion paper

ical processes. This is so because, first, transpiration and soil evaporation are controlled by different environmental variables, but also because the water is drawn from different depths in the soil, which results in different vertical patterns of soil moisture. Third, the resulting moisture profiles affect many biogeochemical processes and fourth, the water fluxes redistribute solutes. It seems that these methods may be ready, at long last, to leap over the barriers that have constrained them. This paper prepares us for that leap by focusing attention on emerging best practices.

The paper begins with a review of earlier work and finishes with suggestions about how to move forward. Its novelty lies in the detection and presentation of trends in the broader literature and in the identification of key results in recent papers. These key results are mostly methodological. Because of this structure, the paper does not so much come to novel conclusions as emphasize promising methods.

It might be helpful to begin with a summary of where the large variation in T/ET comes from. Before the series of T/ET values in Section 2, it would be useful to note that some of this variation is probably real and that some of the variation is predictable, e.g., when soils range from wet to dry or canopies range from isolated seedlings to closely spaced mature plants. The range of T/ET estimates in Section 2 would then make more sense. Also, I understand that Section 2 is intended as a timeline, but I wonder if it could be provided a bit more narrative flow. In particular, topic sentences at the beginnings of the paragraphs would help, if this is possible.

I was puzzled by the fact the isotopic estimates of T/ET were not directly compared to other methods. This is especially surprising because (Sutanto et al., 2014), which is not cited, made this comparion and concluded that the isotopic data yielded lower estimates. It would seem that this discrepancy should be presented and discussed. This could also precede Section 2.

An important strength of Section 3 lies in the text descriptions of the meanings and assumptions of the mathematical models that have been used in this literature. This is not an easy sort of writing, but these authors do it well. My only general criticism was that I found it difficult to determine which of these insights were drawn from previous work and which are new here.

In section 3.2, the authors discuss the isotopic signal of evaporation, focusing almost exclusively on soil evaporation while ignoring evaporation from plant surfaces (interception). Canopy evaporation can consume a substantial part of precipitation and it has isotopic consequences. There is some literature on the latter topic that includes descriptions of its isotopic consequences. The Allen et al. review (Allen et al., 2017) is one place to start. Much of the interception literature comes from forest canopies, but the same processes occur in crop canopies (e.g, Zheng et al., 2019). I presume that the models presented here include interception as part of transpiration, but perhaps they simply neglect wetted canopies. In either case, the treatment of interception should be explained clearly. It will be especially important in long-term estimates, in dense canopies, and where rainfall is frequent and light. I suppose it must also be much more important in sprinkler irrigation than in ditch or drip irrigation. If it is a research gap, I would highlight it in hopes that it will be addressed.

It is also critical to recognize the contribution of Braden-Behrens et al., (2019), who have applied eddy covariance techniques to water stable isotope data, as suggested by the authors. This would seem to fit around line 337, at the climax of the methods section.

I would also suggest more caution regarding the Keeling-Plot technique. Like the other methods described here, it is easily abused. As the authors note, the method depends on three important assumptions: first is that the method can only work if there are twoâĂŤand only two– uniform water sources in the mixture. This can be problem along vertical canopy profiles, where the isotopic composition of evaporating water is likely to vary with the rooting depth of the different species and perhaps, with the humidity and isotopic composition of the atmosphere surrounding the leaves. Perhaps this problem is less severe in a crop monoculture than in mixed vegetation, but the issue should be pointed out. The second problem with Keeling plots is that regression tends to flatten models fitted to noisy data, leading to incorrect estimates of the y-intercept. Because of this issue, some authors in the CO2 literature have recommended using the method only if the data meet fairly stringent requirements for R2. A wide range may help provide a high R2, but it does not guarantee it. Finally, there should be some discussion about which regression method should be used for the fitting of Keeling plots (Pataki et al., 2003; Wehr & Saleska, 2017).

Specific Comments: Line 16, 55: is it "powerful?" The manuscript argues otherwise later, both in a brief statement on lines 810-813 and in its overall tone. More than that, the Sutanto et al. (2014) review raises serious questions about this.

I would drop the first two paragraphs and replace them with the general description of T/ET requested above.

L67-70: the iss issue is important, but complicated, in part because it depends on one's objectives. I know these authors want to talk about this, but I would wait to raise it until later, when it can really be dealt with. L81:futuring is in sec 4, not 3, right? L85: progress L98: "noticeably low" and L104: "exceptionally high." Do the authors doubt these estimates? This should be clarified either as these comments are made or, perhaps better, in a final summary statement about true values of T/ET. As noted above, this paper would be strengthened by a general statement about what values T/ET should take and by a statement of how well the isotopic estimates match the alternatives. L150: the Péclet effect seems important here and it should be explained carefully. It is more than compartmentalization. The effect is described in a bit more detail on lines 640-641, but it is not named there. L265: explain ambient vs. backgd. Could this be called instead canopy vs. troposphere, for example? L280: deltaET, not ET L321: a plus or minus symbol missing? Also there is no earlier equation estimating C from a Keeling plot. If there were, you should cite it by number. L351-2: Not a strong diagnostic as the linear form can survive a linear change in either variable. 395: different footprint areas L460: hatm is determined by TDR? L582: A useful way to conclude this list of complications and worries would be to compare the estimated fluxes to empirical data from chambers or weighing lysimeters. This would allow the reader to decide how well these models work. I would do it with a figure. L719-720: this point is so important, but it is not clearly worded. I would say something like: "…assume ISS and hence treat δxyl as equal to δT. Although this assumption is probably justified for a daily integration, there is growing evidence that plants reach ISS only briefly in the course of a day, especially when environmental conditions change rapidly. Thus the analysis is greatly simplified by daily integration, if that is sufficient for the study objectives."

But perhaps the authors disagree?

L763: point, not punctual

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