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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-

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.

A: Dear Prof. Marshall, In name of my co-authors, I would like to thank you for the time and effort you put into reviewing our manuscript! You will find below a list of answers to your general and technical comments.

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.

A: Section 2 was, as a matter of fact, constructed as a pure timeline to underline new developments in isotopic sampling, analysis, and data interpretation techniques from 1990 up until today. We will give it a bit more "rhythm" in our revised manuscript, e.g., by using topic sentences, when possible, as suggested.

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 comparison 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.

A: Our main goal was to give a technical overview of the ensemble of isotopic partitioning methods and underline their challenges and progresses, and this is why we chose not to question/compare the isotope-derived T/ET results with those of non-isotope techniques. In our revised version, we will however cite the opinion paper of Sutanto et al. (2014) and report its main findings in the introduction section, before Section 2, as suggested, to put T/ET isotopic findings in perspective.

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.

A: Thank you! We will do as asked and better distinguish the existing studies and their results from our suggested improvements/changes, especially in Sections 3.1.2, 3.2.2,

and 3.3.2.

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.

A: The issue of interception (i.e., direct evaporation of precipitation/irrigation water from leaves' surfaces) was not addressed here, this is true! In our revised version, we will clearly say that this additional and intermittent water vapor source is not identified in the mixing equation nor quantified in the literature reviewed in our manuscript, therefore not treated here.

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.

A: Many thanks! We have found another preprint of Jelka Braden-Behrens et al. (2020) in BGD, which will perfectly fit and strengthen Section 3.1.2.

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 two and 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).

A: We are ourselves very cautious in our application of the Keeling Plot in the field, but surely, this should better transpire from our text Section 3.1.2 and Section 4. We will incorporate your valuable comments and cited references, thank you.

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.

A: There is indeed a definite contradiction here with the rest of the text, thanks for pointing this out! We will remove "powerful" from the introduction (as well as from the abstract). Furthermore we will drop the first two §as requested in our revised manuscript.

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.

A: We wanted to list the two factors, which act on the isotopic difference δT-δE, namely the differences in boundary conditions acting on T and E, and differences in transpiring vs. evaporating states (i.e., reaching or not of ISS for T and E). Since one factor cannot alone explain the isotopic difference, we would still prefer to keep them both listed. However we will make special reference regarding ISS to section 3 (and especially section 3.3.2) L67-70.

L81: futuring is in sec 4, not 3, right?

A: Already in section 3, there is mention to future progresses; however, we will make this much clearer, i.e., better highlight the improvements suggested in the literature and also distinguish them from our own suggested ones (, where we focus on monitoring methods in particular). Section 4 is partly a summary of Section 3; it is the place where we give our opinion regarding ways forward for a continuous and non-destructive assessment of T/ET. We will make this also clearer.

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.

A: Yes, we agree (please see our answer to your general comment above.).

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.

A: To our knowledge and from our literature review, there is no study, in which Péclet effect values were determined for the specific purpose of ET partitioning. This stems certainly from the fact that calculations imply steady state in the first place (δT=δstem_water). This is why we did not explain the Peclet effect in Section 3.3.1. To make things clearer, we will remove mention to it L150.

L265: explain ambient vs. backgd. Could this be called instead canopy vs. troposphere, for example?

A: We would prefer keeping the broader terms “ambient” and “background” for the following reasons: for e.g., crops or grasslands, air may be drawn from above the canopy, therefore does not apply strictly to “canopy air”; the background air may be different than troposphere air. However, we recognize that both terms are currently not precisely defined L265. We will do this in our revised manuscript. Thank you.

L280: deltaET, not ET

A: Thank you! This will be revised.

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.

A: We have checked the expression for the slope C and did not find an error. Nevertheless, we will rewrite the sentence L321 to: “We note that, by assuming ˆj_χ_atm≈χ_atm, the expressions for δatm provided by the flux gradient and Keeling plot techniques are mathematically identical if C=χ_bg (δ_bg-δ_ET) R_std=s·R_std, with s, the Keeling plot slope.

L351-2: Not a strong diagnostic as the linear form can survive a linear change in either variable.

A: Yes, this is true! Thank you for your remark, which we will add to the text.

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L395: different footprint areas

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A: Thank you. This will be revised.

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L460: hatm is determined by TDR?

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A: Not by TDR, but by, e.g., capacitive sensing. Thank you for pointing the mistake out! This will be revised accordingly.

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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.

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A: Thank you. We will mention that chambers and semi-controlled conditions experimental setups (such as weighing lysimeters) provide means to test the validity and existence of the abovementioned hypotheses and complications.

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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?

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A: The authors absolutely agree! Thank you for this suggestion. We will use it in L719-720

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L763: point, not punctual

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A: Thank you! _______________

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