

Interactive comment on “New isotope-based evapotranspiration partitioning method using the Keeling plot slope and direct measured parameters” by Yusen Yuan et al.

Anonymous Referee #2

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General comment

I am reviewing the study entitled “New isotope-based evapotranspiration partitioning method using the Keeling plot slope and direct measured parameters” by Yuan and colleagues submitted for publication to HESS.

The authors propose a modification of the Keeling plot (KP) method, which originally consists in determining the evapotranspiration flux isotopic composition (δET) from the value of the offset of the linear regression of the isotopic composition of atmospheric water vapor (δatm) with the reciprocal of the water vapor mixing ratio ($1/Catm$). The KP method is often applied in the specific context of ET partitioning as it provides one of the

C1

three end-members (the other two being the isotopic compositions of evaporation and transpiration, δE and δT , respectively) of the water isotope partitioning equation and allows for determination of the transpiration-to-evapotranspiration ratio (T/ET). One of the challenges in the original method resides for instance in capturing in a short period of time strong dynamics in $Catm$ and δatm that would minimize the error made on δET , and ultimately on the computed T/ET value.

The authors build on the original KP method concept and derive an expression for T/ET , which would exempt users to determine δET in the first place. They find a good correlation between their model output and those of the original KP method.

The manuscript fits the scope of HESS well and is of appropriate length for a technical note, (maybe a bit short for a scientific article). However, it did apparently not undergo internal review prior submission and should be checked for language mistakes! The text is on quite a few places difficult to understand (see my technical comments), which should not be the case, Lixin Wang being the corresponding author.

There is a series of issues that the authors must address. I have, in general, a problem in grasping their method premises. They argue, for instance, that the y-value of the intercept between the two regression lines corresponding to cases $ET=E$ and $ET=T$ is equal to the isotopic composition of the background water vapor. However this is a physical impossibility as it would require simultaneous determination of the two lines, yielding to a situation where both cases ($ET=E$ and $ET=T$) co-exist. But more importantly, I found some inconstancies (errors? typos?) in their trigonometry exercise, which raise the question of the validity of their approach. Finally, the authors run a sensitivity analysis (to unknown parameters), on basis of which they highlight that most of the uncertainty associated with T/ET propagates from that of δET . But a sensitivity analysis does not inform on the model error, rather on its . . . sensitivity. In order to evaluate the relative extent, to which each parameter error contributes to the total error associated with T/ET , the authors should use the error propagation principle. They may refer themselves to the literature for this (e.g, for the theory, Phillips and Gregg, 2001;

C2

and one application example, Rothfuss et al., 2010, which the author already cite.) - Phillips DL, Gregg JW. 2001. Uncertainty in source partitioning using stable isotopes. *Oecologia* 127: 171–179.

The authors will find my specific comments below:

Specific comments

Highlights

Highlights 1 and 2 overlap. I suggest merging them.

Abstract

Check where variable symbol is given however not further mentioned. Furthermore I suggest a bit of streamlining, i.e., 1- don't bother mentioning the other partitioning method; 2- better detail the underlying concept of the modified keeling plot (KP) method; 3- mention the validation step; 4- close the abstract by explaining the benefits of using this new application of the KP method compared to the "traditional" use of the isotopic partitioning equation.

L29-30. Strictly speaking, the partitioning of ET does not help you quantify fluxes. For this you need ET flux density absolute values. Please reformulate

L32. "from the field scale to the global scale"

L33. "often" is a too strong word. Rothfuss et al. (2020, BGD) show that barely 40 ET isotopic partitioning studies were published over the period 1990-2019. . . Please revise - Rothfuss, Y., Quade, M., Brüggemann, N., Graf, A., Vereecken, H., and Dubbert, M.: Reviews and syntheses: Gaining insights into evapotranspiration partitioning with novel isotopic monitoring methods, *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2020-414>, in review, 2020.

L40. "Our study presents"

C3

1. Introduction

Check where acronyms are given however not further used in the text.

L55-57. Check phrase construct. Also if you mention open water bodies evaporation, you may as well mention water intercepted by the vegetation. I suggest you restrain to $ET=E+T$ to define your framework.

L62. "Besides". Revise (colloquial).

L70. Sap flow measurements are not "direct measurements". Please rephrase.

L75. Yopez et al. (2003) did not develop their own "isotope model", they used the KP method for δET , the Craig and Gordon (1965) model for δE , and the steady state assumption for δT . Please revise.

L78-79. A flux cannot "result" in a concentration ratio. Please rephrase. In addition, you may skip or edit the (quite vague) statement "Hydrogen and oxygen isotopes are natural components of the hydrological cycle". Also: the physical principle driving the discrimination against water stable isotopes during E and T are the same (different in mass among the three isotopologues), only the boundary conditions and system state variables are different. Please rephrase.

L84. "suggested that . . ."

L85-86. "resulting in either overestimation (Sutanto et al., 2012) or underestimation (Wu et al., 2017) of FT values compared to . . .".

L86-87. A model sensitivity analysis does not inform on model error. I suggest writing something like "Most of the error associated with isotope-derived FT estimates propagates from that made in estimating δET ". Also in the article of Cui et al. (2020), it reads something else, i.e., "Based on field observations, the uncertainties of end members δET , δT , and δE in the chamber method for $\delta^{18}O$ (δ^2H) were 0.7‰ (4.2‰, 1.1‰ (4.6‰, and 0.8‰ (4.7‰, respectively, while the uncertainties of δET (Keeling plot) and δE (CG

C4

model) in Keeling's RCG method for $\delta^{18}\text{O}$ were 1.1‰ and 1.0‰" Please revise.

L90. This is not the case for the study of Griffis et al. (2008). Please correct.

L93. ", which leads..."

L97-98. Please check English (grammar). I don't understand.

L99-100. Check English (grammar).

L100-101. This is obvious; consider removing.

L102. So, is it new, or "modified from"? Wording here is important.

L103-104. Please detail what the "identical instrumental setting" means in the current context.

L104-105. "the new method was evaluated against..."

2. Materials and Methods

2.1 Isotope-based ET partition methods

2.1.1 Traditional method

L114-120. Language quality drops here substantially. The explanations are hard to follow, although the mass-balance principle behind the KP is rather simple. The reader may try to find a link between Eq. (1) and Fig. (1), although there is none. Make this explicit, by moving/merging the text about the KP method further down L121.

L111-112. Check English (sentence construct).

L114. The "relationships of δE and δT " to what?

L117-120. The interception point between line 1 and 2 is not the point of coordinate δa , $1/\text{Ca}$ since the lines cannot exist simultaneously. The ensemble of scenarios leading to different values of δET are not related at all.

C5

L117-118. What is the source of ambient vapour? You mean background (local) water vapor, certainly.

L124. The distinction between "ambient" and "directly measured" is hard to make. I suggest referring to ambient air (what you measured) and background (local) air (what you could not directly measure).

L125-130. I suggest simply saying that δv and C_v are the mean computed values of the calibrated readings of the laser spectrometer over a given period of time. And please indicate the value of that period of time (e.g., 1 hour).

L130-131. The KP is a scatter plot of $1/C_v$ versus δv . It is not the regression line. This is why a "linear Keeling plot" does not make much sense. On a side note, if there is a way to measure Ca and δa , then one can determine δET from k .

L132-151. Please revise English thoroughly.

L136-137. How can be the evaporation located in an entire layer? You should say that the "evaporation front isotopic composition is approximated by that of the water in the soil layer (0-5 cm)".

L137. This is neither from a terminology standpoint nor a language standpoint correct: it should read something like " ϵ^* and α are the liquid water-water vapor equilibrium fractionation value (‰ and fractionation factor (-), related by:..."

L142. "n" has nothing to do with water stable isotopes, rather with the overall aerodynamic conditions within and above the canopy (see e.g. Merlivat and Coantic, 1975). Please explain why you consider laminar flow ($n=0.67$) here? This is quite unlikely to happen in the field.

- Merlivat, L., and Coantic, M.: Study of Mass-Transfer at Air-Water-Interface by an Isotopic Method, J Geophys Res-Oc Atm, 80, 3455-3464, doi:Doi 10.1029/Jc080i024p03455, 1975.

C6

L144-145. Values for the diffusivity ratio coefficients have been revised back to those of Merlivat (1978) by Luz et al. (2009). Therefore I strongly suggest that you run the calculations anew.

- Luz, B., Barkan, E., Yam, R., and Shemesh, A.: Fractionation of oxygen and hydrogen isotopes in evaporating water, *Geochim. Cosmochim. Acta*, 73, 6697-6703, doi:DOI 10.1016/j.gca.2009.08.008, 2009.

L146. Why “also”.

L149. I suggest that the authors also take into account the increase of flow rate due to transpiration in the chamber as by Simonin et al. (2013).

- Simonin, K. A., Roddy, A. B., Link, P., Apodaca, R., Tu, K. P., Hu, J., Dawson, T. E., and Barbour, M. M.: Isotopic composition of transpiration and rates of change in leaf water isotopologue storage in response to environmental variables, *Plant Cell Environ*, 36, 2190-2206, <https://doi.org/10.1111/pce.12129>, 2013.

2.1.2 New ET partition method

L153-158. I would name the corners of the triangles “E”, “T”, and “ET” and the segments e.g., “EET” instead of “(δ ET- δ E)”. The latter is a segment length, not its geometrical representation.

L161. “Combining Eq (7) and Eq (8) yields to:”

L162. Eq. (9) means that $\pi - \gamma - \beta = \gamma + \beta$, which leads to $\beta + \gamma = \pi/2$, therefore the remaining angle opposite to side “EET” is equal to 90 degrees (isosceles triangle), ultimately conditioning the positioning of the point of coordinate (1/Cv, δv) on the linear regression (thus cannot be randomly selected). Or am I missing something?

L165. This equation implies that $\sin(\gamma)=1$, which means that γ is now 90 degrees, thus the regression has slope $k=0$? I am lost here. . .

2.2.2 Field Experiment

C7

Please revise this section for language mistakes, e.g. “The switch process between two independent measurements were self-acting”, “within the circulation.”, “Our vapor calibration procedure was mainly corresponding to the study by Yuan et al. (2020).” “The isotopic compositions values relative to the Standard Mean Ocean Water (SMOW).”

L197-198. Give information about the sensors used.

L199. Define “atmospheric vapor and mixed vapor” again here. It is not clear to what both expressions refer to.

L214. “avoid residual issue”. Do you mean “memory effects”? Please revise

L219. “minor acrylic glass frame”?

L220. Soil was drilled, not its samples.

L223. “in liquid water mode”

Fig. 3. This is not needed, really... can you give the model name and part of the pump for sampling atmospheric air at canopy height, 2, and 3 meters height? What was the air sampling flow rate value?

L224. Remove practical consideration (“As our water vapor isotope analyzer was occupied due to maintenance and other experiments”). It is not relevant to the reader.

L229. “A quantity control filter”?? You mean a “quality control filter. . .”, certainly. Excluding values outside of range [0,1] is not part of a quality management procedure. It is the sign that, to the contrary, you failed at selecting the valid data prior calculation of FT! In other words, you are merely saying that you did not filtered your data.

2.2.3 Global Sensitivity Analysis

L231. “for both methods”

L233. This does not make sense. You mean certainly something like: “quantify the contribution of change in each input parameter value to the overall change in modeled

C8

FT value.”

L234. What does “The parameter interactions were considered in this approach” mean exactly?

L237. Please list the parameters, to which you tested the model sensitivity, with ranges of variations. Results and Discussion

3.1 Comparisons of the new method with the traditional method

L245. Choose a more explicit title (this goes as well for titles of sections 2.1.1 and 2.1.2)

L250-252 and Fig 4. This is surprising considering the set of equations above. . .Can you report the RMSE of the linear model? Explain the “****” in Fig. 4. Does the R² value applies when the Y-intercept is set to 0?

3.2 The advantages of the new method compared with the traditional method

3.2.1 The elimination of high sensitivity contribution parameter δET

Please revise titles 3.2 and 3.2.1.

I have a fundamental problem with this section. You are investigating the sensitivity of a model to a set of parameters. It does not inform on the error you make. These are two different things! Therefore the statement “This result indicated that Keeling-plot-related parameters (δET and k) brought most of the uncertainty to estimate FT.” is not valid.

3.2.2 The new method avoids extrapolation of Keeling plot

Please revise title.

L274-275. Maybe mention that this study was performed in the lab, so the conditions do not reflect nature.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020->