

Reply on RC1

Matthias Sprenger et al.

Author comment on "Precipitation fate and transport in a Mediterranean catchment through models calibrated on plant and stream water isotope data" by Matthias Sprenger et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-93-AC1>, 2022

We thank Reviewer 1 for taking the time to critically evaluate our manuscript. We are glad to hear that Reviewer 1 agrees that the manuscript addresses a meaningful research question and fits well into HESS. We respond to each comment below in bold font.

Summary

The manuscript "Precipitation fate and transport in a Mediterranean catchment through models calibrated on plant and stream water isotope data" by Sprenger et al. presents a new multi-objective calibration approach ($KGE_Q + MAE_T$) in the StorAge Selection (SAS) function using plant and stream water ^{18}O isotope data. This optimization yields both less variable and older estimation in evapotranspiration (ET) age distributions than that of the conventional calibration approach (KGE_Q only). Though a potential shortage of the SAS-derived young water fraction (F_{yw}) when applying to the highest and the lowest discharge quantiles, the water age estimation from the modified SAS function in the Can Vila catchment well explains the results of the end-member splitting and mixing analyses, and provides support for the Two Water World (TWW) assumption.

General comments

The manuscript addresses a meaningful research question on how to improve the performance of a water transit time model. This topic fits HESS well, and the manuscript is generally well written and structured. However, an inconsistent assumption and untenable objective functions potentially weaken the reliability of the results. Thus, to reach the manuscript better shape, I recommend a moderate to major revision and re-run the SAS function in terms of the two following directions:

The algorithm of SAS calibration target δ_{ET} is based on different assumptions. According to Page 5 Line 20 (P5L20, page num. and line num. abbreviate as P*L* hereinafter), $E_T = 0.77 ET$ and $E_S = 0.23 * ET - E_I$. δ_{ET} in P7L15 would therefore be $(E_T * \delta_{source} + E_S * \delta_{Es}) / (E_T + E_S)$. That means the author assumes $ET = E_T + E_S$. However, $ET = E_T + E_S + E_I$ according to P5L16-21. If the author consider E_I as a part of ET, δ_{ET} should be $(E_T * \delta_{source} + E_S * \delta_{Es} + E_I * \delta_{EI}) / (E_T + E_S + E_I)$. That means the isotope composition of the canopy storage (δ_{EI}) should be a known parameter. If E_I can be ignored in this study, $E_S = 0.23 * ET$ rather than $E_S = 0.23 * ET - E_I$. Then the author should explain why E_I can be ignored, and remedy this mistake in terms of sensitivity analysis. Empirically, δ_{ET} might be

more sensitive to δ_{source} than to δ_{Es} and to δ_{ET} .

Response: Thanks for pointing out that the isotopic composition of Ei was not mentioned in the manuscript. Since the isotopic composition Ei was not measured, we assume that it is the same as for Es: the weighted average of the isotope ratio in the rainfall 30-days previous to each xylem sampling. Accordingly, we will change in the referred paragraph as follows:

“We used the xylem source water δ_{source} to infer the isotope ratios of the combined evaporation and plant transpiration flux assuming the water lost via interception evaporation or sustaining soil evaporation has isotope ratios (δ_{30}) equal to the weighted average of the rainfall 30-days previous to each xylem sampling.... as: $d_{\text{ET}} = 0.77 \text{ ET} * \delta_{\text{source}} + 0.23 * \text{ET} * d_{30}$.”

(2) Ambiguous reasons to apply different objective functions. The author applies KGE_Q , MAE_T , and $\text{KGE}_Q + \text{MAE}_T$ to determine $k_{Q\text{min}}$, $k_{Q\text{max}}$, k_{ET} , and S_0 , but why MAE_T calibration approach is missed to simulate δ_{ET} , δ_Q , and the median water age? Is there any possibility that MAE_T performs even better than $\text{KGE}_Q + \text{MAE}_T$? Prior to emphasizing the advantage of $\text{KGE}_Q + \text{MAE}_T$, the limitations of both KGE_Q and MAE_T should be exhibited. Furthermore, the unit of $\text{KGE}_Q + \text{MAE}_T$ is chaotic. The unit of the best value for KGE is dimensionless, but the unit of the best value for MAE is “‰”. Although $(1 - \text{MAE}) + \text{KGE}$ is normalized to 0 numerically, I don’t agree that this term has physical and statistical significance.

Response: The MAE_T calibration did not miss to simulate δ_{ET} , because MAE_T is a function of δ_{ET} . Thus, δ_{ET} was calculated for each calibration run and you can see the results in the center panels in Figure 3. There, one can see that simulation of δ_{ET} based on the MAE_T calibration is better than with the $\text{KGE}_Q + \text{MAE}_T$, as discussed in our manuscript (e.g., trade offs). We did not show any results for the simulation of δ_Q and neither for the water ages based on the MAE_T calibration, because these results are not meaningful: The performance of such a calibration approach resulted in a KGE_Q of 0.43. To clarify this, we will add to 3.1 the following sentence: “A calibration solely based on MAE_T did not result in meaningful simulations of the $d^{18}\text{O}_Q$ (KGE_Q of 0.43), which is why that approach is not considered in the discussion nor are simulations shown.”

Regarding the units of the multi-objective calibration objective function $\text{KGE}_Q + \text{MAE}_T$, as both of the individual objective functions were normalized via rescaling, they are both without units. After that rescaling, the sum was calculated as follows: $(1 - \text{MAE}) + \text{KGE}$, as described in the manuscript.

Specific comments

P1L21: The author only uses ^{18}O in this study.

Response: We will take out all references to 2H data.

P3L16: Shouldn’t be tracer signals in ET flux together with discharge (Q) could be used to better constrain SAS models? \\\

Response: We will add: “(together with tracer data of Q)”

P3L30-31: By in situ measurement, we could obtain 1-hour (Wei et al., 2015) or even 15-min (Yuan et al., 2022) temporal resolution of δ_{ET} . Xiao et al. (2018) and Rothfuss et al. (2021) reviewed different δ_{ET} fitting methods. While some data in this manuscript was from almost 10 years ago when high-resolution water isotope data was rare, the author should show the sensitivity of input δ_{ET} on $k_{Q\text{min}}$, $k_{Q\text{max}}$, k_{ET} , S_0 , and other output results.

Response: We do not aim to run simulations with synthetic δ_{ET} data to assess the sensitivity of the calibration approach to δ_{ET} variability. There are various modeling studies available that assess the influence of the information content of calibration targets on the calibration performance. While such studies are not geared towards the ET flux, but usually towards modeling tracer and volume of Q , the conclusions apply generally to the data used to optimize the parameter according to the objective functions that include these data. We discuss the limitation of the limited sample numbers in our manuscript.

P4L13-16: Please revise based on issue #2 in the general comment.

Response: As outlined above, we will mention how using the MAE_T objective function alone fails to simulate stream water isotope dynamics, but we do not see that as a research question, as no one would expect that one can simulate stream water isotopes by calibrating a model solely xylem isotope data.

P7L4-6: Add citations.

Response: We will add: (Martín-Gómez et al., 2015)

P7L13: Should be "soil evaporation isotope ratios (δ_{Es})".

Response: The sentence will be changed as provided above.

P12Figure3: In the right panel, y-axis should be MAE instead of MAE_T . If MAE_T is applied here, scatters should gather in the lower-left corner rather than in the upper-left corner. Nevertheless, I still question the validity of $KGE_Q + MAE_T$ based on issue #2 in the general comment.

Response: We cannot follow, why it should be named MAE. The right panel just shows the calibration results for both KGE_Q and MAE_T . As the lowest MAE_T value is 0.321 ‰, the scatter should not be in the lower left.

P14Figure4: Missing the description of x-axis.

Response: The x-axis of Figure 4 is a date. Please, open any manuscript in HESS and check if they have their x-axis labeled as "Date" when they show time series.

P14L10-17: I recommend insight into the reason why highly dynamic rainfall-runoff dynamics could not be fully captured during rainfall events after a long dry period. In my view, it might be due to the lack of observed δ_Q data by the end of the dry period. As the numerical routine of SAS model is based on the classic Euler scheme (Benettin and Bertuzzo, 2018) whose convergence is relatively slow, more data is required to speed up the converging. That potential reason might also be able to explain why short-timescale processes can be well captured from this dataset.

Response: SAS models deal with dynamic flow pathways by allowing the shape of the SAS function to change over time. It is generally expected that, during storm events, the contribution of faster flowpaths increases and this is why we modeled the SAS function to select younger water when the water storage is higher. This is, however, a very simplified approach to deal with changing flowpaths. The fact that the model is less accurate over the March 2013 event means that flowpath reactivation dynamics, especially after a prolonged dry period, are more complex than what can be reasonably captured by a simple shift in the SAS function when the total water storage changes. Having more δ_Q

data by the end of the dry period would certainly help understand the change in flowpaths and perhaps it would help build a more advanced relationship between the SAS function and the changing flowpaths. The convergence of the numerical routine is not expected to change the model results because it is unrelated to tracer data. The numerical accuracy at the current time step is satisfactory and running the model at shorter time steps does not change the model accuracy.

P15Figure5: Missing the description of x-axis. The author should show more detail on the comparisons of salutation results in terms of different calibration approaches, such as RMSE. It seems like KGE_Q based simulation perform better than KGE_Q+MAE_T based simulation in 2013 summer.

Response: The x-axis of Figure 5 is a date. It is not clear what a salutation result is and we do not see how RMSE would be a better goodness of fit than what we present. The KGE values shown in Figure 5 for the KGE_Q approach is 0.75 and thus higher than for the KGE_Q+MAE_T approach with 0,72. Thus, the KGE_Q has a better fit. However, as discussed in the manuscript, this is something we would expect (see discussion on trade offs).

P19L22: Duplicate callouts of Fyw.

Response: Sorry, unclear what this comment means.

References

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