Comment on hess-2022-93

Anonymous Referee #1

Referee 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-RC1, 2022

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:

(1) 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_{EI}.

(2) Ambiguous reasons to apply different objective functions. The author applies KGE_Q, MAE_T, and $KGE_Q + MAE_T$ to determine k_{Qmin}, k_{Qmax}, 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 $KGE_Q + MAE_T$? Prior to emphasizing the advantage of $KGE_Q + MAE_T$, the limitations of both KGE_Q and MAE_T should be exhibited. Furthermore, the unit of $KGE_Q + MAE_T$ is chaotic. The unit of the best value for KGE is dimensionless, but the unit of the best value for MAE is "‰". Although (1 - MAE) + KGE is normalized to 0 numerically, I don't agree that this term has physical and statistical significance.

Specific comments

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

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

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_{Qmin}, k_{Qmax}, k_{ET}, S_0, and other output results.

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

P7L4-6: Add citations.

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

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.

P14Figure4: Missing the description of x-axis.

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.

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.

P19L22: Duplicate callouts of Fyw.

References

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