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Hydrology and Earth System Sciences Discussions
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Interactive comment on "Using StorAge Selection functions to quantify ecohydrological controls on the time-variant age of evapotranspiration, soil water, and recharge" *by* Aaron A. Smith et al.

Aaron A. Smith et al.

aaron.smith@abdn.ac.uk

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Reviewer 1 General Comments

First, the evaluation of the model is not very compelling and needs to be improved to make the model results more credible. The model seems to have a large number of parameters calibrated to a relatively small number of isotopic measurements with high within-day scatter. The main indicator of model skill shown in the manuscript is an ability to roughly reproduce a seasonal signal in isotopic concentration that dampens with depth (Figure 3). The model also, presumably, simulates soil moisture, but this was not compared to data in the manuscript. The calibration keeps the 100 "best" parameter sets out of 50,000 random samples, which seems to be an arbitrary standard that does not consider the absolute quality of fit between observations and simulations. The final values of the model parameters are not reported, making model performance more difficult to interpret and potentially impossible to reproduce (given the stochastic nature of the calibration). The NSEadj values for the isotopic concentrations are adequate (0.34-0.75), but this is not necessarily compelling given the high number of free parameters. No sensitivity analysis is done to show the importance of different model components in capturing the data. I was left wondering, for example, if a large change in one of the outflow SAS functions (say, in the CV at 10cm) would have an appreciable effect on model performance. If not, then the calibrated values might have a lot of uncertainty that is not presented, and the trends observed in the flux ages might not be significant. I was also left wondering, for example, if the difference between Site A and Site B SAS functions were significant, or within expected modeling uncertainty bounds. Some potential ways to improve model evaluation are listed here. (1) The model parameters could be clearly listed with their calibrated values and ranges, to give the reader a sense of the uncertainties. (2) The manuscript could start with a much simpler model and build up to the complex model presented, showing at each step how additional model complexity is justified by the data. (3) The manuscript could report a sensitivity analysis to show how each aspect of the model structure is necessary to describe the data. Second, the description of the model and underlying theory is at times confusing and seemingly imprecise. For example, the same variable is apparently used for age ranked storage in the slow and fast domain (see equations 1 and 2), some equations seem to be dimensional incorrect (see equation 3), and the CDF and PDF of the SAS function are seemingly confused (see equation 5). If the authors can make substantial improvements to better describe and evaluate the model, then the results presented in the paper (e.g., the relative ages of different ecohydrological flows, the shape of the different SAS functions and their storage dependence at contrasting sites, the approach to simulating fractionation) could be significant contributions that merit publication. Many of the issues described above are listed in more detail with

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page references in the Specific Comments section.

Response to Reviewer 1 General Comments

The authors thank Reviewer 1 for their comments. The revised manuscript will include a model parameter sensitivity analysis to aid with the model evaluation and performance and include the final distributions for calibrated parameters. As discussed in the specific comments below, the selection of the 100 best parameter sets was based on a minimum efficiency criteria rather than an arbitrarily selected number of parameter sets. Some confusion on the number of model parameters will be addressed by listing the calibration parameters as well as the number of minimum efficiency criteria to be met. The model structure was derived to be similar to previous studies, to allow for more direct comparison. To address confusion on the model theory, the methods section will be revised and will include a table of variables for ease of interpretation.

Reviewer 1 Specific Comments

R1C1: P1, L16-17: Why do dominant young water fluxes lead to stable soil water ages? I would have thought that would make soil water sensitive to inputs.

Response to R1C1: The statement was referring to the water ages of the water retained in the soil. The authors will modify this statement to: "*Dominant young water in fluxes through the soil, along with relatively low rainfall intensities, results in shorter retention in the soil of young water and a relatively stable soil retention water age.*"

R1C2: P1, L20-21: "More variable" water ages? Meaning 50-65 is more variable than 56-79? The two ranges are not very different.

Response to R1C2: The statement will be modified to state that the transpiration is slightly older, rather than variable, than evaporation on average.

R1C3: P2, L14-23: As pointed out, SAS functions have not been used to recover soil water ages at different depths. But there are other "physically-based" models that can could be modified to do that (CATHY, ParFlow, etc). Why focus on SAS functions? A

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better justification would strengthen the manuscript.

Response to R1C3: The authors were not using the SAS functions to recapture infiltration effects, rather, to inform on potential mixing regimes within the soil which affect output fluxes. The use of SAS functions provide a means to simply assess different mixing patterns, and aligns with previous water age methods used within the catchment, and provides consistency in the comparison. Additionally, the assessment of different mixing regimes in more physically based-models requires significantly more parameterization than the use of SAS functions. We will make this clearer in the revision.

R1C4: P2, L28-34: It would be helpful to outline the structure of the paper to come: theoretical development followed by case study.

Response to R1C4: Thank you for your suggestion. We will modify the manuscript to improve the outline of the manuscript structure. "*We present a further modification to the StorAge Selection approach with the theoretical development and case study of a step-wise approach (feed-forward) with multiple storage volumes.*"

R1C5: P3, L6-7: The phrase "since the time of rainfall" is a bit vague. Consider rephrasing definition of ST.

Response to R1C5: Will be amended to "*the cumulative sum of water in storage, ranked by the elapsed time water has spent in storage*"

R1C6: P3, L11-13: The parenthetical phrases "exponential distribution", "random mixing", and "piston flow" are apples and oranges. One is a distribution and two are concepts. Consider clarifying.

Response to R1C6: As suggested by the reviewer, the authors will modify the examples to "*The function may describe greater movement of young water (young water preference sampling), equal movement of all water ages (random mixing) or greater movement of old water (piston flow).*"

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R1C7: P4, L14: The text refers to a "distribution of inflow ages (ω_j)...". But the notation ω is already being used for the pdf form of the SAS function (line 11), and this is a distribution of age-ranked storage, not age, with different units. This is either confusing notation or a conceptual mistake, and should be fixed.

Response to R1C7: While the term used (ω_J) does have direct relationships with the solution of the SAS function of downward flow from the storage volume above, the reviewer is correct that this notation and definition should be clarified. To better distinguish the distribution of inflow water ages, the authors will change the notation from ω_J to w_J, where w_J represents the backwards transit time of the SAS function of downward flow from the storage above and has units of inverse time.

R1C8: P4, L15: The ζ is described as being a relative age which presumably has units of time (in p5, L12) but is set equal to the PDF form of the rSAS function ω_Q in p4,L15, which has units of inverse storage (as shown, for example, in Harman 2015 equation 5). This should be clarified. In general, the proof would be easier to follow if the units (e.g, length, inverse time) were identified when parameters are introduced.

Response to R1C8: The reviewer is correct, ζ has units of days, similar to *T*. The equation provided ($\zeta = \omega_J(S_z(T,t,z),t) = \omega_Q(S_z(T,t,z-\Delta z),t)$) should not have included ζ. This will be corrected in the manuscript and the units of ζ will also be provided. "*relative age ($\zeta = 0$ days)*".

R1C9: P4, L7-9: The age ranked storage can't be the "cumulative sum of the time", since it has units of storage. It is the volume of storage with age $<= T$. Also, since this is in terms of "absolute age of water", should it be the time since it entered the vertical modeling domain, and not just the CV?

Response to R1C9: That was a mistake, the statement should have read "*cumulative sum of water younger than T*" and will be amended to: "*the cumulative sum of water ranked by the elapsed time it has spent in the modelling domain*"

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R1C10: P4, L16-17 and Equation 1 and 2: The nature of the slow and fast domains was not immediately clear. A few more sentences of explanation would be helpful. Do they represent different conceptual storage volumes with different age ranked storages? Can they be illustrated in Figure 1? Are the left hand sides of equations (1) and (2) really identical? Assuming that they are, then we can set the right hand side of equations (1) and (2) equal, which simplifies to $2 \cdot D \cdot \Omega_D = Q \cdot \Omega_Q + E \cdot \Omega_E$. This suggests that during times when Q and E are zero, then D must be zero. Why so?

Response to R1C10: The two equations are not equal, rather, the equations were simplified to try to reduce the number of variables within a more general framework. The authors recognize that this may result in confusion and the equation and text will be modified accordingly. For additional clarification the equations will be presented as:

$$\frac{\partial S_f(\zeta,t,z)}{\partial t} = Q(t,z-\Delta z) + D_{SF}(t,z) \cdot \Omega_D(S_s(\zeta,t,z),t) - E_F(t,z) \cdot \Omega_E(S_f(\zeta,t,z),t) - R_F(t,z) \cdot \Omega_R(S_f(\zeta,t,z),t) - D_{FS}(t,z) \cdot \Omega_D(S_f(\zeta,t,z),t) - Q(t,z) \cdot \Omega_Q(S_f(\zeta,t,z),t) - Q_{FS}(t,z) \cdot \Omega_{Q_{FS}}(S_f(\zeta,t,z),t) - \frac{\partial S_f(\zeta,t,z)}{\partial \zeta}$$

and

$$\frac{\partial S_s(\zeta,t,z)}{\partial t} = Q_{FS}(t,z) \cdot \Omega_{Q_{FS}}(S_f(\zeta,t,z),t) + D_{FS}(t,z) \cdot \Omega_D(S_f(\zeta,t,z),t) - D_{SF}(t,z) \cdot \Omega_D(S_s(\zeta,t,z),t) - E_S(t,z) \cdot \Omega_E(S_s(\zeta,t,z),t) - R_S(t,z) \cdot \Omega_R(S_s(\zeta,t,z),t)) - \frac{\partial S_f(\zeta,t,z)}{\partial \zeta}$$

where the subscripts f and s represent the relative age-ranked storage in the fast and slow domain, respectively, and $D_{SF} = D_{FS}$ for all time-steps and represent the movement of slow domain to fast domain (D_{SF}) and fast domain to slow domain (D_{FS}). Additionally, the water balance of the soil will be shown:

$$\frac{dV_F(t,z)}{dt} = Q(t,z-\Delta z) - E_F(t,z) - R_F(t,z) - Q_{FS}(t,z) - Q(t,z)$$

$$\frac{dV_S(t,z)}{dt} = Q_{FS}(t,z) - E_S(t,z) - R_S(t,z)$$

where Q_{FS} fills the slow domain ($dV_S/dt = 0$), and V_F and V_S are the fast and slow domain volumes, respectively. For the fluxes in all equations, the subscripts F and S

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represent the volume of water from the fast or slow domain, respectively.

R1C11: P5, Equation (3): It is confusing that S_z is a described here as function of two variables (T, t), one variable (ζ), and three variables $(T+\zeta, t$ and $z-\Delta z)$. More notation consistency is needed. In addition, the equation does not seem to be dimensionally correct: the LHS has units of length, and the RHS has units of 1/L times L times T times T, or T 2 . I think I understand what the authors' are trying to express, but it needs to be more precise.

Response to R1C11: The clarification of the definition of the inflow water age distribution will aid with the dimensional confusion. The inflow probability distribution (new term, w_J) has units of inverse time ($time^{-1}$), which is a function of absolute water age (*T*), current time-step (t), soil volume (at depth, z), and the time it entered the soil volume (determined via the relative age, ζ). Using the relative age ranked storage of a specific control volume ($S_\zeta(\zeta, t, z)$, units of length, mm), the absolute age ranked storage of a control volume is determined by integrating the inflow probability distribution with the relative age-ranked storage over all relative ages: $S_t(T, t, z) = \int_0^\infty w_J(T, t, z, \zeta) \cdot S_\zeta(\zeta, t, z) \cdot d\zeta$

R1C12: P5, Equation (5): Again, doesn't seem dimensionally correct. Seems like the CDF (not the PDF) of the rSAS function is needed on the RHS (Ω_z as defined in line 11)

Response to R1C12: The authors recognize that the previous terminology was confusing. The equation will be modified to show the CDF. Furthermore, the definition of the term (previously shown as ω_z) will be updated for a more explicit/accurate definition of the distribution. For a given time-step, the cumulative distribution of water in a soil volume is related to the total water in the modelling domain via:$P_{DV}(T, t, z) = ((S_t(T, t, z))/(S_T(T, t))) \cdot V_{tot}(t, z)$, where V_{tot} is the total volume of water in a control volume (a given soil layer).

R1C13: P6, L18: What is hs and in which equation is it used? It does not appear in

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equation 7. P7, L10: The text states that "under free draining conditions θ_0 approaches zero." Does this mean that θ_0 is time-varying? If so, this should be explained more clearly, since the reader is likely to assume model "parameters" (as it was called in L9) are fixed.

Response to R1C13: The text should have used h_z rather than h_s for consistency with the CV terms in the earlier equations. These terms were intended to represent the relative humidity of the soil and will be amended in revision. "*In many locations, h_z may be a significant factor by reducing the diffusive flux from the soil to the atmosphere. In wet soils, h_z is at or near 1, and the Eq. (8) is simplified using $h_z = 1$*". The variable θ_o is fixed in time and estimated for each control volume. As this was unclear that it was intended for general application it will be removed.

R1C14: P7, L16: how is V_F calculated? Also, it would be helpful to include an equation for the slow domain volume V_S. If there is a unique volume of storage associated with the slow and fast domain, then the volumes have separate age-ranked storages? Equations (1) and (2) suggest they are the same.

Response to R1C14: V_F is estimated using the equations shown in Response to R1C10, which will be clarified in the manuscript. For the conditions present in the study, V_S is constant, defined using θ_o due to wet soil conditions (i.e. there is always water in the fast domain). The downward flow ($Q(t,z)$) is estimated using the storage discharge relationship: $Q(t,z) = (((\theta(t,z) - \theta_o(z)) \cdot \Delta z \cdot \phi) \cdot a(z) \cdot (2 - b(z)))^{(1/(2-b(z)))}$.

R1C15: P8, L8: What is SM(t)?

Response to R1C15: SM is defined as soil moisture in the text following the equation (P8 Ln 9-10). For consistency and clarity, this will be changed to θ (Eqn 11 and 12).: "*$\eta(t) = \lambda \cdot (\theta(t) - min(\theta(t)))/\sigma_\theta$), λ is a slope parameter for a linear relationship to soil moisture, σ_θ is the standard deviation of soil moisture, and τ is the intercept of the linear relationship to soil moisture.*"

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R1C16: Equation 15: The paper defines ω as the PDF SAS function in line 11, P3. Shouldn't the CDF form be used here?

Response to R1C16: For simplicity in presenting the distribution, the PDF form (ω) was shown. For consistency with the use of the SAS function in Eqs. 1 and 2, the authors will change the form to CDF:

$$\Omega_Q(S_f(\zeta,t,z),t) = \frac{(B_i((\frac{S_f(\zeta,t,z))}{V_F(t)},\alpha,\beta(t)))}{B(\alpha,\beta(t))}$$

where B_i is the incomplete beta function, B is the beta function, α and β are beta distribution parameters, $\eta(t) = \lambda \cdot (\theta(t) - min(\theta(t)))/\sigma_\theta)$, λ is a slope parameter for a linear relationship to soil moisture, σ_θ is the standard deviation of soil moisture, and τ is the intercept of the linear relationship to soil moisture."

R1C17: P8, L23: The variable p is the normalized kernel density probability of what?

Response to R1C17: Apologies, this was misstated in the manuscript. The adjusted NSE used the normal distribution and standard deviation of the samples rather than the kernel density function.

R1C18: P9, L10: Please provide a reference for the kernel density estimation technique.

Response to R1C18: The kernel density estimation method was previously developed to create probability distributions for atypical shapes (Parzen, 1962). To the authors knowledge, the method of using the kernel density estimation to show daily probabilities is used for the first time in this manuscript. The use of the kernel density approach here was to slight modify the GLUE approach. The estimated kernel density function was weighted by likelihood functions. Due to the smaller number of samples meeting the minimum efficiency criteria, we used the kernel density estimation to approximate the distribution of a larger number of samples (Please see *Initial Response to Reviewers 1 and 2* for an example). The use of kernel density estimation additionally produces

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distribution “tails” and theoretically results in more conservative (larger) uncertainty bounds that would not be present when only using the selected parameter sets (i.e. using and empirical CDF).

R1C19: P9, L3-4: One additional sentence on how the "best" calibration was selected would be helpful, with the understanding the reader can refer to the citation for more details.

Response to R1C19: The parameter sets were based on minimum efficiency criteria of 0.4, rather than arbitrarily selected. Since one location (Site B) had fewer than 100 parameter sets meeting the minimum criteria, the next closest parameter sets to meeting the efficiency criteria were included (minimum efficiency near 0.4). The authors will include a more details explanation of how the parameters were selected, and subsequently ranked using the cumulative distribution function (CDF):

$$n(X) = (\cap_{i=1}^{5} \cap_{j=1}^{3} P_{(i,j)}(X \leq x))/100$$

where $P_{i,j}$ is a CDF for a model layer i with efficiency criteria j, and $n(X)$ is the number of simulations meeting the objective ($X \leq x$).

R1C20: P9, L7: The phrase ..."by estimating xylem through root-uptake..." is confusing. What does it mean to estimate xylem?

Response to R1C20: This was unclear and will be amended to: “*...the simulated root-uptake isotopic composition (Eq. 19) with the parameters for the source of R with depth (k_R and u_R, Eqs. 13, 14) were evaluated against measured xylem isotopic composition using the efficiency criteria (NSE_{adj})*”

R1C21: P9, L1-4: The manuscript should describe why this calibration approach it thought to produce meaningful "confidence bounds" (as shown in Figure 3), and explain what the bounds mean. As I understand it, the range of the confidence bounds will approach zero as the number of Monte Carlo simulations goes to infinity (i.e. there will be >100 essentially identical "best" calibrations at a single optimal point in the

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parameter space), which makes the confidence bounds seem arbitrary and difficult to interpret. In other calibration techniques such as GLUE, the confidence bounds approach finite values as the number of MC simulations gets large, which makes the outcome more easy to interpret. I reviewed the Aho-Aho et al 2017 reference, and did not see this issue addressed.

Response to R1C21: As mentioned in Response to R1C19, the 100 parameter sets were chosen based on a set minimum efficiency criteria rather than arbitrarily selected. This will be clarified in the manuscript. Similarly (Response to R1C18), the kernel density approach is used to estimate the confidence bounds in a slightly modified method of the GLUE approach.

R1C22: Section 3.1 and Figure 3: The similarity in the isotopic concentrations observed at site A and B across time and depth (shown in Figure 3) is striking. The values and trends in isotopic concentration seem to be the same at both sites, even if individual values vary a bit. This similarity is unexpected given that Site B is described as more freely draining and has a different soil moisture profile (Figure 4, top panels). It seems important for the manuscript to comment on the similarity and whether there are any significant differences in the data collected from the two sites, since the models are calibrated to this data. Related to this, it also seems important to comment on why the difference in the drainability of the soil at site A and B does not seem to affect the measured isotopic concentrations.

Response to R1C22: This is a good suggestion which may be addressed in more detail on Figs. 3 and 4. The statistical differences of isotopic compositions may be provided on the figure to show how Site A and B differ with depth. The authors remind the reviewer that the isotopic compositions are bulk soil samples, which include young and old water. While a site may be freely draining, it still retains water which may not mix thoroughly with younger water. This is what the simulations of the sites show. The young water, leaves the soil very rapidly (Fig 4 outflows), which results in very small replenishment of the bulk soil water with young water. The scale on Fig. 4 outflows will be adjusted to better show the differences of how young water moves through each site.

R1C23: Figure 2: What are the two dotted lines that split the Slow and Fast Domain in figure (a) and (c)? Also, consider adding political boundaries and labels to the map of the UK, to orientate the reader

Response to R1C23: The dotted lines represent the uncertainty of θ_o using the iterative solver. These uncertainties are small and do not provide a significant influence on any results. The authors will add a to the figure caption to explain the dotted lines.

R1C24: Figure 4: The upper soil moisture plots should be included in the figure description.

Response to R1C24: The authors will include the moisture plots in the caption.

R1C25: P14, L8-9: Here it states the "CVs for each site" are shown in Figure 4. This is the first reference I see to the actual number of CVs modeled. Is the bottom of each CV the number shown in the Y axes of Figure 4? This ambiguity speaks to a wider problem, which is that the number of final model parameters and the calibrated parameter values is not reported. Given the stochastic nature of the calibration, the calibrated parameter values used in the manuscript are needed to reproduce the results.

Response to R1C25: The authors apologize for this confusion as it seems we were not clear in our original description. The soils were discretized into 4 control volumes. Since sampling encompassed the soils within a specific control volume (i.e sampling at 5 cm included samples from 0 to 5cm), the CVs were discretized to include the soils sampled for a given depth. We will include: "*Since the soil samples were an aggregate of water between the soil depths (i.e. soil at 5cm includes soil samples from 0 – 5 cm), the modelled soil layers were discretized into 5cm intervals. From the surface, the layers are named 5 cm (0 – 5 cm), 10 cm (5 – 10 cm), 15 cm (10 – 15cm), and 20 cm (15 – 20 cm).*" The calibrated parameters were previously not included as the

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discussion of parameters does not always provide a clear indication of the mechanisms within the system. For this reason, the shapes of the SAS functions were provided as they provide a more meaningful comparison of high soil moisture and low soil moisture conditions at each site. The authors can provide the distribution of best parameter sets for each site in the revision.

R1C26: P17, L5-7: The sentence "The selection of deeper soil water at Site A relative to Site B resulted in slightly resulted in..." raises some of the same concerns described above. First, is the difference between the sites considered significant because the confidence intervals don't overlap? If the calibration used 10,000 monte carlo simulations instead of 50,000, would the confidence intervals be different in a way that could affect the significance test? Also, if the difference really is significant, is the effect of this difference apparent in the measured isotopic data? To convince the reader that these small differences in performance between Site A and B are greater than model uncertainty, it is helpful to show how they arise from the calibration data.

Response to R1C26: As discussed in Response to R1C19, the best parameter sets were selected based on a minimum efficiency criteria rather than an arbitrarily set number of simulations. This will be clarified in the revised manuscript. As suggested by Reviewer 1 (R2C22), the authors will include some statistical differences between the measured isotopic compositions of the sites in the appendix. The authors will also include statistical differences between the simulated ages of each site in the appendix.

R1C27: P18, L7-10: "*It is notable though, that of the five xylem sample days, one (June 2016, Fig 3i, 3e) showed isotopic compositions different from either the simulated fast or slow domain isotopic concentrations.*" I was confused by this, because I do not see a uniquely bad fit between the simulation confidence interval and the observations on June 2016 in Figures 3i and 3e. I see that the simulation is not very good that day, but it is also not very good in 10/15 site B. Also, I don't see any differentiation in this plot between the fast and slow domain. Consider clarifying.

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Response to R1C27: The large deviation occurs with $\delta^2 H$ (red squares) rather than lc-excess. The difference is much more noticeable for Site B than Site A. In Site A, most of the measured samples are more depleted than the simulation, with the exception of one sample. For the reason that there were no samples for the fast and slow domain independently, the isotopic compositions were only shown as the bulk water (all water in a CV). The authors will modify the statement to indicate that focus should be on $\delta^2 H$ and that the fast and slow domains are not shown on the figure.

R1C28: P18, L24-26: The meaning of PT (> 0.5) is not clear.

Response to R1C28: The definition of P_T was provided in the methods section (P8 Ln 4), and is effectively the CDF of ST. Therefore, $P_T(0.5)$ is the median age, and $P_T(> 0.5)$ is water older than the median age. The authors recognize that this definition may not have been clear and will modify the description.

R1C29: P18, L28: The phrase "one of the difficulties of identifying SAS functions at catchment scales is the shape of the SAS function..." seems to be circular logic. Also in general it was relatively difficult to follow the logic of this paragraph. Consider reviewing and clarifying.

Response to R1C29: The authors thank the reviewer for the suggestion and agree that the statement was previously confusing. The statement will be modified to: "*One of the primary difficulties of identifying the temporal variability of flow paths at catchment scales is the shape of the SAS function*" The authors will further revise the discussion section.

R1C30: P19, L32: Why is it that the median water ages were similar to previous estimates despite the similarities of the derived SAS function? I would have thought the median water ages would be similar because of similarities in the derived SAS function. A bit confusing.

Response to R1C30: This statement will be revised to: "*The use of the temporally*

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variable selection for E yielded little time variance at either site, and resulted in estimates of evaporation water age similar to previous catchment-scale flux tracking on hillslope AET..."

R1C31: P19, L2: The "Figs. 2b, 2c" do not show simulated isotopic enrichment. Is this the right figure reference? Also, in general, the sentence starting with "Notably," is confusing. Consider clarifying.

Response to R1C31: This was misstated and should refer to Figs. 3d, 3h and the simulations at 20cm for $\delta^2 H$ at both sites. The statement will be amended to: "Although the evaporative fractionation primarily occurred within the upper 5cm, some isotopic enrichment of deeper soil water was observed in annual cycles of negative lc-excess (Figs. 3d, 3h)."

R1C32: P19, L23: Not clear what is meant by the phrase "with the selection of young water" in the context of the sentence. Consider clarifying.

Response to R1C32: The statement will be modified to: "*The average age of the soil water was older than expected for shallow soils (upper 5cm) due to the preferential selection of young water for downward flux. However, the median water age through all soils depths was broadly consistent to ...*"

R1C33: P20, L11-12: How was "a general reduction in the uncertainty of the SAS function" observed in Figures 4 and 3? As best I could tell, the certainty of the SAS function was not explicitly shown in the figures.

Response to R1C33: This statement will be modified to: "*A general reduction in the uncertainty of the water age estimation (narrower bands, Fig. 4) and $\delta^2 H$ (narrower bands, Fig. 3) during wet conditions, while during dry conditions the uncertainty is the highest. This may indicate a convergence of the shape of the SAS function during wet conditions while drier conditions are not as sensitive.*"

R1C34: P20, L30: "relatively simple framework"... relative to what? The approach

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seems fairly complex, even without accounting for lateral fluxes.

Response to R1C34: This was intended to state that a non-physically based model provides additional insights into soil water mixing, with shorter calibration run times in a probabilistic framework. We will modify the statement in the revision.

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