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Discussions

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.

Anonymous Referee #3

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1 General comments

This paper examined the water ages of evapotranspiration flux, soil water, and recharge and those time-variability. The potential contributions of this paper are: 1) development of the "feed-forward" model using the SAS function approach, 2) presenting the fascinating isotope dataset, and 3) explaining the differences of age in the fluxes and in the soil and examining those time-variabilities based on the developed model calibrated against the dataset. However, the current manuscript needs significant improvements. First, the benefits of using the SAS function approach in the study are not clearly stated. Second, the model development was not described clearly with the several miss-typed equations and potential errors. The poor description eventually made it difficult to read the discussion part as the discussion part is mainly written based on the calibrated model. Third, the model evaluation was also not described well and poorly performed. Fourth, several logics in the model (result) interpretation should be better clarified. The four points are described in more detail in the following section.

2 Specific comments

2.1 The use of SAS function approach

The advantages of using the SAS function approach in this study are not clear (and not stated explicitly). If I understand correctly, the fundamental advantage of the SAS function approach is its capacity of simulating the time-variable transport in a "parsimonious way" (as in Line 16 on Page 2). While the authors may argue that their model is parsimonious (in Line 21 on Page 1), the proposed model has quite a large number of parameters (six). Moreover, there are several assumptions in the form of the SAS functions that are not supported by data. For example, why do the SAS functions for Q (or downward flux) have the same form at each layer? Why are the SAS functions for D uniform? Why are the SAS functions for ET and R at each layer uniform? To this end, I am concerned if the SAS function approach was used because of its arbitrariness on choosing functional forms for the SAS functions (which could be a disadvantage of the approach but allows unpleasant flexibility to a modeler), not its parsimoniousness.

2.2 Development of the model

The model (Equations 1–12) is not described well and potentially wrong. My concerns are listed below.

2.2.1 Equations 1–2

1. The symbol S_z was used for both fast and slow flow domains. I assumed that the S_z in Equation 1 is the age-ranked storage for the fast domain and that the S_z in Equation 2 is the storage for the slow domain, as it is stated in Lines 16-17 on page 4.

2. No influxes: There are no influx terms in Equations 1 and 2. All storages will be continuously depleted. I believe it is a typo. However, it is important to know if the influxes go either to the fast flow zone or to the slow domain, or if the influxes somehow partitioned into both domains. If the latter is the case, how the partition occurs also needs to be described.

3. Doubled root water uptake: Root water uptake occurs in both domains with the rate of R_z. Thus, the total root water uptake from the layer is: $2R_z$.

4. Fraction of root water uptake: What fraction of R was drawn from the slow domain, and what fraction was drawn from the fast domain? How were the fractions determined (or assumed)?

5. Slow domain influxes: As previously stated, the slow flow domain is a source of root water uptake in the model. If there is no influx to the slow domain, the slow domain will be continuously depleted as the net flux through the matrix diffusion D is zero.

6. Evaporation only from the mobile zone: This is also an assumption in the model that needs to be clarified and justified.

7. Not clear age-exchange between the fast and the slow domain: If I understand correctly, there is no net-flux between the fast and slow domains, and there only is age-exchange controlled by the SAS function ω_D, which was assumed as the uniform function in the model. However, the physical meaning of ω_D is very obscure, and how the model works would be different from the cited papers in Line 16 on page 3.

8. $Q \rightarrow Q_z$

2.2.2 Equation 3

It would be surprising if the age-ranked storage can be estimated only using the influx. Don't you need to consider outfluxes and the aging of water inside the storage?

2.2.3 Equation 4

Integration is inappropriate. That should be the summation as you did in Line 10 on Page 4.

2.2.4 Equation 5

I don't think the ω_z here is a SAS function but is a cumulative residence time distribution mapped on the age-ranked storage.

2.2.5 Equation 6

1. δ_z should be defined better as it is a function of S_z on the right-hand side term and is a function of z on the left-hand side term.

2. And, again, ω_z is not a SAS function.

2.2.6 Equation 7

The z and t dependencies of the terms are not described well.

2.2.7 Equations 8–9

1. It is not described well how the δ_E, estimated from Equation 8, can substitute the one in Equation 7. In Equation 7, δ_E is a function of T, while δ_E in Equation 8 is not.

2. Potential internal inconsistency: I think that this δ_E is different from δ_E estimated using the SAS function. How can you resolve the inconsistency in the model?

3. What are the values of the parameters used? Also, were the same values used for all the layers?

2.2.8 Equations 11–12

1. Equation 12 is a water mass balance model using the relationship described in Equation 11. Thus, the introducing sentence which reads "Eq. 11 is rearranged to solve for the ..." is not correct.

2. Potential inconsistency in slow and fast flow domain storage: There are two different slow domain storages in the model: $\theta_0 \Delta z$ (as stated in Lines 9-10 on Page 7) and $S_T(t,t)$ (used in Equation 2). If those are different, this inconsistency should be introduced and treated carefully in the manuscript. Such inconsistency also exists for the fast flow domain.

3. In addition to the above point, I think there are three domains in the model among four available combinations: fast$_\theta$- fast$_{S_T}$, fast$_\theta$- slow$_{S_T}$, slow$_\theta$- fast$_{S_T}$, and slow$_\theta$-slow$_{S_T}$, where fast$_\theta$ is the fast flow domain determined by θ, fast$_{S_T}$ is the fast flow domain determined by S_T, and so on. These four available domains are not described at all in the manuscript, and the manuscript misleads readers by stating that there are only two types of domains.

2.2.9 Equations 13-14

The authors stated that the parameter u can be time-variant. However, how u was formulated is not described. There is no λ in Equation 13-14, while the authors stated that "λ in Equations 13&14 was permitted to equal 0, time-invariant conditions" in Lines 5-6, Page C3 in AC2: Response to Reviewer 2. λ was introduced later under Equation 16 but not used in Equation 13-14. Perhaps, an equation similar to Equation 16 is required to formulate u here. Also, it would be arbitrary that how the functional form for u was selected. Can it be justified?

2.3 Model Evaluation

2.3.1 Model performance measure: Equation 19

1. Was the adjusted NSE newly developed in this study, or are there any references to cite?

2. It is unclear what samples were used in the density estimation. Thus, I had to guess that the replicated samples (n=4) were used to construct the density. It is also not clear what bandwidth was used for the kernel density estimation.

3. Moreover, wouldn't the (perhaps chosen arbitrarily) bandwidth plays an important role in considering the measurement uncertainty in the adjusted NSE? I wonder what the benefit of using the kernel density would be compared to the likelihood functions which considers the measurement uncertainty in a statistically more rigorous way (by using statistics of the measurement such as standard deviation). I don't think the use of the kernel density would be a better way of accounting the uncertainty than using such likelihood functions.

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2.3.2 Parameter estimation

It is not described well how the 100 parameter sets were chosen using the multiple NSEs (for different types of measurements and different measures). The only description is: "The "best" calibrations were selected using the NSE_{adj} for all measurements (..) and a cumulative distribution function (Ala-Aho et al., 2017)" in Lines 2-4 on Page 9. It is unclear what the "cumulative distribution function" is in this context until one looks at the cited paper. More detailed description is required so that potential readers can grasp what the authors did without looking at the cited paper. (By the way, the description (Equations 6-8) in the cited paper is written with typos, so it was hard to understand the method. Thus, I think the equations should be re-written in this paper). Moreover, selecting the 100 best parameter sets (not 200, 1000, ..) is quite arbitrary, and the arbitrariness makes it difficult to interpret the model's uncertainty estimation.

2.4 Model interpretation

This part, mainly the discussion, was very hard to read as I don't have a clear picture of the developed model. Thus, I wrote only a few comments on this part at this stage.

2.4.1 Time-variability

The authors stated that "this model structure does not make the assumption that uptake is time-variant or time-invariant" (for example, in Lines 6-7 on Page C3 in AC2: Response to Reviewer 2), and they argued that the model and data supported the time-variant hypothesis as perhaps the NSEs were higher when the time-variability was allowed (when the parameters u_F is time-variant).

I don't agree with the statements for two reasons. First, the criteria for choosing the time-invariant model is unclear. Second, and more importantly, I don't think the authors have enough data to discuss the time-variability, and the detected time-variability could be an artifact. I will discuss these in more detail in the following.

First, when can you say the beta distribution in Equations 14–15 was time-invariant? When the calibrated parameter (let's say λ) is "exactly" zero? Or, when the absolute value of λ is less than a certain threshold?

Moreover, it seems to me that the isotope dataset (presented in Figure 3) is perhaps not sufficient to test the time-variability. With the above (threshold) issue in mind, perhaps the best way to test the hypothesis on the time-variability is to see how the model works for two different cases: one with the λ parameter set to 0 and another by allowing calibration of the parameter. If the authors can identify several periods when the model with $\lambda \neq 0$ captures the observed time-variability, the authors perhaps can say that the time-variable model was required. However, I don't think the dataset is enough to be used for this, and perhaps the model still would do a relatively good job with the λ parameter set to 0. Thus, I suggest the authors show the model results with the λ parameter set to zero. As the use of NSE would not be sufficient to discuss it, the time-series of model results (similar to Figure 3) should be included (at least in Supplemental material) so that readers can agree to the argument on the time-variability.

2.4.2 System-scale SAS function for ET and R

It should be described better with an equation of how the SAS functions in Figure 6 were estimated.

2.4.3 Comparison of the estimated range of water age

Page 20, Lines 24-25: It seems to me that the ranges overlapped each other quite a lot; thus, it is hard to agree with the statement.

3 Technical correction

Page 3, Line 15: The term "diffusion" is too broad here. Please specify.

Page 9, Equation 21: The equation is not about model evaluation. Please consider relocating or removing the equation.

Page 17, Line 14: 'More detailed' is not correct.

Page 17, Line 15: Isn't it median water age in storage not average?

Page 18, Line 23: Possible type: "depth of the simulation"

Page 19, Line 24: "Older than expected": What was the expectation and why?

Page 19, Line 25: Possible typo: "Despite the dispite"

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