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Hydrology and Earth System Sciences
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.

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The authors thank the reviewers for their comments. As requested by the Editor, we here make some initial responses to the issues raised by. From the review comments that have been received, there seem to revolve around three main concerns: 1) the clarity of the theory section resulting from inconsistent terminology and definitions, 2) the description and execution of the calibration process including the parameterization, and the 3) methods of model evaluation and consideration of uncertainty. Technical Clarity With hindsight, we can see that some additional clarity in the theory section would be helpful. In many of the cases highlighted by the reviewers the (understand-

able) primary confusion is the slight inconsistencies of terminology and definitions through the section. To address this, in the revision, the authors will add a glossary table in an appendix with definitions and units of each term. The inclusion of a glossary will help clarify this issues while simultaneously providing a means to assess the dimensional balance to each equation. Model Calibration The calibration section (Section 2.5 and 2.6) will be revised and updated to provide more detail of the application of the depth-dependent SAS functions to each study location (Site A and Site B). This includes the total number of control volumes (4 depths of 5 cm each, with fast and slow domain) and their parameterisation. This has further led to some confusion about the number of parameters calibrated in the model. For clarity in the revised manuscript, the authors will emphasise that there are 3 parameters for the soil water SAS function (downward flow), and 3 parameters for each the evaporation and root uptake. This effectively leads to a total of 6 SAS function parameters (soil and evaporation SAS functions) to estimate the soil layers. The SAS function parameters are identical for each control volume. δ2H, δ18O, and lc-excess is simulated and evaluated in each layer. For a simulation to be considered "behavioural", the parameterization of the soil water SAS function must provide reasonable fits (according to the efficiency criteria) for all control volumes. Hence, the sensitivity of one layer on the next, as suggested by Reviewer 1 is resolved because the sensitivity of each layer to the SAS function parameters is implicit. The authors agree that the inclusion of a sensitivity analysis would help clarify this issue as well as providing additional insights for readers looking to apply this model structure in different regions.

Model Evaluation One of the calibration concerns raised was the number of Monte Carlo parameter sets (50,000) and the number of retained parameter sets (100). The authors agree that the selection criteria was not clearly stated. The following explanation will be included in the revised manuscript to best explain the calibration process. The use of 100 best parameter sets was used due to the initial restrictive minimum criteria threshold (NSE $\geq$ 0.4) for δ2H, δ18O, and lc-excess for all control volumes (5, 10, 15, and 20 cm) in addition to the simulated xylem water δ2H, δ18O, and lc-excess. The

total number of simulations meeting this criteria at each site was <100. For each site, the number of retained parameter sets was increased to 100 by ranking the minimum efficiency for all control volume simulations (EFF): EFF=minâĄą([E_1 (δˆ2 H),E_2 (δˆ2 H),. . .E_n (δˆ2 H),E_1 (δˆ18 O),E_2 (δˆ18 O),. . .E_n (δˆ2 H), E_1 (lc-excess),E_2 (lc-excess),. . .E_n (lc-excess)]) where the subscripts 1 to n are number of control volumes (plus one for xylem water). This ranked efficiency ensures that the remaining retained parameter sets were near the minimum criteria threshold. With the minimum threshold criteria, the uncertainty bounds will not decrease with more Monte Carlo simulation set as suggested by Reviewer 1. Regarding the use of GLUE to evaluate the model performance uncertainty, the author considered this originally but felt that a full GLUE-type analysis of the parameter sets would not provide significantly more information. However, to justify this, we see that further description in the manuscript of the Kernel Probability Density Estimation (KDE) for uncertainty will be necessary to help the reader interpret the model uncertainty. The uncertainty bounds (Figures 3-5) were estimated on daily time-steps by fitting a KDE to the daily simulated isotopic compositions, weighted by the likelihood functions. Figures 4 and 5 show the PDF of the KDE, while Figure 3 shows the resultant 95% bounds of the KDE CDF. Unlike the GLUE approach which uses an empirical CDF (eCDF), the KDE may actually be more beneficial with fewer parameter sets since it interpolates and extrapolates the occurrence of likelihood values (beyond observed likelihood values). In circumstances of models with a smooth likelihood population (e.g. normal, gamma, or exponential distributions), the KDE may converge to the shape of the likelihood population faster than the eCDF (Figure 1 in this comment).

For the figure (Figure 1 in this comment), the mean absolute error is calculated as the difference between the estimated CDF (either the KDE or eCDF) and the known 'population' distribution. Reviewer 2 raised model uncertainty in relation to structural and measurement uncertainty as a weakness. The authors agree that some additional comment on the structural and measurement uncertainty of the model should be added to the manuscript. Firstly, the uncertainty of the isotopic measurements is

implicitly included in the calibration (Equation 19), which uses the statistics of the replicate measurements (mean and standard deviation) of soil and xylem water to inform the goodness-of-fit of the model. Using the standard Nash-Sutcliffe criteria requires the only the mean of the measurements and would likely result in reduced uncertainty bounds. This would occur due to higher sensitivity of the model goodness-of-fit to the mean measurement values. Suggestions to test the model structure is secondary to the goals of this study, and while worth examination in additional studies, cross beyond the scope of this study. Regarding the use of the Craig-Gordon model (isotopic model); this has been widely used in many isotopic modelling studies and has been adapted for isotopic fractionation in soil water. The use of a beta distribution for evaporation and root-uptake (Equations 13&14) is necessary due to the confined soil depth simulated. The use of a gamma distribution for either flux results in selection of deeper soil water (and isotopic compositions) which were not simulated. Hopefully this initial response shows how we can revise the manuscript to address the issues raised by the reviwers.

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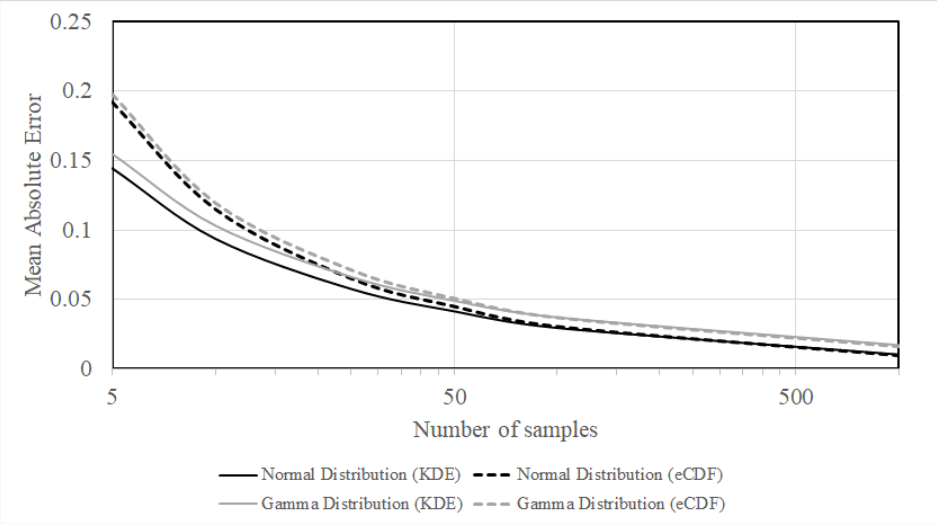

Fig. 1. Comparison of the mean absolute error of the CDF of kernel density estimation and emperical CDF estimation against a known CDF distribution